Interview with

Robert E. Sloan

by Joe Cain

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A series of interviews with **Professor Robert E. Sloan** regarding his career, the recent history of paleontology, and the life of an American scientist in the second half of the twentieth century. These comprise 16 sessions, from tape recorded interviews. These transcripts are from cassette tapes now on deposit at University of Minnesota Archives, Twin Cities campus.

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These interviews were made possible with a generous Mellon postdoctoral fellowship from the Department of History of Science, University of Oklahoma.

To download the latest version of Sloan's autobiography, click here (594K Word 7 for W95, April 1996). Download a ZIP file of the 16 transcript files as *.htm documents (166K)

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Interview with
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Session 1

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#1 Tape 1, Side 1
11 March 1996

RES: Professor Robert E. Sloan, Department of Geology, University of Minnesota
JC: Dr Joe Cain, interviewer

JC: This is Joe Cain. It is 11 March, 1996. I'm here in Bob Sloan's house in Winona, Minnesota. This is Tape 1, Side 1 of a series of tapes that we are going to be making this week. We've just finished watching videotapes that Bob and his students put together in the late 1980's, and we are going to talk a little bit about the K/T [Cretaceous/Tertiary] extinction. Bob is going to explain something about the Pele hypothesis. Go ahead, Bob.

RES: The Pele hypothesis began with work by Robert Berner of Yale who went to work on the mass balance of carbon dioxide, oxygen and coal in the geologic record over the entire Phanerozoic Eon. And Bob found out that the cycle time for carbon dioxide in the atmosphere is only about a quarter of a million years. So there is a possibility of very rapid changes. He made intensive analyses of how much coal and limestone was deposited at any given time, meaning how much carbon dioxide was fixed, either as sugar or as calcite. He also looked at what this meant for the amount of oxygen in the atmosphere, and when you have intervals of time such as the Pennsylvanian or the Cretaceous, with very large amounts of coal and oil, for every mole of carbon fixed in the form of coal, oil or limestone, you also release a mole of oxygen into the atmosphere. So by going through the geochemical data on the amount of coal in various parts of the geologic column, he was able to come up with a curve showing primarily the oxygen content of the atmosphere. And this has varied a great deal over a period of time according to his calculations. The source is of course carbon dioxide released from the metal by sea floor spreading and super plumes. The sinks for carbon or coal, and to a lesser extent, limestone, any carbon dioxide that is fixed by green plant photosynthesis or even bacterial photosynthesis, produces a mole of oxygen. So knowing something about the rate of sea floor spreading, you can predict what carbon dioxide might be.

Looking at the amounts of fixed carbon in the geologic record, you can also figure out what the oxygen level was at any given time. The sink for oxygen is usually weathering of rocks. And it results, among other things, in the conversion of the common ferrous iron, (which is the normal iron present in most igneous rocks) into ferric iron, which is of course, at the 3+ instead of 2+ and also simply the breakdown of ordinary igneous silicate minerals, such as feldspars and [3 words inaudible] into clay minerals, all of which remove oxygen from around the surrounding environment. And by looking at these things, he came up with a curve in the amount of oxygen with time. He wanted to check this because it seems that the normal view that the oxygen has forever been at where it is right now, at 21 percent of the atmosphere, didn't seem reasonable at all, particularly in view of what [Preston] Cloud had done in the way of Precambrian oxygen concentration. So he did these calculations, and then he looked up an isotope geochemist to see if there were some way that they could literally measure the oxygen concentration at various times in the past. They got some samples of amber from the late Cretaceous, essentially Judithian about 75,000,000 years ago from Saskatchewan and looked at the bubbles of gas inside the amber.

There had been a general feeling that amber was too reactive, that this couldn't possibly be air and yet when they took these pieces of amber and crushed them in a vacuum line in screw vice slowly so that they literally could then, as each molecule of gas or blob of gas leaked out of the amber, they would run it through a mass spectrometer and literally measure what the mass distribution was. And you would easily pick the points for nitrogen and the points for oxygen and argon and a few other things, and they found to their very great surprise that they got about 35 percent oxygen in the bubbles, in the amber.

Well, it would be relatively easy so everyone thought, to reduce the amount of oxygen in amber by reaction with the resin of the amber. But how would you increase it? Or where would the oxygen come from? So they published a preliminary paper, and of course, they were jumped on. Everyone said that amber was too reactive, couldn't possibly work. So they did a whole series of analyses. They kept one piece of amber, at 10E-19 torr, which is about the same vacuum as in outer space, for three years, and nothing changed. And they put some more amber in a reactor and made argon 39 and then checked to see what the argon 40/39 ratio might be in the gas that came out of the bubbles and surprise, they got Cretaceous values of the argon 40. So the net result was after a lot of effort, Gary Landis, the geochemist who was then working for the USGS of Denver, came up with a couple of papers in which he refuted all the arguments about this.

In the meantime, it was interesting to have this one datum point that 75,000,000 years ago there was roughly 35 percent oxygen. What we needed was a lot of data points. And so in 1986 or 1987, one of the things that my class did on our annual Bug Creek camping trip was to literally mine coal beds for amber. And we started with the coals, beginning with the Nul coal just 60 feet under Bug Creek Anthills, and worked all the way up to the highest coals in McCone County.

JC: Did he call and ask you to do that?

RES: No. I suggested it would be an appropriate thing to do.

And it was and he started running these things. He was also getting amber from many other places. He had Baltic amber. He had modern Kauri resin from New Zealand. He managed to get hold of a couple of samples of early Cretaceous amber which showed significantly lower amounts of oxygen than the other. He was trying to wonder, where did all the oxygen come from? What he hadn't seen was a paper that appeared in the *Journal of Geology*, by Roger Larson, who is a major oceanographer, pointing out that there were major super plumes in the Southwest Pacific starting at about 120,000,000 years ago and that there were many consequences of these. The first and most obvious consequence that Larson pointed out was that this must have come from deep in the mantle, because these super plumes literally drew enough heat off the mantle so that for the next 30 million years, there were no magnetic reversals in the world. And the interval of high super plume activity and high rates of sea floors spreading exactly coincided with the Cretaceous Long Normal magnetozone, 30 million years long. And he also looked at the actual overall global rates of sea floor spreading and showed that starting at 120 million years ago, the rate of sea floor spreading doubled globally in about 5 million years. Which from our point of view, automatically means there's a doubling of the rate of production of carbon dioxide.

