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 its 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 balls 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. At 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.
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 pursuing 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.

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 this 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](image-url). 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.
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 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 than 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 accomplishments 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 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.

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-
replaced 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 its 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...
KUNZ—ON THE WINGS OF HEROES

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 biology 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—an 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 unsuspectingingly 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.

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 gynelayer 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 Niagra and Current Rivers, two white-water rivers (both part of the National Wild 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, seized 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, tape 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 Lasiomycteris 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, *Nycticeius 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 brasiliensis* 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 uncataloged 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 *Nycticeius 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 *Nycticeius humeralis*. This discovery was exciting because some of my specimens were of juve-

tiles—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 *Nycticeius 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 Nicaragua.

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 verbatim—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 Mar-garet 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 anecdotal, 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 handwritten 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 Nycticidae, 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 Nycticeius 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 griseascens, 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 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 requirements 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 *Lasius borealis* and the obligate cave-roosting species *Myotis griseescens*. 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 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 townsendii, 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.

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.

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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 extralinaire, 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 distin-
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guished 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 paleontologist), 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) Echtenacht 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 “special 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 of 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 publications can be found on my website (www.bu.edu/ceeb).

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.
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.

**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.
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.

<table>
<thead>
<tr>
<th>Passion for organismal biology</th>
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<tr>
<td>Passion for reading</td>
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<tr>
<td>Passion for writing</td>
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<tr>
<td>Field and laboratory skills</td>
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<tr>
<td>Common sense</td>
</tr>
<tr>
<td>Patience</td>
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<td>Perseverance</td>
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<tr>
<td>Enthusiasm</td>
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<tr>
<td>Commitment to do your best</td>
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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.

Literature Cited


