Oral History Cover Sheet

Name- Mike Bogan
Date- October 9, 2006
Location of Interview- Corrales, New Mexico
Interviewer- John Cornely

Approximate years worked at Fish and Wildlife Services- 20+

Offices and Field Stations worked- National Smithsonian- Washington Dc,

Most Important Projects- Bat Biology of the Southwest, Florida Mammal Study

Colleges and Mentors- John Nichol, Jerry Tomanek, Gene Fleharty, Jerry Choate, Charles Ely, Tony Mollhagen, Jim Finley, Don Wilson, Dan Wilson, Jay Druecker, John Darling, David Ligon, Mike Merris, Gary Hulett, Hal Black, Hugh Genoways, Clyde Jones, Karl Koopman, Al Gardner, George Lowery, Bob Fisher,

Most Important Issues- Bat Population of Myotis Californicus (Mouse-Eared Bat)

John Cornely:
This is John Cornely, it's October 9th, 2006 and I am with Mike Bogan, he is retired from Fish and Wildlife Service and USGS, and we are in his home in Corrales, New Mexico, and we are going to do an oral history today, and I am going to turn the microphone over to Mike and let him just start and talk to us about himself.

Mike Bogan:
Hi John, it's a pleasure to be here. I have, as I think I told at my retirement party back in June in Fort Collins, I have great affection for the Fish and Wildlife Service. We can go into the career aspect of this later, but I am certainly happy and a little honored to be able to participate in all of this.

I a mid-westerner, but specifically a Kansan, I was born in Kansas City, Kansas; my father was in medical school at KU Med Center at the time. When I was about 2 years old, I guess, my father had finished his medical school and a year of internship at KU Med Center, and we all moved down to the extreme southeastern corner of Kansas, Baxter Springs, and that's really where I grew up. We had, I think, three different houses, each one a little more imposing than the one before. My dad was humble in many respects, but he also felt like a doctor in a small town, had a certain image that had to be maintained. My sister was born in Kansas City, but in Missouri, and why that is I don't know. Both my folks were natives of Kansas, by dad was born in Derby, Kansas near Wichita, and my mother actually was born in her folks home in LaHarpe, Kansas near Iola, Allen County. My sister and I grew up not very far from where my folks had grown up as well. Both my folks were college graduates; they were the first college graduates in their respective family. My mother was a musician actually and got her degree, she had a bachelor of fine arts from the University of Kansas, and subsequently she also got a teacher's certificate. And for a few years before they were married and while my dad was going through the first year or two of medical school, she actually taught school at some little town near Topeka, and I've forgotten which town that was. My mother's upbringing I think was pretty straightforward; her folks had come to Kansas, her mother I know in a covered wagon in the late 1800's, my grandfather actually rode over, having left work at a ranch in Missouri, as I was told. We ended up spending a lot of time with my mother's folks there in LaHarpe. Both my sister and I really liked those people a lot, and I think my mother's upbringing was pretty straightforward; they were people of modest means. My grandfather kept cows and his wife, Gertrude, kept chickens, and they had a beautiful garden every year. My grandmother was quite religious, she was a Methodist. My father moved around a whole lot more, and I think his family probably prior to the depression had accumulated a considerable amount of wealth, and all of that was lost in the depression, and I think that was something that my paternal grandmother never got over. She had a tendency to put on airs, so to speak, and was a little judgmental, I think, about people and so forth, she was a little reserved. And, as you might imagine, she was not my favorite grandmother, and I don't think she was my sister's favorite either, but we spent a lot of time with her too and in fact she had told me about the depression and about

the impact on my dad. My father ended up completing high school but then took some time off and actually sold magazines across the country, and I guess that was common at that time, that's what he said that's how a lot of young men were making money and it was also obviously a good way to see the country. He had decided apparently early on that he was interested in going to medical school but it took him awhile to get there, and without any money, it was necessary for him to work. He was a short order cook in Lawrence, Kansas while he was going to school, but he eventually got his medical degree. We moved to Baxter Springs, and Baxter really was where I grew up.

I think I was fairly typical of young boys. Baxter was a great place to grow up, we had a river nearby, the Spring River, and we had limestone caves we could go explore. Even then I think I hated the heat and humidity in the summertime, and only D.C. is worse I've decided. I was a boy scout as a young man, I really enjoyed the scouts, I spent a lot of time camping. I remember those early camps with Army surplus shelter halves and kapok-filled sleeping bags that did everything except keep you warm, you know you just froze in them, they were heavy if they got wet, why you were in the world of hurts! But I really enjoyed being out, and I think it was probably those early camping experiences in the scouts that probably had a big impact in terms of opening my eyes that there was a world outside that appealed to me, that I wanted to be a part of. My paternal grandfather, as I mentioned to you earlier, was a sportsman, and I had gone hunting with him, or if I didn't go I was there when he came back and festooned his '46 Chevy with greenheads. He is probably one good reason why we have limits on waterfowl these days! But I think he was probably pretty typical of the times. But he enjoyed being outdoors, and I spent some time with him and always enjoyed those times.

I was sick with an unusual skeletal-muscle-skin disease in the late '40s and early '50s, and I don't really need to go into that unless there is some aspect of it that you want. I really, like any child that age, I was like 8 through 10, I really hated being inside, I was bedridden, but my father literally and absolutely saved my life, I think if he hadn't been an M.D., I would have probably died in 1949 or '50. He ended up moving the whole family as far as Boston, Massachusetts to make sure that I got adequate medical care, and he kept a close eye on me once we returned to Baxter. I was out most of the 3rd and 4th grades, and then the 5th grade I actually went to school at home via a device I had never seen before called an intercom. They actually strung an intercom line between my grade school and my house, and that's how I went to school, I had a school desk and the whole bit. The germane part of all of this I think is that I was bedridden, I couldn't walk, I didn't really understand it. On the other hand, I think children maybe tend to accept a lot of that, I don't recall laying there thinking why did this happen to me, and all of my friends are out running around and I can't do that. What I ended up doing, spending a lot of time doing was reading, and what I particularly enjoyed reading were books on animals. I think there was a magazine at the time that was called Junior Natural History, and I think it probably came out of the American Museum in New York, and they had stories about explorers, they had stories about archeologists, and biologists, the whole bit. The ones that really intrigued me I think were probably the archeologists and the people that were going into the pyramids and that kind of thing. But the underlying theme I think again was these people were going to strange, exotic places and were having great times. They

were either finding things new to science or they were verifying those scientific discoveries, and it just really intrigued me, and it was something that sounded like that's what I wanted to do when I grew up.

Now here is where my father and I kind of parted company a little bit because I think my dad was like a lot of M.D.'s, he really expected his son in particular to become a medical doctor to. I spent the early years of my life in Baxter Springs, 5000 people being referred to for years as "little doc." I don't think my father really understood that I wasn't going to be medical doctor until after I had gotten my master's degree in zoology and had gone to New Mexico to work on a PhD, and I think at that point why it finally sunk in because up until that time why he would approach me and he would even say, "Well now when you get your master's degree you will be ready to go onto medical school." But, bless his heart, it didn't happen. I ended up graduating from high school, barely snuck into the top quarter of the class as I recall, graduating class of 1960. Apropos to what we're doing here, I have really enjoyed in the last 15 or 20 years going back to high school reunions, it's really incredible to see those people and touch bases with them all, find out what they've all done over the years. We have very good longevity in our class, I'm happy to say so, I hope those reunions continue.

But one thing I couldn't talk my dad out was that I had to go to KU, which was where both he and my mother had graduated from, and so I went to Lawrence. My first year of college was an unmitigated disaster by and large. I think in retrospect I probably didn't have any really good study skills, my high school was probably not so challenging that I couldn't go through it in my sleep in some respects, I wasn't challenged very often, I would do things at the last moment and still get good grades. So I don't think I had any study schools, I also don't think I was prepared for a school as large as KU was then. I don't remember what the enrollment was but it was probably 12,000 or 15,000 even then; I graduated from high school in 1959. So that was a disaster, and I went to summer school the summer of 1960 and I did okay in summer school with just a light class load, I went back to KU in the fall of '60 and again had the same problem. At that point I was trying desperately to talk my dad into just letting me go back to painting houses, I had spent some time painting, and he simply wouldn't have any part of that. He either knew or found out that there was a small Methodist school south of Lawrence by the name of Baker University in Baldwin City, and so my father insisted, he couldn't control my admission down there obviously and he knew that, but he insisted that I go down there and talk to them and seeing about getting into Baker. And so I went down there and was interviewed by the Dean of the college, a guy named Benjamin Gessner I think was his name, and to someone like me who's academic career was either a nonstarter or in complete shambles, I don't know which, I was just intimidated to be held by Gessner. But he saw something there I guess and decided it was worth taking a chance, maybe they just needed some extra tuition money, I don't know, but I was admitted provisionally at Baker. I started there in the spring of 1961, and Baker and I got a long great. I liked the small class sizes, I liked the intense, personal interest on the part of the professors, and I really got what I needed. If I had a problem I could talk to them, I could find them, and they would take the time to help me with it. I ended up majoring in biology and minoring in chemistry of all things, and I took all the chemistry I could at Baker up to

P-chem, I took a bunch of psychology, which I liked, I took some art and drawing classes, and so for me Baker was eclectic and just really, really good. The best thing about Baker was a young professor by the name of John Nichol, who subsequently got a D.Ed. and went to the University of Wichita. At the time, Nichol had just gotten his master's degree at Fort Hays, what is now Fort Hays State University, in western Kansas, and Nichol taught histology, anatomy, physiology, and lots of these classes. Baker was mostly a training ground for premed students to tell the truth, I think they taught one bird watching classes but they didn't teach anthology per say.