This suddenly explains a lot of the really weird things about the Cretaceous. The Cretaceous is a green house period with very warm climates from pole to pole. Dinosaurs from Tasmania, which was part of Antarctica at the time, all the way to Point Barrow, Alaska. There are other things it does. When you have high amounts of carbon dioxide in an atmosphere up to a certain limit, which is considerably higher than it is now, the result is green plants grow very much better. For example, there was a kid in northeast Minneapolis who had grow bulbs in his mother's basement, essentially carbon dioxide tents over a whole series of plant tables and was growing a magnificent crop of marijuana in the basement at very high carbon dioxide concentrations (until someone turned him in!). The effect of this high carbon dioxide would be not only to increase global temperature, by the normal greenhouse effect, but also greatly increase the rate of plant photosynthesis globally, and hence, at exactly the same time, to greatly increase the oxygen concentration. Well, Berner and Landis hadn't realized that the time of the oxygen peak was precisely associated with the time of the super plumes in the South Pacific, that basically produced the whole shallow area of the South Pacific that has the big reef archipelagos (whose initial explanation was by Darwin shortly after the *Voyage of the Beagle*).

JC: Right. And you put that together.

RES: I put that together. And since this is carbon dioxide coming out of volcanoes, whether surface or submarine, it doesn't make any difference, I suggested that an appropriate name for this would be the *Pele Hypothesis*, since Pele is the Polynesian volcano goddess, to whom fair virgins were sacrificed. And so Pele Hypothesis became one of the subsidiary effects with the increase in the rate of photosynthesis, there is a corresponding emphasis in standing crop of both marine Plankton (algae) and land plants. Further, this is precisely the time when the angiosperms explode and globally replace conifers and cycads as the major vegetation, essentially pushing them off into marginal habitats, where they remain today. That is the secondary effect, or maybe it is tertiary at this point. The Quarternary effect is this high rate of production of both groups of plants means that there is corresponding high production of animals.

One of the things that has always puzzled me, and makes the Cretaceous truly bizarre, is that in the Cretaceous the taxonomic diversity of marine organisms is over 10 times that of the Jurassic. There are major radiations in snails, in clams, in brachiopods, in sponges, in corals, in crustacea, and in fish. Everything radiates. This is not something I have ever seen anybody specifically mention, but if you simply look at a compendium, like Moore, Lalicker and Fischer or *Index Fossils*, and simply count pages of major fossils belonging to certain periods, the Cretaceous is 10 times as diverse in marine organisms as is the Jurassic. So this was an explanation for that.

And it is precisely at this time that the recovery from the first dinosaur extinction takes place. When the super plumes come and carbon dioxide increases, and the oxygen correspondingly increases as a result of photosynthesis. Nobody has tried to explain the first dinosaur extinction. There are 25 genera of dinosaurs in North America in the Morrison formation in the latest Jurassic just before the Cretaceous. By the time you get well into the early Cretaceous, that is down to 9. That is a major extinction by any point of view, and furthermore, there is a distinct reduction in size. The biggest dinosaur in the Albian is essentially a *Tenontosaurus*, which is a 20-foot long duckbill relative, an herbivore and the biggest carnivore by that time is *Deinonychus*, about 8 feet long and weighing about as much as a big timber wolf. And all the big carnosaurs of the Jurassic are gone. And the earliest Cretaceous, like Stovall's things, I really want to know what Stovall has and what formations they come out of for this reason. But then this is globally. The early Cretaceous dinosaurs are not taxonomically very diverse. Although you are starting to get regional differentiation because of the breakup of this Pangean supercontinent. Well, the net result is that in the late Cretaceous, starting at about 100 million years ago, the taxonomic diversity of dinosaurs becomes very high and so does the oxygen concentration. And yet the super plumes did not last forever and they started to die at the end of Cretaceous.

We already have dropped from the peak values by the time we get to the Null coal just under Bug Creek. And by the time you get to the lower part of the Z coal below the iridium clay layer, you are down to about 27 percent. Then you have the big Deccan Traps in India which are a tremendous, big outpouring of lava, most of it in a couple of million years, and things jump up again to 28 and then slide from there. So one of the things that was becoming very much of a problem for dinosaurs was the oxygen concentration of the atmosphere at the end of the Cretaceous. The other things were going on. The temperature was dropping. The climatic diversity was increasing but on top of all these other things, we now have the collapse in oxygen values.

My colleague, Richard Hengst, from Purdue, went into the physiology of dinosaurs. He took a very, very good look at the skeleton of *Apatasaurus* or "*Brontosaurus*" in the Field Museum, was able to calculate lung volume, because the ribs over the lungs are of slightly different shape than those over the abdominal region. And by looking at the way the ribs articulated, the maximum amount of volume change of the lungs per breath could only be had by the ribs rotating forward and aft. They could not belly out. This means that the maximum oxygen air exchange C02...

JC: We are going to take a break because the doorbell just rang. [Pause in the tape] We are back. It is Tuesday morning, the 12th about 11 o'clock, and we are going to pick up our conversation where we left off yesterday afternoon, Bob talking about the Pele Hypothesis. Go ahead.

RES: As I was talking about before, Richard Hengst of Purdue is a physiologist who came into the project, the project now consisted of Gary Landis, Lawrence Snee, who was one of his principal technicians. They were doing the isotope work. Keith Rigby, who is my partner in crime in matters Bug Creek these days, and I, who collected samples and provided stratigraphy, I also provided ideas, and Richard Hengst, who is the physiologist. Rich Hengst worked on the anatomy of *Apatasaurus* at the Field Museum and showed that as a result of the volume of the lungs and the total amount of volume change that could take place in normal breathing, the maximum air exchange per breath would be about 1/40 of the lung volume. In contrast, in birds, the whole sternum and the ventral ribs move in and out with flight so that the whole atmospheric circulation in birds is such that there is a complete exchange of all the air in the lungs about every 7 breaths. The same is of course true, in mammals and mammal-like reptiles back to the first, the only survivor among carnivorous therapsids, at the Permo-Triassic extinction, *Thrinaxadon*. *Thrinaxadon* is the first mammal-like mammal to have a diaphragm as well as a fully developed secondary palate. I don't think it is at all accidental that a reason for the rapid turnover and rapid rates of evolution among mammal-like reptiles in the last 8 million years of the Permian, is that this is a time when Bob Berner has suggested that the oxygen level in the atmosphere dropped to as low as 15 percent.

JC: One of the nice things about the stuff that you are working on now is that it is integrating a lot of materials. It is bringing together a lot of independent observations.

RES: It sure is. Integration is something that I have worked on throughout most of my career. One of the things that has disturbed me most about my paleontological colleagues is that they very seldom look beyond the immediate consequences of a particular problem. Whereas I tend to look at chains of circumstances in case of Pele, we are working on [a combination of] circumstances that involve as many as six sequential steps, and yet are quite fully documented at each step all along the way. So dinosaur diversification and extinction in the Cretaceous, the major changes in the origin of [open-air] breathing are essentially correlated to the composition of the atmosphere which is ultimately correlated to motions within the mantle. In other words, there is a direct tie-in between evolution and Mantle and Plate Tectonics.

In any event, large dinosaurs really required to be living in an oxygen tent. An atmosphere in the neighborhood of 35 percent oxygen would be considerably more compatible with large dinosaurs than one in the neighborhood of 28. And so this suggested to me that this was perhaps a significant reason for the first dinosaur extinction, and probably one of the major factors in the second, the terminal dinosaur extinction, other than the birds. It also neatly tied together all of the really bizarre features about the Cretaceous. The Cretaceous was a very bizarre period in geological time. It is the time when there is more chalk being deposited than at any other time in the geologic record including today. It is the time when angiosperms undergo a tremendous radiation. A radiation which has been a major problem in evolutionary studies since Darwin's day. I recall reading somewhere that Darwin wrote to Hooker about the abominable mystery of the origin of the angiosperm.

Well, here may very well be one of the significant reasons. The Cretaceous is clearly a green house period as opposed to the present ice house that we have. The Cretaceous is a time of tremendous biological diversity. It is usually said among ecologists today that it is not possible to have more than seven trophic levels in a trophic pyramid. I would be willing to bet, looking at how I have seen ecosystems in the Cretaceous, that there were at least eight and maybe nine trophic levels possible, which would require very much higher temperatures and a much richer food supply than is available at the moment. Well, the rich carbon dioxide of course provides for a much greater biogenic diversity. There is a tremendous radiation of angiosperms and most other organisms in the Cretaceous. And angiosperms like everything else, are directly determined by carbon dioxide concentration in the atmosphere.

And when you double the rate of sea floor spreading, the carbon dioxide concentration goes up. This went up so much that 72 percent of the world's petroleum supply comes from Cretaceous rocks. There is the biological diversity. And this biodiversity led to a complete remodeling of marine ecosystems. I spoke earlier of the 10 times greater diversity at any one time of mollusks, brachiopods, crustaceans, all major marine invertebrate groups, as compared to the Jurassic that came before. But it is more than that. This is also the time when the big radiation of [one word inaudible] fishes took place. And if you look at the number of trophic levels in the Cretaceous, we see the phytoplankton radiation, we see the phytoplanktonic foraminifera, which represents the next trophic level. We see copepods which appear at just this time and not earlier. The result of some planktonic larva of a benthic crustacean like a lobster or crab, we have no idea which one, but some decapod, became sexually mature, while still a larva which gave rise to a whole new subclass of the class of crustacea, the Copepoda. Above that, we have essentially sardine-size fish. We have herring-size fish. We have mackerel-size fish. We have tarpon-size fish, and we have tarpons that are three times as large as the largest living tarpon, all radiating and diversifying into every possible teleost mode of life. Well, if you have sardines, and herrings and mackerel and tarpon and supertarpons, that is 5 trophic levels, and into the three you had before, and you would still have room at the top for the shark radiation. That is 8 or 9 trophic levels, which cannot be supported today.

JC: Do you think it is unusual for paleontologists to see end-Cretaceous as an ecological problem?

RES: Yes. I am quite sure of it. It is unusual for paleontologists to really work out whole ecosystems. They generally look, even the very best of them, at only one or two trophic levels. And part of this is the strategy for study of paleontology that I developed very early on from Lowenstam and Olson. And this was always reconstructing the food chain. And it is not something most people do. Most people just say, "Oh, there is a food chain" and don't attempt to reconstruct what there is. Well, in the Cretaceous maritime community, you have 8 or 9 trophic levels that we know about and that is considerably greater than the seven trophic levels we have today.

JC: Right.

RES: And that requires a greater food base which can't be more insolation, but the additive effect of super-abundant carbon dioxide would certainly have this effect. One of the problems that people have always suggested about these high levels of oxygen at various times in the past, is that this is comparable to what you have in an oxygen tent in a hospital. And what about wildfires? What they forget is that the reason for this high oxygen is that there is also a high carbon dioxide level. We are talking about carbon dioxide levels 6 to 10 times the present carbon dioxide level. And that is more than enough to essentially combat wildfires.

JC: So there is a balance.

RES: So there is a balance. Hengst and Rigby have done some analyses of what might go on in such atmospheres. And you can't burn plant trash in great quantities because it essentially is self-limiting. So there doesn't seem to be a problem with critters living at this. There are other consequences of this. When you have...

[end of Tape 1 side 1]

[Tape 1, Side 2 is inaudible]

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Interview with

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Session 2

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#2 Tape 2--Side 1.

March 12, 1996.

RES: Professor Robert E. Sloan, Department of Geology, University of Minnesota.
JC: Dr Joe Cain, interviewer.

JC: We have been talking about Moore, Lalicker and Fischer's book on invertebrate paleontology and the *Treatise of Invertebrate Paleontology*.

RES: The Blue Ribbon Committee then essentially set to work to try and reorganize classification at the family level. There were some volumes that were produced very quickly. For example, Ray Bassler did a quick and dirty job on the Bryozoa and it was fine as an introduction but it did not go far enough. Other groups took longer because they took the job much more seriously and undertook original research to see just what the pattern of familial and ordinal level evolution really was in the record and they did this very well. The result is the roughly 50 volume series of the *Treatise of Invertebrate Paleontology*.