I really liked John Nichol, I liked the way he taught, I liked the way he dealt with students, and the way he dealt with people in general, we hit it off. And as I approached graduating time in the spring of '64 at Baker, my dad was leaning on me big time to go to medical school, big, big time, and I took the MCAT (Medical College Admission Test) and all of this stuff. I applied to a bunch of schools, got an interview at KU as a matter of fact, but my heart was not in that, and so I asked John Nichol was his ideas might be, and John said, "Well, you know you really aught to drive out to Hays and talk to some people there at Fort Hays and see what you think about that place." He wasn't pushing me strongly but he said, "There's a couple of people there that you really aught to go talk to." One of those was the then chair of biosciences, and that was Jerry Tomanek, and the other guy he suggested I talk to was a young man who had just gotten his PhD at the University of New Mexico, Gene Fleharty. He said, "Go talk to these guys and see what you think about them and what they think about you." And so I drove out, in fact I think I went on my own to Hays, I had called ahead of time and they were both perfectly happy to see me, this was in the summertime and I was probably lucky to catch them both. I went and talked to Tomanek first at his suggestion; and Jerry is a gentleman, a scholar, he later became President of Fort Hays, an excellent teacher, someone who really values the graduate student experience. Jerry told me a lot about Hays, told me about its strong background in botany and zoology, and the particularly strong background that the school had always had in field studies. And so after he kind of got be prepped then he sent me down to Fleharty, because I had told I think the both of them of the various (unclear) groups at least I was most interested in mammals, and so I went down and then I spent about an hour with Fleharty. To make a short story long I guess, why I ended up leaving Hays that day and driving back to Lawrence, but with a pretty strong feeling that, I mean I took an application blank with me, with the strong feeling that I was going to apply to Hays. They encouraged me to do that, they said if I would do that why they probably could even offer me a teaching assistantship. Relative to the way these things are done now, this was a very informal, and for me, almost a last minute process, I mean I think I probably made that drive out there in June, I'm guessing, and school was going to start at the end of August or early September.

But I went to Hays, and I have been to several Hays reunions lately. I don't think there is a one of us that doesn't value our time and our experience at Hays, and a lot of people from Hays who got master's there have gone on and gotten PhD's elsewhere. I am one of only maybe a score and a half or two score of people that have done that. A lot of really famous biologists have come out of there, mammalogist in particular under Gene and more recently under Jerry Choate. I got what I wanted at Hays, I continued to get that

personal involvement from professors who I felt were genuinely interested in where I was headed, but I also got to spend a lot of time in the field, and I took most of the 'ologies, as we called them, like ichthyology, I didn't take herpetology for some reason, ornithology, mammalogy, limnology, on and on and on. There wasn't a semester I was at Hays that I didn't have one or more field classes, but I also took a really good stat class that helped me for years and years, and a variety of ecology classes, and I took a lot of botany classes. At that time, and I think probably still, Hays had an extremely strong Master's program. And now I've forgotten, but in a two year program at Hays, not counting research hours, I think I had something like 80 or 85 hours of actual course work. So they kept us busy, of course in the plains of western Kansas there is not a lot to do sometimes.

We got out occasionally, and one of my favorite stories there at Hays was I had done a lot of duck hunting with my grandfather and with my father, I'm not a big fan of sitting in a cold blind and freezing, but at Hays I learned more about jump shooting ducks, and we went out and we did that a few times. I had never shot pheasant, and I shot pheasant for the first time out there. We went out to Cedar Bluff Reservoir, I don't if you know where that is but you should, and we went out to Cedar Bluff early one morning, there were about six or eight of us, it was Gene Fleharty and about six of his students, and we were all after pheasant. We were walking the draws and Fleharty was up on a little ridge between a couple of draws, I don't think I had ever seen a pheasant to tell the truth and I certainly had never shot one, well I was really focused on the ground and really looking for pheasants and all of the sudden I hear somebody's shotgun go off about three times, and I realize it's to my right and I look over and it's Fleharty, and there are three greenheads dropping from above him. And so he had not just been looking for pheasant but he had been looking for other things as well. And about that time other people started opening up and I realized I was going on and I opened up and I got one. We got all done and Gene gathered us together and he said, "Well now I hope we're not going to have a problem, but do you all have a duck stamp?" And as a matter of fact, we didn't all have a duck stamp and we were already over limit for the duck stamps we had! So Fleharty dispatched a couple of people into town that didn't have stamps to get ducks stamps so that we would be covered. While they were off doing that, I shot my first pheasant; I was between two of my close friends there at Hays, and a pheasant got up right in front of me, it looked like a B24, it was just moving straight and away from me and it was huge, and I just knew there wasn't going to be a problem at all in dropping this bird and so I shot, and it was a hen! And one of my friends said, "Nice hen shot Mike." And we just walked on by it, I felt so bad about that. I don't think I violated any wildlife laws since then.

But Hays was great, we had a really good time, it was a good group of people. I first learned about collections at Hays; Fleharty had started a natural history collection of mammals, Charles Ely was the ornithologist and Ely had started a wonderful bird collection. Fleharty was picky about who could do what in the collection, and for years and years in fact, he was the only one that could write in the catalog. A good friend of mine, whom you know, Tony Mollhagen, had beautiful handwriting at the time and eventually Tony was allowed to catalog in there. But people like me with kind of raggety handwriting just didn't stand a chance, but I prepared a lot of specimens at Hays. The

other great thing that happened I think while I was at Hays was one of the legacies of the department was summer field trips, and in the summer of 1965, Gene and myself and three other students, I think, took a two and a half or three week field trip. I'm embarrassed to say I just discovered in the last couple of years or so that I don't seem to have any field notes from that trip, and I would almost kill if I could find them or if they existed, they may not exist. But we left Hays, we drove straight south, we camped at the Oklahoma-Texas border near a lake whose name escapes me at the moment, but it was at Prairie Rattler City, and we were out trapping and watching rattle snakes carefully, and then each morning we would go run our traps, and you've done this of course, come back to camp and prepare the specimens, hit the road and then go onto the next place. After two or three stops in Oklahoma and Texas, we ended up at the golf course in Carlsbad, where Gene assured us it was a great place to trap Spermophilus Mexicanis. Gene had been there in years past and knew the groundskeeper, and so we went out and, indeed, we cleaned up on Mexican ground squirrels, we got eight or ten of them I think. From there we went to the Sacremento's and we trapped in the Sacremento's. From there we went to the Bootheel Hidalgo County; the first place I ever netted bats and I just fell in love, number one with New Mexico and number two with bats and bat netting. We finished up the trip by going to Tucson and going to the desert museum, which was relatively new at the time. Then we came back into New Mexico and went into Gila and stopped at two or three places that were favorite spots of Gene's. We came into Albuquerque and spent some time at UNM and met Jim Finley, who had been Gene's major advisor. And then back to Hays and back to my last year at Hays. Come January, I went in and talked to Gene about going on for a PhD, and at that time really what I was mostly interested in was teaching in a small school like Fort Hays, that was absolutely what I wanted. To tell the truth, I didn't know what the Fish and Wildlife Service was at the time, I really didn't. If I had been on a refuge, it was transparent to me, and I don't think I had ever hunted wildlife on a refuge at all. Gene thought it would be great if I'd go to New Mexico. I wrote Jim Finley and asked Jim if he was going to be taking any students, he said sure, go ahead and apply. It was the only place I applied. Finally, as I said earlier, I think my dad realized and saw the writing on the wall; this guy is not becoming an M.D.