JC: In the fifties, when Moore was putting this project together, what was the sense that you had of the project as an outsider: knowing that this project was under way, knowing that it was coming? Did it have any affect on what you were doing or on what you were anticipating?.

RES: Just that as the *Treatise* volumes came out, I read them and reorganized my paleontology lectures around the *Treatise* because in effect the best single textbook in invertebrate paleontology today is the *Treatise of Invertebrate Paleontology*. At $600 for a complete set (you got a special discount price if you bought them all) it is a little too much for most students to buy but it is in fact something students ought to buy and read thoroughly. And so one of the things that I did was make certain that there was a complete set of the *Treatise* in the paleontology laboratory [in the Geology

Department at the University of Minnesota] to be used in the normal part of paleontology exercises. Some students use it, some don't. That is the normal sort of thing but they are there to be used.

JC: Right. Because you had originally used Moore, Lalicker, and Fisher, right?.

RES: I used Moore and then kept reading the literature. There was, for example, a very useful symposium volume published by the Museum of Comparative Zoology in which all the various *Treatise* editors talked about the new things that were coming up in their *Treatise* volumes. This was not something that most paleontologists bothered to read but in effect I got preliminary notices from this, of the direction the *Treatise* classification was going, then could revise my lectures and how I treated Moore, Lalicker and Fischer as a textbook from this. So routinely my courses in paleontology were always in advance of the publication level.

JC: You would use Moore as a base?

RES: Moore was a base. Moore was a tremendous library of illustrations. Moore did all of his illustrations. He was an excellent artist, and he could draw quick pen and ink drawings that were very satisfactory depictions of fossils. You could see what the fossil looked like from his drawings, despite the fact that they were the easiest sorts of illustrations to look at and decipher. His morphology was good. His paleoecology was not bad, it was not complete by any manner of means. In effect the paleoecology part of my lectures essentially came from the two volume *Memoirs* 67 of the Geological Society of America, the treatise of marine ecology and paleoecology, which was produced by Walter Bucher.

Bucher was a structural geologist but foresaw which sorts of changes would prove most useful and ram-rodded a committee of the National Research Council to produce this treatise on marine ecology and paleoecology thinking it would be a good thing. Well it was a very good thing and then Derek Ager around 1970 came up with his useful elements of paleoecology or principals or whatever it is. Other than the treatise of marine ecology the ecological text that proved most useful in my own training and in developing my thinking beyond Olson and Lowenstam was a very unusual introductory ecology text by Odum, *General Ecology*. Odum's first edition was a spectacular improvement over the kinds of ecological textbooks available because Odum went specifically into energy flow and its consequences in ecosystems rather than talking about cute kinds of interspecific relationships, which is the sort of thing that Emerson sort of stressed in the book commonly known as The "Great APPES" after the initials of its authors Allee, Parks, Parks, Emerson and Schmidt.

JC: What sort of information did Emerson provide?.

RES: He gave you lots of examples and no principles.

JC: I see.

RES: On the other hand with Odum, for the first time in any ecology book, you have principles of ecological organization based on food chains and interspecific competition.

JC: And that gave you a conceptual architecture?

RES: That gave me a conceptual architecture which now had a theoretical foundation beyond what Lowenstam and Olson had given me.

JC: So while you've said to me many times before that your paleoecology stemmed out of the Lowenstam-Olson group in Chicago, there is clearly another stage. When Odum's book appeared?

RES: [Nodding] There was this other stage: Odum.

JC: Odum comes out later [after your time in Chicago and] slightly later there was an expansion in your work.

RES: Yes it really did [expand] and I have had several major threads in my career as a paleontologist. .

The first one was basically the equivalent of my master's thesis and my Ph.D. thesis research. The master's thesis problem was not treated as such but it in effect was a problem that Marvin Weller gave me. I asked him for a potential research problem and he told me that I ought to look at the problem of *Glabrocingulum* and *Worthenia* because all the figures and most of the specimens referred to *Glabrocingulum grayvillensis* were slightly different from the actual types. Weller knew precisely where the type locality near Grayville was, he had been there, one of the few people who had collected there and he had me go down to Grayville which was about 20 miles away from my grandfather's home in Albein and in fact was the place where my great grandfather had mined coal. I collected topotype material of this species, *Glabrocingulum grayvillensis* Norwood and Pratten 1847.

Very few people had seen these things and having collected the topotype material I found yes indeed it was different from most of the specimens that had been referred to the species under it several new names and while working on that I also worked with a variety of other Pennsylvanian snails. I had one problem in which I had a limestone with a silicified tiny snails from a fraction of a millimeter up to a quarter of an inch and I dissolved this thing, identified the larger ones and determined what the age distribution by size of shell was in acid insoluble residues of this chunk of limestone Pennsylvanian in age. I got the types of the genus *Glabrocingulum* from a Scottish museum through Weller, and they had been very poorly illustrated. It is not a very big snail. It is about 7 or 8 mm. in maximum dimension and that was the size of the original illustrations. J. Brooks Knight who was the premier student of Paleozoic snails, had redone the snails for the 1944 revision of *Index Fossils of North America* in which he completely reorganized snails for the first time in years. It was essentially a *Treatise* treatment very early, and he had transferred this species to the genus *Glabrocingulum* based on this Scottish species. Well I photographed lots of *Glabrocingulum* from many parts of the world. I found literature, references, took [inaudible] in China, had Russian citations translated. They were always Pennsylvanian or early Permian age. I photographed the type specimens, very large size.

One of the other things that I learned to do was in fact be a photographer. I had played around with photography and read everything about macro photography. So in this case I used a very long focal length lens. It was an 8 inch focal length projector lens. I mounted it on a 3 foot mailing tube which I put in the front of a double extension bellows view camera with a reducing back to 3 1/4" by 4" and a 1/4 sheet film and wound up with a distance between the lens and the film of something like 5 feet which meant that I had enlargements on the negative of about 4 times and since it was a long focal length lens, essentially the perspective problems were greatly reduced. It was nearly orthographic and I published some of these illustrations in *Bulletin of the Field Museum of Natural History* and the others were used to provide the illustration and the *Treatise* volume on snails.

JC: One of the other things that you did in that work is study variation of a highly variable genus, and one of the things that you invented was this piece [of instrumentation].

RES: Oh yes. My first publication in July of 1951, the new instrument for measuring fossils.

JC: You should give the citation.

RES: *Journal of Paleontology*, Volume 25, page 525 and 526.

JC: Tell me about this illustration on figure 1.

RES: This illustration shows an instrument that I built from a junk petrographic microscope at the University of Chicago. One of the things about most snails is that they follow an equiangular or logarithmic spiral about a single axis of rotation and it is a difficult job to measure all of the various aspects of morphology of the snail so I essentially used the rotating table with degree scales of the petrographic microscope that was the base of the petroscope, to this added a couple of drill rod stems and a sliding block of brass machined by Bill Schmitt who was the machinist of the University of Chicago.

(Bill wound up training most of the geology students who needed to build pieces of equipment how to do such things.) I had two small metric micrometers set up one to essentially measure the radial distance from the access of rotation of the symmetry of the snail and the other one a vertical distance and it worked rather well. Basically in line with the axis of rotation of the petrographic stage I put a pin. The tip of the pin was the zero point for both the radial measurement and the vertical measurements. You mounted the base of the snail on the pin like impaling one of Vlad the impalers victims on a stake, held it in position with a little modeling clay. Made sure it was exactly in position my making sure the pointer and the horizontal pin was right at the protocone and then you could literally measure radial distances and construct anything you wanted in the way of a rho, Z and X coordinate system where rho is the angle of rotation and Z is the vertical position of any given element and X the horizontal distance. This is much more useful than a Cartesian coordinate system and it made possible the easy measurement of how frequently growth lines or ornamentation, what trilobite specialists call the prosopon and everybody calls just an ornament.

JC: And it is a quantitative assessment.

RES: It is a quantitative assessment of the detailed morphology in terms of the actual growth mode of the snail. And so I wrote it up and submitted it to the *Journal of Paleontology* and it was my first publication.

JC: And the point of making the apparatus in the first place was?.

RES: Is to express the quantitative measures of snail morphology in terms of the actual logarithmic spiral. The spiral that snail shells are built around. It could be used for other things as well because there are a very large number of groups of organisms that have logarithmic spiral shells. This technique could be used for any of those. I used it for snails.

JC: And getting quantitative measures allows you to get a handle on the variability of different populations?

RES: Highly variable snails. Most people working in snails simply measure the height, the diameter, maximum diameter and the apical angle or some other angle. Almost nobody measures the spiral angle of the expansion of the logarithmic or equiangular spiral. It also made it possible for me to well basically get a handle on some more quantitative variables. While I was doing this, Weller went off to the Philippines, and Ralph Gordon Johnson (who later became the head of the Department of Geology at the University of Chicago) and I were the principal invertebrate paleontologists around. Ralph had started as a vertebrate paleontologist but switched to invertebrates. I was always an invertebrate [paleontologist] with an interest in vertebrate paleontology but ultimately switched the other way around.

We were given the chore of teaching the introductory paleontology course in the fall quarter of the last year I was there, and when the fall quarter ended one of those students, David Raup, wanted to continue so I gave David Raup his advanced invertebrate paleontology which was essentially a summary of some of the things that Marvin Weller had done but I also included a number of things that I had dabbled in. For example the equiangular spiral growth of mollusc shells met and some speculations on some things that Francis Pettijohn had talked about in sedimentary petrology the fact that any kind of echinoderm, every single plate of an echinoderm, was a single crystal of calcite oriented in a different optical direction than any of the others. This was new material that I had generated. It was not available anywhere in the literature. I gave it to Dave Raup and surprise, his Ph.D. thesis was on the orientation of calcite crystals in echinoids. His first major study after that was the elaboration of the equiangular spiral. Raup, of course, went on to be a member of the National Academy of Sciences and also was for a while the head of the Department of Geology or Geophysical Sciences at the University of Chicago.

So Dave Raup was my first graduate student.

JC: While you were a graduate student? A senior graduate student?

RES: I had a masters degree, and I was the sort of person who would have been hired as an instructor.

[Pause in the tape].

JC: We are back, it is a little after 2:00 on the 12th and we are going to pick up where we left off basically in talking about the *Treatise of Invertebrate Paleontology*.

RES: The *Treatise of Invertebrate Paleontology*, after Ray Moore got it started, turned out to be a major research project all of its own that completely changed the character of classification of invertebrates and phylogeny of invertebrates. The typical zoological classification routinely taught in textbooks, zoological textbooks and to a lesser extent in paleo textbooks to this point had been heavily influenced by experimental embryologists following Haeckel in believing that ontogeny recapitulated phylogeny despite all of the arguments by DeBeer and many others that there were many alternatives to the biogenetic law. This was not the be all and end all of evolution and one should not depend solely on the evolution of embryos to tell you something about the evolution of the groups to which they became.

JC: So how did the *Treatise of Invertebrate Paleontology* develop?

RES: It started out with a few volumes in which the senior specialist in a field, usually a fairly small field would summarize everything he knew about that group and so Ray Bassler of the U.S. National Museum wrote the first Bryozoan volume of the *Treatise* which turned out to be about 3/4 of an inch thick, relatively thin volume listing all the orders and families and all the genera but not providing anything new that was not available to a diligent library researcher. Another example of the same thing was volume P, Part II, which was the Chelicerata. This was done by Alexander Petrokavich who was a specialist in spiders both living and fossil and he did all the rest of the Chelicerates as well and it was again simply a summary of everything Petrokavich knew about Chelicerates with very little other than minor library research to make certain he covered all the papers. There was, of course, a biography of all the papers consulted in the course of construction of the thing and there were a set of illustrations usually created specifically for the volume, pen and ink drawings, some photographs, not many to illustrate the classification and so the first few volumes were of this sort and they were useful but not nearly as useful as they became.

JC: So the first couple of volumes are much more like compilations?

RES: They are compilations. They are strictly compilations by a world renowned specialist in the subject with all of his (and they were all men) idiosyncrasies and some of them were pretty idiosyncratic. However in other, more important groups of animals the situation rapidly got out of hand. It was clear that paleontology for each class or order being talked about in a *Treatise* volume had essentially grown like Topsy. Wherever people worked that was where we knew something and there were big gaps of information mainly because no one had paid a great deal of attention and when some of the (I think) more responsible senior editors of the *Treatise* board found this situation they started major research programs to fill in the gaps. They had graduate students who would work on specially made collections and they would review all of the museum collections that had been made but not been described and things of this sort and eventually would come up with a much more complete summary.