In the summer of 1966, my first wife, Arnetta, and our two kids, Diane and Shawn, with my folks help moved down to Albuquerque, and we moved into a small two bedroom house not too far from campus, and I started graduate school at UNM. I am sure there are other Hays stories but I'm not dredging any up at the moment. The most amazing thing to me I think, when I look back at my schooling at Hays and then going to UNM is that at both placed I had what I still think must just be mostly incredible good luck at falling in with a wonderful group of individuals, and it happened at Hays and many of those people that I went to school with at Hays and I'm still in contact, we get together and we even go to the field occasionally. Tony Mollhagen is probably my closest friend, and Tony and I have known one another now for 42 years or something like that, we just got back from a field trip to Utah a couple of weeks ago. But the same thing happened at Hays, I fell in with another incredible group of guys that were really interested in animals, really interested in wildlife, most of us pretty interested in systematics and taxonomy and museum collections, and all of really interested in being outdoors. Finley accepted three or four students I think, and about the time I came down here he had just accepted Don

Wilson as a student the year before. Don got his bachelors at the University of Arizona either in 1964 or '65 I believe, yes he got it in 1965, he came over here and when I first met Don Wilson he was working on a masters under Finley, and Don's project was Peromyscus in the Sandia Mountains, sort of taxonomy, distribution, ecology. Don and I kind of hit it off early on; we were both married, we both had a couple of kids at that point, we didn't have the options that some of our colleagues had to run out to the bar at least every night, so Don and I became fast friends fairly early on. But there was, like I said, a great number of other really good students. Dan Williams was just starting a master's degree, he has done a lot of work on heteromyids that you are familiar with. Jay Druecker had just finished a master's degree on Bats of the Bootheel and was working on migratory bats at that point, reproductive cycles in migratory bats. A particularly close friend of mine that was working on birds was John Darling, who was a student of Finley's initially and then when David Ligon came to the department, why John transferred to Ligon and got a master's degree. One individual that was here at the time was an undergraduate, and that was **{Mike Merris}**.

In the spring of 1967 I made my first trip to Mexico, and I had heard a lot of stories, especially from Don, about going to Mexico and in those days most people in Arizona and New Mexico would go to the west coast down Highway 15 to Mazatlan or San Blas, places like that. And so in the spring of '67 I went to Mexico with Don, Mike Merris, who promised as the only undergraduate that he would work and he would cook for us and he would bring some local red and green chili with us, a guy named Keith Grisham, who ended up teaching schools for a career right here in Albuquerque, John Darling the ornithology student, and myself. We took off in my '66 Suburban carryall, which was a gift from my folks, and I don't imagine any of us had more than $25 or $30 bucks in our wallets. We did not have a Mexican collecting permit but we had lots of bat nets and bat poles and a few snap traps. We ended up having a great trip, we just drove Highway 15 past San Blas and then we drove onto Puerto Vallarta, and at the time the road in Puerto Vallarta was incredibly rough, it was essentially a one lane dirt road. I haven't been back since they paved it and everything. But we ended up spending three or four idyllic days in a little thatched hut on the beach at Puerto Vallarta, and every night we were up in the hills netting bats. I think for me in many ways why that was really a turning point, I have never gotten enough of that kind of life, I love being out, I love camping, I love being with friends, and I love looking at animals. We ran afoul of all of sorts of things, we netted some farmers horse one night, and if you have ever seen a horse in bat net, they come literally unglued, you cannot believe what happens. And it was either that same episode or another one, where we dislodged some farmers drainage hose out of the spring, and so we are about halfway through having a great time looking at our (unclear) of four to six different species, and this very irate Mexican farmer comes up, and in those days Mike Merris spoke virtually no Spanish as near as I could tell at least, he may have understood a fair amount of it, and Don spoke a little but not a lot, and it wasn't until the farmer reached down and grabbed this hose and started shaking it, and he was really mad, that we finally understood what had happened. So we promised him we would put everything back the way we found it and we wouldn't disturb him after that. But we came back from Mexico with two new tires on the carryall and no spare, and no money,

and probably 30 or 40 specimens of mammals that Jim Finley was perfectly happy to acquisition into the museum.

Let me go back; one thing that I overlooked was what I actually did at Hays for a masters thesis John, and I must admit, I think when I went to Hays I knew that I was going to do a masters thesis, and I think Tomanek and Fleharty had explained to me a little bit about what that was going to be about, that was the terminal degree then and now at Hays. But I think when Fleharty finally called me into his office, and this was probably the winter of 1964 or '65, and by then I had a semester of classes, I had mammalogy, I was taking ornithology, I had had Fleharty's ecology class, so I was starting to pick up on a lot of the lore of Hays and of people that you and I know in common, but when Fleharty called me in and said, "Well Bogan, what are you going to work on for your master's degree?" I was just clueless, I didn't have any idea. As nearly always is the case why truthfulness is the best out, and I just said, "Dr. Fleharty, I really don't know." He asked, "What appeals to you? What do you think would be fun?" And I said, "Well, I would really like to do a field project, clearly I want to work on mammals for my masters degree. I got out and saw a lot of neat animals and a lot of neat habitats last fall in mammalogy, so something in that category is I think what appeals to me." Gene then and now was really an ecologist, and he sat there and thought for a minute and he said, "Well, do you remember when we on that field trip to Cedar Bluff Reservoir and I was talking to the class about a field of here that really had a nice growth of mid grasses on it even and even some nice blue stem, and right next to it was this pasture that had been severely overgrazed and there was buffalo and blue grama out there, which are natives of course, but none of the tall and mid grasses at all?" We did a little trapping out there and he talked about that, and he said, "You know, it might really be nice if somebody would go study some replicates of that situation, and just look at small mammals on grazed and non-grazed grasslands." And he suggested that if I was interested in that, why it would certainly be a good master's thesis. I'd certainly want some botanical input, and one of his close friends and colleagues there was a botanist by the name of Gary Hulett, and he suggested I go talk to Gary about the plant end of things because obviously we would want to quantify the impact from grazing and so forth. So I ended up talking to Gary, I also went back and talked to Jerry Tomanek some about the same thing; Tomanek was really the grasslands person at Hays and, in fact, I took a grasslands course from Tomanek. The only course I ever took from Hulett I think was the biometry class that I mentioned. But both of those individuals thought that probably the best way to approach this would be to go out and clip the vegetation on all of my study sites, go out and trap them and then late in the summer make a round so that they've got a summer's worth of growth where appropriate or where it happened, and go back and fill shopping bags, paper shopping bags with whatever I could clip out of a square meter quadrant. And so that's what I did, and then bring that back, dry it, weigh it. I also did a point frame analysis on all of them, all of the study sites so that I had mammals I had trapped, I had plant diversity in relative abundance, and I had grazing impact based upon weight of dried forage. So that's what I did for my master's degree. No surprises there I might add, and I'm sure Fleharty knew this at the time. The grazed grasslands tended to be inhabited by ground squirrels of one kind or the other and deer mice, and the non-grazed grasslands, why voles in particular, but a much greater diversity in general of mammals. I also took a fair number of

Sigmodon, I had some areas that were frankly successional areas, and that was built into the design as well I think. In those successional areas I took a lot of Sigmodon, this was at a time when Sigmodon was just starting to be appreciated for its northward movements into Kansas and subsequently even farther north then that. So I was out, I did all of this field work on my own, I think a few guys might have gone out with me a time or two. I remember going into a mid to tall grass pasture one night to run the traps, I'd set the traps at like 6:00 and I'd go back two or three hours or later and get the first catch out, go to bed, drive home if I was close to Hays, if not I would camp right there, get up in the morning, pick up the traps and go onto the next site. One night at a site not too far from Hays, I ended up getting lost in this huge pasture. I was certain I was going to be able to drive out on the tracks I made coming in and somehow when I made a u-turn I missed those tracks, and I thought I was never going to get out of there. The wind started blowing, and it even rained a little bit. I made it through and persevered. The regrettable thing about the master's is that I never published it, and I don't think it was until years later that I took another look at that and I thought at least at the time I could have published that, there was some pretty good data in there.

One of my good friends here at New Mexico was a guy named Hal Black, and it turned out Hal had done a very similar study in Utah. He was a student of Steve Durrants' at the University of Utah, and Steve leaned on his people, even at the masters level, to get these things into print, and Hal had gotten some money I think from the Forest Service or USDA at least, and he managed to publish his thesis. It was very, very similar to mine, we often compared results. I think I tend to view a master's degree as an opportunity for a young student to learn how to do research, and I can almost agree with the idea that whether they actually do really good research, much less great research, is almost incidental. If you get a good master's thesis, that's almost a bonus. But what's really important I think is the process, getting into the literature, talking to people, incorporating ideas, doing the statistics, and so on and so forth.