The character of the *Treatise* volumes followed very much the pattern that the book Moore, Lalicker and Fischer followed. There was a description of the anatomy, of modern members of the group as well as unique features of the fossils, a well illustrated glossary of all the morphological terms that had ever been applied and where there were many terms to the same feature, there was an attempt made to regularize things and there was the stratigraphic range chart usually done at the level of a third of a period. And they were organized phyletically. There was never any discussion in the second set of volumes of the *Treatise* about who beget whom. That came still later but the *Treatise* rapidly evolved into a major research project with major committees with a chief editor for a particular volume and a set of people assigned particular parts. Some of these were very good, some of them were brought out and then later it was found that there was still more material to be had and so in some cases, as in corals, we are now in the third revision of a *Treatise* volume. Volume O, on trilobites, is expanding from one thick volume, to 3 volumes, one of which will appear momentarily and instead of a single editor in chief there will be a group of editors.

JC: When do you think the *Treatise* changed from being something basic to being something much more sophisticated?.

RES: Basically around 1959, 60.

JC: Why do you think that happened?

RES: Simply because some of the editors in large groups rather than small groups like Graptolites or Bryozoa saw what was coming and realized there were these great gaping holes that needed to be filled. There were whole major families and orders to be described. There were particular transitional sequences of fossils that needed to be looked at. The major transitions and radiations following an extinction needed to be looked at in detail. One of the early ones of this sort was the Trilobite volume and essentially they threw up their hands in horror because the trilobite extinction at the end of the Cambrian is perhaps the 98 percent level and so there were trilobite specialists who dealt only in Cambrian species and then there were trilobite specialists in the Ordovician and later and at that point there was no way for the two to talk to each other over. That came out in 1960.

Over the next 25 years people found the Cambrian ancestors of the distinctively different Ordovician orders and were able to sort out who beget who and where geographically the ancestors were because before they radiated into the broad tropic warm seas into the Ordovician forms and then ultimately a proper phylogenetic analysis of the trilobites was well begun.

[End of Side 2 tape 1].

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Interview with

Robert E. Sloan

by Joe Cain

Session 3

A series of interviews with Professor Robert E. Sloan regarding his career, the recent history of paleontology, and the life of an American scientist in the second half of the twentieth century.

These interviews were made possible with a generous Mellon postdoctoral fellowship from the Department of History of Science, University of Oklahoma.

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#3 Tape 2, Side 2
March 12, 1996

RES: Professor Robert E. Sloan, Department of Geology, University of Minnesota
JC: Dr Joe Cain, interviewer

JC: So what effect did having a treatise in a particular group have on researchers, both specialists in the area and people working in other areas.

RES: It set a new target for a standard of paleontological research. It meant that if you were really going to be a thorough going paleontologist rather than a hack instructor at some podunk college you had to consider all of the various aspects of paleontology and in particular you had to consider classification . And the first one of the major groups for which this was truly successful was the Ammonoid volume and here again there was the usual problem, there were the Paleozoic Ammonoid specialists, the Triassic Ammonoid specialists, the Jurassic ones and the Cretaceous ones, one for each radiation. But they finally got together and started talking to each other and developed the continuity across the several extinctions in initial radiation intervals and came up with a pretty fair package.

This was so exciting to them that they had a couple of other volumes on Ammonoids that were not treatise volumes but essentially were revisions of the treatise and finally of course there will now be about three new volumes of Ammonoids in the treatise but in the meantime we had the benefits of the treatise volume with its comprehensive picture, its detailed phyletic charts with definite implementation of ancestral-descendant relationships and so phylogeny became a much more patterned and organized and structured way of doing paleontology rather than the gross outlines and broad generalizations that had been made before.

Some of the volumes of course never came off. We are totally missing the Coleoid cephalopods particularly because one senior author (J. Jeletsky) wrote a couple of papers of the initial planning and accumulated some manuscripts of parts but never did any thing more. He is dead. And similarly for the gastropods. J. Brooks Knight way back in 1936 (or maybe it was 39) in a publication Paleozoic Gastropod Genotypes photographed the type specimens of every single genus of Paleozoic gastropods. It was a monumental piece of work. He did it just before he did the Index Fossil snail part and he of course together with his students had done a bang up job in reviewing the entire literature of Paleozoic snails and getting it all in order so it was ready, but little or nothing had been done about the Mesozoic or Cenozoic snails. Originally there had been planned to be one snail volume and Brooks prevailed on the rest of the treatise committee to get the Paleozoic snail volume out now including Paleozoic members of advanced snail groups but excluding the Mesozoic and Cenozoic members of primitive snail groups while he was still living and it was a good thing. We still don't have Mesozoic and Cenozoic snails because the premier author Norman Sohl of the U.S. Geological Survey worked with Mesozoic-Cenozoic snails all his life for the U.S.G.S. Never had enough time from the U.S.G.S. to do anything other than plot an outline. There is one little short paper in which he outlines a classification of Mesozoic Cenozoic snails and that is all there is and it is only at the family level without saying what is in the family.

And so that is a very unsatisfactory situation and I am afraid it will continue to be. It is just a very big chore and with the, ..., well, I can only think of two North Americans who could tackle the job of this at the moment, David Jablonski of Chicago and oh can't think of his name from the east coast who had been working in Cretaceous snails and has shown that there is a lot of synonyms between European Cretaceous snails and Gulf Atlantic Coast Cretaceous snails.

JC: The treatise is an evolving set of documents.

RES: The treatise is definitely an evolving set of documents and the cycle. It is a joint publication of the University of Kansas and the Geological Society of America.

And the Chief Editorship of the treatise has changed a few times. It grows in scope every so often. I talked about the problem of the biological classification being based on comparative embryology. The treatise evolved into the major phyletic analysis of invertebrate animals and it is only recently that there is an additional method in terms of sequencing proteins and nucleic acids as a way of doing the same thing but for years even though the treatise volume had completely reorganized the classification of a major class, basic zoology texts went on with the standard 1920s kind of classification that was post Haeckel. Even the great biology textbook *Life* that Simpson wrote did not take full advantage of the tremendous phyletic work that was done on the treatise.