The one thing that I did do Hays that turned into my first publication was a biometry project that Tony and I did on wind training in prairie trees of all things, so I'm actually published in the botanical literature. Everybody in biometry had to pair up and then do a research project, and I think it was probably Tony's idea, but we had been out quite a little bit and the wind really does train prairie trees, the longer branches are always in the lee of the tree and the shorter branches are on the windward side. We were new into ANOVA's and fancy things like that, and we just wondered if we could establish that wind training in any believable way. And so we developed an experimental design, bounced it off of Gary Hulett, I'm sure he had some input to it, and then Tony and I went out and measured trees in four to eight different directions, measured the canopy, measured the size of the bowl, tried to choose representative trees from across the landscape in western Kansas, and then went back and cranked through an old Marchant Calculator that you had to hand crank literally. Low and behold, the F values were significant, and under certain condition these trees really were being trained by the wind. At that time there wasn't a lot of literature on this, I remember that the only paper we found that was even close was in the Columbia River Gorge, where somebody had done a fairly similar study. I don't think we found that paper until later. But after I left Hays and

then a year later Tony went to Texas Tech, why we got to talking and, again I think it was Tony's idea, we really aught to publish that paper, and we did. In 1998, Terry Yates and I co-hosted the Southwestern Naturalist's Meetings here in town, and Terry and I got up and gave sort of the welcome to the group that first morning, and I mentioned that my first paper had been published in the Southwestern Naturalist, and that I had always been proud of that paper and that I thought the Southwestern Naturalist showed great foresight in accepting that paper, and almost as an aside I mentioned that it was on wind training in prairie trees. And after Terry and I did our welcomes then the president of the Southwestern Naturalist got up and gave his greeting back to us, and one of the first things he said, he was a botanist, he said, "You know, I remember that paper." I don't know if he was telling the truth or not.

I think the master's thesis for me was important in introducing me to how you do research, and that became obvious to me fairly early on then when I came to New Mexico because I was often involved with this project or that little project in the collection here at UNM, and we did projects I think in several classes and Finley was always giving other people advise on how to do things, how to prepare something, how to measure something, and so on and so forth, and he pretty much left me alone. I think at the time I was a little puzzled by this, but in retrospect I think Finley just trusted Gene to bring us up and to teach us the right way to do things. I remember one thing in particular we did, we were doing some little project on skull measurements, and what Finley wanted to demonstrate was that people who had never done research or who had limited experience to that were going to have larger errors around there main values and, indeed, it turned out to be true. Those of us that had a master's degree, whether it was from Fleharty or not, were typically a little bit more careful and smaller SE's around those means. But Fleharty was a student of Jim's. Another individual that I later spent a lot of time with was Clyde Jones, and Clyde and Gene had both gotten there bachelor's degrees at Hastings, I think they were a year apart as I recall.

John:
I think 1959 and '60 as I recall, I think Gene was '59.

Mike Bogan:
Yes, and so Gene came down here first and then Clyde came down a year later, they had played football at Hastings, I've heard them talk about their 60th reunion for the football team, that team from Hastings won their league or their division or something like that, I don't remember. But indeed they both came out of Hastings, I later met someone you know well that you've interacted with a lot, and that was Hugh Genoways, Hugh was out of Hastings. As you said, has there been some grand design in all of this or has it been sheer luck that we have all ended up where we have ended up? But it was at Hays I think where I first started learning the lore of places like Hastings and New Mexico, and what Gene was trying to build at Hays and what he eventually did build, and Jerry Choate has carried on. A lot of these names, especially in those early years for me, just kept cropping up again and again and again. Gene and Clyde, Art Harris was another early student down here, an excellent plasticine paleontologist, and those guys I think really caught Finley at his apogee, so to speak, for field work. Art and Clyde and Gene and Jim

were all over the state of New Mexico, they literally skimmed the cream off of mammalogy in the state, and they went to all of these wonderful places. When I later came to New Mexico and did field work, I went to very few places that those guys hadn't already been there; their footprints were always out there in front. But you learned so much from those people, I learned a lot from Gene and it was a real treat for me to come down and work for Jim.

One of my regrets with Jim frankly was that by the time I got down here in 1966, Jim was sort of starting to think about the next phase in his research life, his family was growing up, I think maybe that was calling to him a little bit, but he started spending more time at home and less in the field. I think I was out in the field with Jim three or four times, we took his mammal class out a couple of times I know and, again, try to pass some of his lore. I have often wished that maybe I had been born five or ten years earlier, I could have been down here with Gene and Clyde and Art and the rest of them at that time. But we did okay, I got out and I saw a lot of the state, I saw a lot of the state's wildlife, I made multiple trips to Mexico. Again, I think I had been at New Mexico at UNM only a semester and half, and I had the predictable talk with Finley this time about what I was going to do for a PhD and, again, conducting field work in support of the PhD was important to me, I wanted to collect new material that would shed light perhaps on systematics or variation or taxonomy of some mammals. It turned out that Jim was actually working on a couple of small species of Myotis at the time, Myotis californicus and Myotis ciliolabrum and, in fact, he had measured up some animals in the collection and currently had an undergraduate that was measuring specimens, and he suggested I might take a look at the data sheets and maybe run some statistics and see if that might be something that would appeal to me, in particular, whether or not working on these small Myotis might appeal to me. One of the things that were going to be required was some additional field work at different localities to collect samples for comparison. In addition, it was obvious that I was going to have to borrow specimens from elsewhere, and Jim had been over to the collection at the University of Arizona and was familiar with what Lendell Cockrum had over there. So early on it was obvious to me that visiting other museums and looking at other collections was going to be an important part of it as well.

And so the summer of 1967, I started field work on my PhD. Jim and I sat down with maps and with the 1959 edition of Hall and Kelson, that sort of served as a bible for a lot of us I think in those days, and talked about and looked at lines that had been drawn by Hall and Kelson and by others, and looked at descriptions of these species, and made decisions on where to go. And so in the summer of '67, I spent that entire summer, and I think I had a Shell credit card or something because that's how it worked in those days, we didn't have Bank Americards and Mastercards, I think there were probably American Express Cards but there was no way I could have gotten one of those, and so myself I think and a lot of my friends we had one or two gas credit cards, and you could also charge motel bills on those, at least some times, and so on and so forth. I spent my whole summer traveling all over New Mexico and part of Arizona, netting small Myotis and preparing specimens, and not just of californicus. Almost anything that flew into the net was going to end up in the collection. Once a month I had to decide how I was going to

pay that credit card! I don't think I got any money from Finley that first summer of '67, I got some money from my parents that helped support those early field costs, I had saved a little from my TA in the winter, but there wasn't a lot there. But I just remember it was all great fun until that Shell credit card bill came due once a month, and that was always a struggle and you had to pay them all off as I recall, I don't think you could carry a balance fortunately. In the summer of '68, Finley and I had agreed that we really needed new samples of these bats from Mexico, and I had a good friend, Mack Hardy, who was a PhD student in herpetology here at UNM, who was planning a trip to Mexico, and Mack and I compared notes and a lot of the places that I wanted to go were the same places where Mack wanted to go. He was working on a group of snakes called blind snakes, Typhlops, but I don't remember some of the other name related genera. But Finley provided some funding for that trip, he had gotten a small grant from the NSF, and it was to revise Myotis, and he decided that supporting a two month field trip in Mexico for me to go down and collect Myotis was appropriate to his research goals as well. So I got some money from Jim, and Mack and I took off and went to Mexico and spent the summer. We ended up spending about a week's total time in Mexico City and we visited the collection there in Mexico City at Autonomous National University of Mexico (UNAM), and I first met Bernardo Villa down there. We stayed with a young mammalogist in Mexico City, then young, by the name of Ticul Alvarez; Ticul, now let me think about this, Mack Hardy had gotten his master's degree at KU, I'm not sure whose student he was, Fitch perhaps, and a colleague of Mack's was Roy McDiarmid, who later went to work for Fish and Wildlife in the museum in Washington. But Mack had also met Ticul Alvarez at KU, and I think probably they overlapped a little on Ticul's master's degree, and then I think Ticul had come back and visited a couple more times. But Ticul had told Mack, "If you're ever in Mexico City let me know, you can stay with us." And he and his wife were just consummate hosts, they were just wonderful, and they treated like us family. Like I said, we stayed for about a week and every night we would come home and his wife would have made some wonderful Mexican meal, and we'd have a few beers or a little Tequila or something like that. We spent the day in the collections down at UNAM or at the collection that Ticul was in charge of which was at the Polytechnic University there in town. It opened my eyes to interacting in an international scheme of things I guess, and of course they spoke great English but it gave me a chance to practice my Spanish a little. Mack and I had a great time that summer, we had camps that got rained on and we got flooded out of, and I remember taking my Coleman stove down to the creek and trying to rinse the mud out of it. I think I lost half of my bat nets one night in a flash flood, all of the rebars in the aluminum conduit were bent double and the nets were just history. But I saw bats that I just never expected to see, we caught a lot of fruit bats and we caught and saw vampires when we got farther south. So it was a great trip, it was an eye opening trip for me I think. Mack and I continue to be pretty close friends; he has ended up spending his career at Shreveport, University of Louisiana at Shreveport.

But we came back and it was about that time I think that I became an assistant curator of the museum, which was one of the better TA/RA appointments that you could have, and it really gave me a chance to kind to learn the nuts and bolts of how to care for a scientific collection. Jim was around, and he and I would kind of talk about my marching

orders, but I ended up supervising the work study students that were working in the museum. At that time, most of the graduate students were responsible for taking care of their own museum material. That's not so much the practice now I think, but at that time it was. And so I was mostly responsible for taking care of my own stuff and then incidental materials that came into the museum, and there were a surprising number of those. I mean we skinned the occasional zebra and monkey from the Rio Grande Zoo as well as collections that somebody else would donate to us. I continued working on Myotis californicus, I took a semester off and went and replaced Gene Fleharty on a sabbatical appointment at Fort Hays. That was very much inline with my career goals, I was still looking at returning to a school about the size of Hays or maybe Hasting even, something like that, and teaching, that was uppermost in my mind. I went back and had a great semester at Hays, I had an incredible teaching load, I think it was at least three if not four classes, and so I was really, really busy, but I really loved working with the students. One of the classes was a field techniques class, and so we spent a lot of time in the field, we did some overnights. All it did was really strengthen my desire to go ahead and teach in a small to medium sized school, a school even the size UNM was not very appealing to me, and in those days UNM was a lot smaller than it is now, I think UNM was probably 9,000 or 10,000 maybe tops.

That time in my life is sort of mixed because I went through a divorce at about that time. I was gone a lot, I take a lot of the blame for that one frankly, I don't think I was there when I should have been, clearly I wasn't. So I came back from that sabbatical leave and went through a divorce. The person that really saved me at that point, Finley I think was maybe starting to doubt the wisdom of having accepted me just a little bit, and partly I think that's because Jim, like a lot of advisors, myself included now, kind of want to see students march through and get these things done, maybe not in lockstep fashion, but in some sort of a predictable fashion, so you know that one of these days they are going to be the hell out of here you know, and somebody else can come in the door at that point, and I think I was probably jamming things up a little for Jim. On the one hand he was really encouraging me to get finished and move on with my life, but exactly what was going to pay for that was not clear. And at that point, the chairman of the department of biology here at UNM stepped up, that individual was Loren Potter; Loren and I remain good friends to this day, he is still alive, he is still active, he is a senior ski instructor at Sandia Peak in fact, and he is well into his 80s. I'm not sure what Loren knew or didn't know, but I was a pretty good teacher and I think Loren knew that, and I ended up being one of the very first people that was ever hired to fill what is now called a lecturer spot in the department here. And I took over and ran the biology for major lab program and supervised the TA's in that program and I also taught two sections of anatomy and phys, and that was a class that was mostly for nurses, dental hygienists, and a few jocks were in there. And so I had two lecture sections of that and then I had to oversee the A&P lab as well, and so I was really, really busy, the pay was more than I had ever made, and I think at least when I left UNM, I think I was getting paid between $8,000 and $9,000 for that. In retrospect, that was... should have been paid more probably!

But the downside of that was that it didn't leave me a lot of time to keep working on Myotis californicus, but I would struggle along. Then in 1970 or '71, the first meeting of

a group of Southwestern Bat Biologists was held in Tucson, and that meeting I might have been in Hays that semester, I don't quite recall, I didn't go to that first meeting, but the next meeting, that annual meeting, they had decided to have a second one, and that second year that meeting was held in Albuquerque at UNM, and I couldn't not attend that meeting, and furthermore, I wanted to present my first paper. I had done some comparisons on Myotis californicus and what we now call Myotis ciliolabrum, I had measured skulls and recorded the external measurements, and I had done some statistics and I thought probably without too much work I could go ahead and expand on that a little bit. So I gave a paper at the second annual meeting of the Southwestern Bat Biologist group, I honestly don't remember if there was a more formal name or not. That group still exists, it's now the North American Symposium on Bat Research, they meet every year, and they are now having their 38th annual meeting I think this month. That was an eye opening event for me, I gave my paper, it went pretty well, Karl Koopman, as he always did, was right on the front row, and I had finished my paper and asked if there were any questions and Karl's arm just shot right up, and you've heard him talk but it was, "Mr. Bogan, this is a very interesting paper you've presented here on geographic variation on these two species, but I'm just wondering what you can tell me about character displacement and how that might fit in." And I had worked on character displacement a little bit, so my answer wasn't a total disaster, but I was just paralyzed that the very first question I was going to get on this was going to be from somebody like Karl Koopman, whose name I knew well of course, but it all went great.

One of the individuals that was at that meeting was Clyde Jones, and Clyde had left UNM, he had gone to Tulane (you really should interview him and get his take on all of this) and he got a grant to work on primates in Rio Mooney, now called Equatorial Guinea I think, and Clyde went over and spent two or parts of three years maybe in Rio Mooney; he took his family over there and did some wonderful science. He wrote some bat papers, he wrote some rodent papers, he wrote several primate papers, and they are wonderful papers, they are some of the best work I think that has ever been done on small mammals in West Africa. Clyde had come back then to Tulane, and I think maybe at the time of the Albuquerque meetings, he had just been hired by Fish and Wildlife Service. I met him that year but didn't spend a lot of time with him I don't think. Many people assume that since I went to UNM a long time ago that overlapped with Clyde and Gene and so forth and that, of course, is not true. It was the early '70s before I first met Clyde. The next year the bat meetings were in San Diego, and I went to San Diego and I gave another paper. By that time, Don Wilson had completed a post-doc with Dan Jansen in Costa Rica, and Don had been hired by Clyde to be a mammalogist in a mammal section at the museum in Washington. And so at the San Diego meetings Clyde sought me out and he asked me what I was thinking about doing when I completed my PhD, and I told him that I had started to apply for teaching positions, mostly in the west, and that I was pretty discouraged. I had applied to several positions where I was one of 50 to 100 to 200 or more people that had applied, many of these positions were in places that I wasn't very anxious to go to, parts of the country where I didn't want to live, I told him that it was a little disheartening, and he said, "Well, when do you think you're going to finish?" And I said, "Well right now I'm hoping to finish next spring, the spring of '73." And he said, "Well, I can't promise anything because I work for the federal government now and

I have to be careful what I say, but there is a possibility that we might have some funding next spring to hire a mammalogist. Is that something you would be interested in?" And I said, "Well, what kind of a job is it?" And he said, "Well, it's in the Smithsonian Institution." And although I still hadn't heard of Fish and Wildlife I don't think, I knew what the Smithsonian Institution was, and furthermore, I was familiar with the lineage of that august group back there that started with C. Hart Merriam and Vernon Bailey and on and on and on, Palmer and Fisher, you name 'em. I said, "What's the job about?" And he told me it was in the Smithsonian Institution, it was going to be as a curator of mammals, that I would have territorial responsibility, I would have research responsibility, and then for the outfit that was going to pay my paycheck, there would probably be some work that I would have to do in connection with taxonomy or reviewing things or something like that. And I said, "Who is going to be paying the bills on this?" And he said, "Well, it's the Fish and Wildlife Service." And so that was really the first time that Clyde explained to me, that I had ever heard explained, exactly what the interactions were between the Fish and Wildlife Service and the Smithsonian Institution back there in D.C.

As it turn outs, Clyde not only had money to hire one mammalogist that next spring, he had money to hire two, and so the two people that he ended up hiring, and there were several good people that applied for those jobs, and some I've seen heard wish they had applied but didn't, but the two that got hired were Al Gardner, who had just completed a PhD under George Lowery at Louisiana, LSU, and myself. I was actually signing some of the paperwork for this job before I had defended my PhD. I was in nothing short of a panic about whether I was going to get the damned dissertation done in time, but it all came together. The dissertation ended up being primarily an examination of systematics and variation in Myotis californicus. The ciliolabrum end of things I had sort of taken care of in a paper that was published as a result of that one I gave at the bat meeting. So in those days we didn't worry about multiple chapters, I mean whole dissertation was just one chapter, and it was about Myotis californicus, and it was subsequently published as the first research bulletin of the Fish and Wildlife Service I think. The then editor for research of FWS and Clyde and others decided we should put together a research bulletin of the Fish and Wildlife research, and so my dissertation ended up being published in that series. But I got finished up and by then I had remarried and had a young son, a third child Justin, and in June of 1973, why we all packed everything we could into a vehicle and took off for Washington, D.C. and began a career back there.