JC: Right they didn't have an invertebrate paleontologist.

RES: They didn't have an invertebrate paleontologist and they did not even look at it. And this leads to real idiocies such as a common name for the existing cephalopod subclass being the Dibranchiata (two Gills). Two gill branches as opposed to the Pearly Nautilus which has four (Tetrabranchiata) and everyone in zoology put all the shelled cephalopods in with the Tetrabranchiata . Well it turns out that 90% of all the fossil shelled cephalopods in fact have two gills just like squids, octopus and cuttle fish. The treatise uses Coleoidea for this group and not Dibranchiata but you will routinely find that the classification in invertebrate textbooks will have

Dibranchiata.

JC: That suggests that not a lot of zoologists have much experience in invertebrate paleontology.

RES: They have not done their homework because there has been very little in the way of comparative anatomy of invertebrates of any sort since Libby Hyman. Libby Henryetta Hyman was a comparative zoologist who specialized in invertebrates and produced a monumental set of 7 volumes covering most of the invertebrates but never getting into arthropods for obvious reasons, it was just too big and her life was not long enough. She summarized everything in sight, she did not do much in classification although she certainly summarized everything that was known. It is a magnificent compendium . It is equal to the treatise in its scope but it is missing the complete phyletic scheme and there is a very interesting tale about how Libby H. Hyman financed her research. She wrote one of the most widespread comparative anatomy textbooks that has ever been used and it was a textbook of vertebrate anatomy and since medical doctors have to take comparative anatomy, she sold loads of copies and her royalties essentially paid for a lifetime of research. It was not really until Romer came out with his various versions of comparative anatomy that Libby Hyman's book was finally replaced. It was a magnificent piece of work. She was an excellent scholar and as was usual for female scholars, she was sort of stuffed off in never land. She never did get married. She concentrated on her research and a family was something that women (scientists) just did not do. By staying a spinster she was able to do what she wanted to do.

JC: And had the financial means to do the research.

RES: And had the financial means to do the research. I forget what institution she was associated with, it does not matter. But the only thing comparable to Libby Hymen's work is a two volume by Belusov in Russian which was translated into English oh 15 years or so ago and supplements Libby Hyman very nicely. But these were routinely ignored by textbook writers because textbook writers simply add new material to the old textbook material without really revising the old textbook.

JC: And if they are not communicating across their professional disciplines then the new work is not being incorporated.

RES: Well that is precisely the problem. So this had a chilling effect on the study of evolutionary patterns in invertebrate animals until the treatise came along. Now finally you have a few people who are combining molecular genetics and differentiation with the comparative anatomy, comparative embryology and the Treatise of Invertebrate Paleontology information.

That is I guess what I meant when I was suggesting that as an evolving document, the treatise should be absorbing methodological changes, innovations that are appearing and particular times so for example when the treatise appears in the fifties two of the most exciting things happening in theoretical methodological things happening are the new systematics coming on line in a very strong way and the results of the evolutionary synthesis material.

JC: So what was an example of this in the Treatise.

RES: The best one I can think of was the situation in clams. This major class of organisms, I am using clam in the loosest sense of the word. Even the name of the class varied depending on where you are. If you were a Brit the class was called the Lamellibranchiata, if you were French or American or Canadian it was usually called Pelecypoda and one of the knock down, drag out battles in production of this thesis was what to call class and they finally compromised by calling it the Bivalvia which is an old Linnaean term that nobody has used for years but in the case of this class the bivalves, the clams in the loose sense, there had been several major modes of classification. They were all based on single characters, so most British paleontologists classified the major subdivisions of the class as stages of gill construction and the major orders within the class were the Protobranchia with gills essentially like snails and monoplacs, the primordial sort of molluscan gill. Then there were the Filibranch gills and the Eulamellibranch gills and there was one more category that applied to one small group and I can't think of it at the moment, (Septibranch) it does not matter. Problem was fillibranch gills apparently arose from protobranch gills a couple of times and eulamellibranch gills from fillibranch gills a couple of times, septibranch I guess is the fourth one arose from eulamellibranch gills once. In other words there was parallel evolution and so you had some very, very strange bedfellows in terms of families in each of these orders. The American classification on the other hand was based mainly on shell structure and on tooth type. The French had a completely different system of dividing this major class into orders and a couple of people had proposed an analysis based on the ligament structure. And all of these were single character classifications and nobody could make sense of what the real phyletic arrangement of the families was because of the parallelism in each of these features. Finally at about the same time the treatise editors for Bivalves got together and did the initial research on what was actually going on in the initial Pelecypod radiation and at the same time John Pojeta who just recently retired as a major officer of the Paleontological society and continues to work for it, did his Ph.D. thesis and his earlier master's thesis on clams mostly from the Cincinnati area covering the last quarter of the Ordovician. This is close enough to the initial Pelecypod radiation in the second quarter of the Ordovician diversification of later families that were in fact closely related still in the late Ordovician so he had a strictly phyletic approach. It turned out that the most useful approach was a modification of the French approach using data from all the others.

It was an eclectic classification. Douville had a classification of bivalves into sedentary, burrowing and boring and normal bivalves and it turned out the sedentary branch in particular was essentially monophyletic and a great big chuck of Pelecypods or of clams and the others could be sorted out into several groups that had essentially the same ecological behavior and hence the same kind of anatomy. When they combined Douville classification with the data on hinge teeth, on shell structure, on gill structure in modern forms and ligament types, lo and behold they had a picture of a very rapid differentiation of the class from its origin in the early Ordovician to a essentially all of the orders that exist today by the end of the Ordovician and the biggest problem is that very few people had worked on these early clams. And the result was a reclassification system based on all of these things that truly reflected the phyletic analysis of what was going on and this was really the result of major serious discussions going on over a period of 10 years with continuous input of new kinds of data. Much later on it was further elaborated when Pojeta and Runnegar showed that all of the microscopic mollusk shells from the early Cambrian can be shown to radiate in precise stratigraphic order into things that are the earliest clams, the earliest snails and the earliest cephalopods.

JC: It strikes me that this work not only showing new data but also adding new methodological content.