Well there I was, at the Smithsonian Institution, I was absolutely awed I think when I went back there, and it was interesting because the fellow that had been hired at the same time as I had been hired, Al Gardner, he was up in Alaska doing a mammal survey on the Noatak. Don Wilson, whom I had expected to be there and lead me through the minefield a little bit, was in Costa Rica of course, and Clyde was there but was getting ready to leave on a month long field trip with Bob Fisher, who was our mammal collection manager at the time. But Clyde introduced me to the Service, my very first day we went over to the Matomic Building, which I think is at 1717-H, it's amazing how those things pop out at you, and I went over there and I got signed up and filled out papers and so forth, several of which I recently saw again when I got a copy of my file when I retired. But I hadn't been at the museum more than a day or two before Clyde

walked in and dropped a stack of papers, maybe three-quarters of an inch thick, legal sized paper, and down one side was a series of scientific names of marine mammals and then across horizontally were various characteristics of those species, a brief description of where they lived and what they fed on and what research had been done and so forth, and these papers had been about half filled in, and at the time Clyde was working on either or both of CITES list and marine mammal protection taxonomic list, and he needed these things completed by the beginning business the next day. And one thing I didn't learn in the arid southwest working on bats was anything at all about marine mammals, and I hardly knew where to go to begin! But the point of the story is that I don't think Clyde did this with everyone, but I think he knew it would work with me or he could get away with it with me or something, but almost from the get go there at the museum, Clyde assumed I could do research because I was a Finley student like he was, he assumed that I could curate because I spent time in collections, but I think he wanted to make sure that I was also contributing to the Service and trying to help FWS with some of their information needs.

I ended up during my tenure in D.C., I was in D.C. for a little over eight years, and I served in just about every acting capacity that I could have in the Matomic Building, I was over in Main Interior on an acting assignment or two, those assignments were anywhere from a day or two to maybe a week or so, I think was acting chief of Wildlife Research in the late '70s for two weeks or so maybe. That education I don't think was ever wasted on me, I learned what the FWS did at least, and not just research, I certainly learned what was going on in research, but I learned about regions, I learned about refuges, I learned about migratory bird programs, the whole bit. I really enjoyed that, everybody complains about those acting assignments and so forth, there were a lot of people in the museum that Clyde never asked to do that. At the time, I didn't think much about that, in later years why that's meant something more to me I guess. I really valued that. I would say that even though there are always downs, I liked the service part of working for Fish and Wildlife Service as a matter of fact. There were others who I think maybe emphasized curation more and others who emphasized research more, Wilson was and is an extremely prolific author of good scientific papers, and Gardner is probably a better curator, but neither one was probably as adept as I was at going across town and being acting somebody or the other for a few days. In later years, as I became a research supervisor in Colorado, I knew who to go to if I had questions, I knew what the pathways were if there was a funding problem and so forth, and so even though the payoff took a few years to come to light, it really did pay off for me. By the time it started to pay off for me as a research supervisor, Clyde was gone, he had taught me what he could teach me and then he was gone, but we had great times in the museum. Believe it or not, the Natural History Museum is not my favorite my museum, when they opened the Air and Space Museum that was the one I used to go to during my lunch hour.

But that first summer I was in the museum, I would spend my lunch hours upstairs, and in those days all of the original typewritten reports by the early biological survey field crews were all upstairs in these file cabinets, and I would grab a quick lunch of a sandwich and I would go up and spend twenty minutes and I would read these old reports. I read Vernon Bailey's reports of his New Mexico travels and C. Hart Merriam's

reports of being in California, and on and on. It was very humbling to be there and to be able to read those reports, it was quite a privilege and honor I think. In general, we had excellent interactions with our Smithsonian colleagues, I never felt like they looked down there nose at FWS curators; we all worked as a group together. I was essentially encouraged by Clyde, the first thing he wanted me to do was to write up my dissertation and get it published because he had been working on a new outlet for FWS in that regard. Beyond that, he really encouraged me to maintain my interest and my expertise in bats, but also to think about anything else I might want to do. He had a project of his own in the west where he was going back to a lot of Merriam's original field sites and was collecting; I was encouraged to join him on that. One of the things that took up a lot of our time was that I think Clyde and Don together decided that what we really needed, so to speak needed, was a research project in Mexico, and Clyde had been tapped by Lynn Greenwalt, who was then the director, to participate in some of the annual meetings between FWS and la Fauna Silvestre in Mexico. And in later years I attended several of those meetings, I gave the report for research at some those meetings, and other years I just participated on this or that mammal project. But Clyde encouraged Al and Don and I to think up something, and what we thought up was the mammals of Nayarit, and we ended up spending a fair amount of service time and service dollars, obviously, in the Mexican state of Nayarit. We collected thousands of mammals from that poorly collected area. The unfortunate thing about it is that we have not yet gotten that all synthesized for the final report on the mammals of Nayarit, but we have published a lot of papers in the intervening time.

I continued to work on bats while I was there at the Smithsonian, but another thing that I got into fairly early on was assisting FWS Refuges with information needs they might have on mammals. Clyde and I had talked some about doing a little report on Florida mammals, that was at least partly because there was an old manuscript by Allen, I think, on mammals in Florida, and we thought maybe we could build on that with some recent information. I ended up starting what later became sort of a major focus of my federal time, and that was conducting mammal surveys on public lands. Some of the earliest work we did was actually done in conjunction with the forest service in Florida on the Ocala National Forest. There were a lot of questions that were being raised at the time regarding phosphate mining and so forth in Florida, and we went down and conducted mammal surveys for some priority species of concern of the forest service on the Ocala National Forest, but then we also began responding to some information needs that some of the refuges had down there at the St. Marks National Wildlife Refuge. I ended up, I think, making four different trips to St. Marks, and we collected a lot of mammals, there were a few papers that came out of that. We also did bird work and herp work as well, and the final product, I think from the service standpoint, was a report to the refuge on what we had found. Some of those were couched in terms of comparisons, again burned and non-burned or grazed and non-grazed or pigs and not pigs in the case of some of those refuges down there. But again, personally I felt really good about doing that kind of work, I really liked the museum, I liked collections and systematics and taxonomy. Most of my papers at that time continued to be explorations of variation and so forth and systematics of mammals, bats but not solely bats, I also did some carnivore work, did some rodent work at the same time, and I often presented those papers at the mammal

meetings; Clyde was big on all of us going to the mammal meetings and that kind of thing. I really liked trying to help out people in the field, perhaps I was naive at times, but in general I felt like most of the people at the refuges really needed this information. Many of them had at least some of that information anecdotally, but they didn't have it enough to try to refute this or that freight train that was coming down the track at them.

So that was really the start of that, and I subsequently have done a lot of work on refuges, and probably the best refuge work we did was in Nebraska on the various refuges up there. Later I moved into working on national parks and doing essentially the same thing. But that was the heyday of my federal service I think. I am a little embarrassed to admit it, but we had an annual work plan of some kind, and we had to write progress reports, publish papers, and so on, but I was almost ignorant of any real budget, if there was something we needed to do, or even frankly something we wanted to do, why usually we had the money to do it. And it stayed like that through the 1970s until 1981, when Reagan came in. I remember thinking at the time and realizing as a lot of feds did, that the times were changing, this was going to be a different way of doing business. In the early '80s, I had just started a cooperative project in Baja, California with la Fauna Silvestre, we were still working on the west coast of the mainland but we had slowed that work down a little bit, primarily due to what we perceived to be increasing drug traffic. We had an episode or two in the field that I can tell you about some time; it's in the book chapter I might add. We also had a lot of unannounced checks in the field and on the highway, and I think even Al Gardner, who spent a lot of time in those situations, was feeling a little uncomfortable about traveling down there. We had been approached and in fact I had done a preliminary trip or two to Baja, California, and so I started a Baja, California project, and Clyde was going to be a co-PI on it, but essentially it was going to be the mammals of the southern part of the peninsula and we had several cooperators with la Fauna Silvestre down there that we worked with in the field. Again, I guess the word is applied, but the applied aspect of this of trying to help somebody in the field, tease out something or understand something, is something that applied to me, it resonated with me I guess is the term. I really enjoyed doing that, I enjoyed working in Mexico, and I never really had a bad experience in Mexico.

It was very much because I wanted to continue that work in Baja, that when Clyde made me the offer to move to Fort Collins in 1981, I took it pretty seriously because I thought the way things were going in D.C., it would be much easier to stage a trip to Baja, California from Colorado then it would be from northern Virginia or D.C. At that point, Clyde had moved from his position as director of what was initially the Bird and Mammal Labs, that's who I went to work for, that's an historic name that goes back a couple of decades at least, Hartley Jackson was one of the individuals who worked for the Bird and Mammal Labs years ago. The Bird and Mammal Labs was down to about four people at the time Clyde came in, and he was hired by Tom Baskett as I recall, who spent a lot of time at the coop unit in Missouri, and Baskett, I think, essentially hired Clyde to try beef up the Bird and Mammal Labs and bring them back to their prime. Clyde took that charge seriously, and by 1980 or so, when he then became the Director of the Denver Wildlife Research Center, the staff in the Bird and Mammal Labs, which by then had a different name, National Fish and Wildlife Lab (NFWL), we by then had I'm guessing

fifteen people in the museum, from a staff of what had been four. In addition, we had three or four field stations with ten or more people at each one of those field stations, and the old Bird and Mammal Labs was no longer just doing museum work in D.C. and the occasional trip out to a refuge, but we were also involved in ecological studies in California, Colorado, and Florida, and we were involved in marine mammal research in Florida with manatees, and in California with sea otters. In addition, we had a couple of people eventually stationed up in Alaska who were doing research up there, there was another big Alaska component to research at that time as well. But Clyde moved to Denver; Denver's primary reputation at that time, and still is to a large extent, is animal damage control, pests and so forth, what do we need to do to understand their biology so we can apply control methods in an educated manner. One of the first things that Clyde did when he moved to Denver was coerce the powers that be in wildlife research in D.C. that the lab he had just left should become part of the Denver Wildlife Research Center. An assertive move, I think to say the least, and so before long our marine mammal component, our museum component, and the ecological component were all a part of the Denver Wildlife Research Center. What had happened in the old lab was that Bob Brownell was hired by Clyde to take over the marine mammal work, Don took over the museum work, and I became responsible as a branch chief for ecological studies. At the time Clyde moved to Denver, Chuck Stone was the ecologist, and Chuck left not long after Clyde's arrival, and I can only speculate on what was going on there, but clearly Chuck wasn't interested in staying under Clyde, I guess that's obvious. And so Clyde restructured slightly and he opened up two positions, one of them was the chief of ecology in Fort Collins, which I ended up taking, and the other was a new chief of mammal pest studies, and Mike Fall, who was already at Denver, was hired to fill that position.

And so in 1981, I moved to Fort Collins and made weekly, or more often trips down to Denver, the management team would meet down in the basement of building 16. I became the supervisor of records for fifteen or twenty, at times more than that actually, but the core duty was responsible for fifteen or twenty research grade scientists. I was not intimidated by that, by that time I had almost nine years in with the service, I felt like I had been well trained and I was ready to do this. I was concerned about what it was going to do to my research career, and it certainly had an impact in that regard, but I liked my time as a research supervisor. Almost all of the individuals that I supervised, and you know all of these people, but they certainly included John Oldemeyer and Fritz Knopf and a whole host of other people, they were all excellent scientists and they were serious people who knew how to have fun nonetheless. The thing I really liked about the ecology group was that continuing commitment to the service, at least as I perceived it. The marine mammal folks were very much into their animals, and not always was that clearly related to service needs or certainly not to service property. The museum group was, so to speak, perhaps the most esoteric group of the three. But the ecology group, most of these guys that I was supervising had at least one project on a FWS Refuge, and most of them, several of them had only projects on refuges, and again, Fritz Knopf comes to mind, he had just started a project at Arapaho at that time. I really liked that continuing obligation and responsibility to the service, it just really worked for me, and it never didn't work. Things looked very good for me for about the first year I was in

Colorado, and then Clyde, I think, had increasingly come to the attention of powers that be in D.C., and I think he had also crossed swords with a few folks locally, Region 6 maybe even, and so within a year of my moving to Fort Collins, Clyde was called back to D.C. to serve as ARD for the Endangered Species Program, (I can't think of the guy that had been in there) but Clyde was acting and I think the plan was, as I recall, the expectation was that he would be put in there permanently. Neither Clyde nor his wife, his then wife, were enamored of returning to D.C., and I've heard it described in various ways, but the best way to say it I guess is that Clyde and the Service agreed to a mutual parting of the ways, and I think Clyde walked away with his retirement money and so forth, and everything was fine. What Clyde was going to do next was an opened questioned but, as you know, he ended up going to Texas Tech and became director of the museum down there; Knox Jones had a lot to do with hiring Clyde at Tech.

The next director of the Denver Center was Paul Vohs, a long, long reputation in NFWS, but a very different person than Clyde, a very different background than what Clyde and I had shared, and my life changed a lot when Paul came in as the director of the center. The primary way it changed was not so much in my service duties, if you will, Paul left me alone, I think he had complete confidence in that, but Paul had other things that he wanted done and one of those of things was he didn't really see a place in a branch chief's time to do research. I made one or two more trips to Baja, and then essentially the message was that I was going to have to call that to a halt because we just had too much to do. By December of 1985, I was personally supervising through three people below me I was supervising a group of research scientists that was bigger than northern prairie at the time! What happened in December of 1985 was that Congress decided in its infinite wisdom that the animal damage control program and its research, almost exclusively done by the DWRC (Denver Wildlife Research Center), would be much more responsive to farmer's needs if it was in the U.S. Department of Agriculture. And so in December of 1985 they moved all of the DWRC into AG, except for the ecology branch, which I was supervising, which still included museums, marine mammals, and actual ecology studies. So we were a branch without a tree; I actually called Rey Stendell, who was the director at northern prairie, and pleaded with Rey to pick us up, and Rey said he wasn't against that at all, he would like it, but he said he just didn't think that was something that he was going to be able to pull off. What ended up happening is that there was already another sizeable FWS group in town that was associated with what was then called the Office of Biological Services (OBS program), something that Ted LaRoe was instrumental in getting off the ground, and that group was the Western Energy and Land Use Team (WELUT), and what ended up happening was that my branch became a part of WELUT, and I became the assistant director of WELUT, and the director was the former, they didn't call them directors they called them chief's I think, was the former chief of WELUT, and that was Ralph Morgenweck. I liked Ralph a lot, and I still do, I have nothing but respect for Ralph, he is the best, bar none, he is the best listener I've ever met in my life. He can sit in a meeting where everybody is hollering at him or at somebody else, and at the end of the meeting he can synthesize exactly what happened, he's beautiful. Having said that, however, I will say that being the assistant director of this new group was the worst job I've ever had in my life! It was all office work, it was very

bureaucratic, I was saddled with more responsibilities for budget because it now included not only the folks I'd been supervising but others as well.

The best thing that happened, that merger happened in the spring of 1986, and I think it was probably the fall of 1988 that I finally arm twisted Ralph into letting me go back to a research position. And so I went back to a research position, we were still occupying two different facilities there in Fort Collins at the time, but I went back and started trying to pull together the pieces of my research career, and some of that was still dealing with refuges, and it also started to include parks as well on the Colorado plateau. That worked liked a charm until, and I loved doing what I was doing, Ralph left me alone; Ralph departed not too long after that, and Frank Dunkle came in following his tenure as Director of FWS, an extremely interesting situation, we can talk about that another time. One of the changes, for example, that Frank implemented was that any time one of us were going to a meeting where there was going to be a representative of another agency there, either state or federal, we were to wear ties, and for all of us, OBS or research, was a complete turnaround. Eventually what happened was that too a large extent, I think, the research functions that I had been responsible for and the OBS functions that Ralph initially, at least at that time, had had responsibility for, they merged, it was not the happiest marriage in the world, but it wasn't a bad one. There was, and I think still is, a certain amount of confusion and disappointment and misunderstanding about what research grade scientists do, and whether everybody should be in the research grade or not, but it really wasn't too bad. As it began to get better Dunkle left and Rey moved in, and I really like Rey, he is a human being, he mostly understands people and he really appreciates people. Rey came down and insisted that I move back into supervision, I didn't want to do it but I felt like it was a genuine offer from somebody who needed the help.

And so from about 1990, or thereabouts, I was back in research for a year and a half I think, and then from '90 to '93, I was back in harness as a research supervisor. I don't remember how many people, fifteen or twenty maybe, mostly research grade scientists. I was again really getting unhappy about that assignment, and by then Tom O'Shea had come in as Rey's assistant director. And so starting in about '92 and culminating in '93, I was able to come to terms with Rey and Tom, and that we would move the collection from Fort Collins down to Albuquerque to the Museum of Southwestern Biology, and Cindy and I would move down here with it, and we began to set that in motion. The impetus for the collection move, as far as I was concerned, actually came from Buffington, and Buffington had made a comment, perhaps offhand or not in a meaningful way, but had made a comment nonetheless to Rey that we needed to move the collection out of that building where we were in Fort Collins. Even today I don't understand that, I think maybe it had more to do with Doug not understanding collections in spite of the fact that we had tried for years, because Doug was a known entity by then. Anyway, we made plans to go ahead and move. Then, of course, in 1993 came the news that Babbitt was going to start a new agency, the National Biological Survey, and I had had gotten to know Ted LaRoe pretty well by that time, and I knew what Ted had been doing. Ted had spent like the last year, year and a half of his life trying to move this thing to fruition, and one of the things that we were concerned with, my move and Cindy's were not the only

moves that were being contemplated, we were going to Bruce Bury to Corvallis, and a couple of other things were pending there. We all got very concerned about whether we were going to be able to do these moves if we waited until NBS got online, and so we managed to get the travel papers cut and everything for a bunch of PCSs by about the end of August I think. We got them all signed, sealed and everything, and Cindy and I made our first house hunting trip I think in September then of that year, and then Cindy and I ended up moving down here in November of '93. By then, NBS was if not a real thing, almost a real thing, but the bottom line was we all managed to get moved and get ensconced in new quarters before they knew what had happened quite frankly.

NBS is in many respects a story worthy of its own time; I think the general attitude on the part of most of the people in the center was one of optimism. I didn't go this meeting but many people did go to this meeting, and it was a meeting that Babbitt held down in Denver, and he had been selected by Clinton as Secretary, and Ted LaRoe was spending a lot of time with him and with a group of other people to try to implement the NBS. I remember Babbitt coming down to Denver and having a big meeting in the regional office there, you were probably at that meeting, and he said, "We are turning the lights back on in the Department of Interior" or something like that. I think most of us viewed NBS as a favorable idea. I think in retrospect it was not well implemented, and I'm tempted to say that, and there are others closer to the situation then me, goodness knows, but this was at a time when Clinton was having trouble with his attorney general not paying social security for her nannies and so forth, and it was as though a lot of these people simply couldn't implement anything. From my standpoint, one of the things that I think really got in Babbitt's way was he was having a fight with Congress over what the grazing fees were going to be in the west, and he goes off and he has this big battle with Congress over this issues, and then tries to start a new agency. It was just not well implemented. In retrospect, I think there were other things about that idea that turned out not to be so good. From my standpoint, one of those was hiring an academic from outside, Ron Pulliam, to come in and run the agency, even though Gene Hester, who spent his whole life in FWS and in FWS research, Gene was his deputy, Pulliam had strong, strong ideas, and Pulliam brought in a lot of people, most of them at least from academia. It wasn't long before we started hearing more of this research money that had been going to the centers, maybe it should be going out the door as grants to academic researchers. So that was a concern, and I think made a lot of us worried about what the future was going to bring. From that point, things just went downhill, and that's all well documented. I remember a time, I think it was in 1996, the winter of '95-'96 maybe, thinking I might just get fired, and by that time I had twenty-three years in, I spent over twenty with FWS and I had not quite three with MBS; I still have my charter plaque from MBS as a charter member; it won't even get me a cup of coffee, certainly not at Starbucks!

John:
Put me in your will, we will put that in the archives!

Mike Bogan:

There you go, we can do that! But things were really dismal at that time, and the compromise, as we all know, that was agreed to then was for MBS, the personnel of MBS to go into USGS as a new division, Biological Resources Division, and as compared to being on the street, that's a hell of a deal. I'm for that and I'd vote for it every time. There were some skeptics that appeared before very long, and they were concerned about business practices within USGS and concerned about how the financing was handled in USGS, concerned that USGS at least had the perception that they were really over heavy administratively, and I don't think it was true throughout FWS, but in the various offices that I worked out of in over twenty years with FWS, we were always administrative poor, administration poor. We had a very small, but always fortunately very dedicated group of people who would handle the personnel actions, who would handle the budget stuff, who would get the fiscal reports to you, and that service was never better exemplified then by Phyllis Jackson and her group, who were in **WELUT** when we joined them. Phyllis could be tricky to deal at times but she really knew what she was doing, and she and her staff did a wonderful job at keeping us legal and keeping us honest, letting us know when something was about to hit the fan. And Phyllis was one of the people early on who started attending meetings with USGS administrators, and she would come back and if you could get her alone or get her over a gin and tonic or something, why she would share her concerns about how things might not be all that rosy under USGS.

I left an exit interview with USGS when I retired, I doubt if anybody will read it, and I'm not sure how much of that I want to share right here tonight, but I think the biological research as currently being implemented by USGS is not being done in the spirit at which this whole program was developed and which all the people were hired, and I think increasingly we are dealing with people who don't value scientists, they don't value wildlife, natural resource scientists at all. There is a lot of talk about meeting the bottom line, about making sure we don't embarrass anybody, but not much talk about, "Boy that was a good paper you gave." "That was a great paper you published." "This was an excellent report that you provided of such and such a refuge." That's just not happening, and we increasingly seem to be supervised by people who have no research background of their own. I don't want to be snotty about it, but there was a culture under Dick Smith, I think that suggested that your very best research administrators were former research scientists, and the ones I interacted with generally proved that to be the case. There were exceptions, but they weren't very common. And now we seem to have gone very much the other way. I think there are obvious reasons for that; I think the current political situation, the war, all of these things. I wouldn't want to be president now and I wouldn't want to be secretary of interior, but it's tough times. I guess the big concern I have is that a lot of the people that interacted with over the years, and that I have helped along the way sometimes, whom I have fought with occasionally are having an increasingly difficult time in today's DOI research establishment, I think.

Unverified Spelling: Mike Merris pg 7,

Key Words: Mike Bogan, Cedar Bluff Reservoir, Baker University, Fort Hays State University, Jerry Tomanek, Gene Fleharty, Jerry Choate, mammalogy, ornithology, ecology, Tony Mollhagen, Spermophilus Mexicanis, Jim Finley, Don Wilson, Peromyscus in the Sandia Mountains, taxonomy distribution ecology, heteromyids, Jay Druecker, John Darling, David Ligon, Gary Hulett, biometry, Sigmodon, Hal Black, Stephen Durrant, wind training in prairie trees, botanical literature, Terry Yates, Southwestern Naturalist, Clyde Jones, Art Harris, Hugh Genoways, plasticine paleontologist, Myotis californicus, Myotis ciliolabrum, Lendell Cockrum, 1959 edition of Hall and Kelson, Mack Hardy, herpetology, NSF grant, Autonomous National University of Mexico (UNAM), Bernardo Villa, Ticul Alvarez, Roy McDiarmid, Loren Potter, Southwestern Bat Biologists meeting, North American Symposium on Bat Research, Smithsonian Institution, Karl Koopman, C. Hart Merriam, Vernon Bailey, Al Gardner, George Lowery, Fish and Wildlife Research Bulletin, Noatak mammal survey, Bob Fisher, mammal collection manager, Matomic Building, CITES list, marine mammal protection taxonomic list, FWS curator, Lynn Greenwalt, de la Fauna Silvestre meetings, Nayarit mammal study, Ocala National Forest, phosphate mining, St. Marks National Wildlife Refuge, Baja, California la Fauna Silvestre cooperative project, Bird and Mammal Labs, Hartley Jackson, Thomas Baskett, Denver Wildlife Research Center, National Fish and Wildlife Lab (NFWL), ecological studies, marine mammal research, manatees, sea otters, Alaskan research, animal damage control methods, Robert Brownell, Charles Stone, chief of ecology, chief of mammal pest studies, Mike Fall, research supervisor, John Oldemeyer, Fritz Knopf, Arapaho National Wildlife Refuge, Endangered Species Program, Paul Vohs, U.S. Department of Agriculture, Rey Stendell, Office of Biological Services (OBS), Ted LaRoe, Western Energy and Land Use Team (WELUT), Ralph Morgenweck, Frank Dunkle, research grade scientist, Tom O'Shea, Albuquerque Museum of Southwestern Biology, Doug Buffington, National Biological Survey (NBS), Bruce Babbitt, Bruce Bury, Corvallis, Ronald Pulliam, F. Eugene Hester, Phyllis Jackson, Richard Smith, Department of Interior (DOI), Biological Resources Division, Phyllis Jackson