RES: As much as it is adding new methodological content it is also using the information at hand to see where the biggest holes are and where the biggest advance could be made with minimal investment in research time. There is never enough research time to do it all and so Pojeta's Ph.D thesis on the Ordovician clam radiation turned out to be very critical and the analysis of these small shelly fossils from the early Cambrian similarly turned out to be very critical. You pinpoint a particular region in time and space and morphology where you have got to focus your attention and if you focus your attention there you will have major results.

This is in fact what I have done in totally different field. When I started to work on the Cretaceous extinction problem it was because I was getting major static from my department about not doing original research. I had been working on the geology of Southeastern Minnesota and describing occasional fossils of new things as I found them because this is what I could get money to do. It was not necessarily what I wanted to do but it had to be completely redone for reasons that are listed in my autobiography, my predecessor once removed Clinton Stauffer had done an absolutely abysmal job of measuring the stratigraphy and mapping the rocks of the states and I found so many errors that I ultimately had to completely redo almost everything he ever did.

JC: And the money there came out of the states.

RES: Out of the Minnesota Geological Survey and it was put to all sorts of useful results but the department could not see anything really useful coming out of the stratigraphic paleontology at Southeastern Minnesota because we know all there is to know about those things, right. You are not finding anything new and exciting. You are not on the cutting edge and so I sat back and thought about what are the problems that are solvable that would provide the greatest impact in terms of theory or documentation of previously existing theory about evolution in the fossil record. And I had read what Simpson had said about mammal evolution in his Mesozoic mammal papers and where mammals came from and I had read a lot of simple nonsense written about the transition between dinosaur bearing rocks and mammal bearing rocks at the K/T boundary and it was truly awful.

JC: What made it awful?

RES: Well because the standard practice was to say the last fossils we found were dinosaurs and we did not find any more fossils for another 200 to 600 feet and then there are late Paleocene fossils and so even though there was 200 to 600 feet of rock in there, there is a big unconformity. There is time missing. You are missing all of the early and middle Paleocene. What nobody paid any attention to was the fact that where you had several different kinds of them in the same area with fossils and the best data was that of Jepsen from the Big Horn Basin. The early Paleocene is very short. The middle Paleocene is somewhat longer and the late Paleocene is most of the sequence of rocks. Looking back at what Simpson had said about what came to be known as adaptive radiations but he called it quantum evolution. It was very apparent to me that this is just exactly what you would expect. People had divided Paleocene time into three parts based on approximately equal amounts of change in mammal evolution. So there was the early Paleocene and it was just about as different from the middle Paleocene is the middle Paleocene was the late Paleocene with a late Paleocene from the early using and because these were always blocked off in correlation diagrams as the same size block, that is what you do with correlation charts. Everyone automatically assumed the thickness ought to be there and if you only had 600 ft. of rocks between dinosaurs and late Paleocene and then you had a thousand feet of late Paleocene you must have an unconformity in there.

This is completely disregarding the fact that early in a radiation one would expect rates of evolution to be very high and the stratigraphic interval to be very short and the rates of evolution would decline exponentially with time and the stratigraphic intervals would increase exponentially with time as they do and it was clear to me that although the dinosaurs were extinct and the mammals underwent a radiation nothing else changed or nothing much else changed. There were some minor changes in plants. They were relatively minor and there were essentially no changes in the fish, frogs, non-mammal vertebrates and nobody knew anything about the birds and so the obvious thing to do was to look at the ecology of the extinction of dinosaurs in a region where there was essentially a continuous record of sedimentation, as closely as you would determine.

JC: You said that was obvious, why.

RES: It was obvious to me because what needed to be done was the assignment of relative thicknesses and durations to the early, middle and late Paleocene mammal stages and also to look at what happened in the extinction of dinosaurs, was it catastrophic, was it gradual, did they all go at the same time, what was the interval over which dinosaur extinction took place. And at the same time you are doing this, make an analysis of the community structure. Extinctions are ecological catastrophes. You have a major change in community structure in which you remove one well actually several trophic levels of terrestrial communities by causing dinosaurs to disappear and then regrow them from new with a different group of animals, mainly mammals and so the obvious thing to do was to go find a place where there were continuous sequences of rock across the Cretaceous boundary and collect them stratigraphically in detail so that you know which things were contemporary and see just what was really changing across the boundary.

Up until that time it had been dinosaurs, mammals with this big change and no stages in the process. I applied to NSF for funds to do it and of course they turned me down. I was an invertebrate paleontologist, what would I know about such things. So at the same time I had raised a student, an undergraduate student, Bruce Erickson who could never get through calculus so could not get a degree in geology. He was an excellent collector and preparator of fossils. I got him a position as preparator at the Field Museum where essentially he served an apprenticeship under my old friend Orville Gilpin, Chief Preparator of vertebrate paleontology in the Field Museum. He did the usual preparatory things (cleaning of fossils). Then he came back. He did get a bachelors degree from the Zoology Department. where there was not a calculus prerequisite and I managed to get him a position as Curator of Paleontology at the St. Paul Science Museum which is now the Science Museum of Minnesota. The museum was ready to change. And we developed some very, very nice exhibits based on what they had. I had a little Oreodon skeleton that my baby brother Jimmy had collected in 1956 or 57 on a field trip and it was a complete skeleton all curled up from the turtle Oreodon bed. Gave that to Bruce and he proceeded to make a mount of an Oreodon. He made a number of other mounts from things he collected. The paleontology part of the museum was going great guns and he convinced the Museum that they needed a dinosaur. Well we knew about Barnum Brown's trip to Hell Creek in 1892.

[end of tape side].

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Interview with

Robert E. Sloan

by Joe Cain

Session 4

A series of interviews with Professor Robert E. Sloan regarding his career, the recent history of paleontology, and the life of an American scientist in the second half of the twentieth century.

These interviews were made possible with a generous Mellon postdoctoral fellowship from the Department of History of Science, University of Oklahoma.

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#4 Tape 3, Side 1
12 March 1996

RES: Professor Robert E. Sloan, Department of Geology, University of Minnesota
JC: Dr Joe Cain, interviewer

JC: We continue our conversation about the need in the late 1950's for Bob to come up with a sexy research topic and his activity with Bruce Erickson at the Science Museum.

RES: As I was saying, it was not possible to get federal research funding for this so I had to use my wiles to see what I could do. Bruce was able to convince the Directors of the Museum to spring loose about 600 dollars and five of us headed out for Montana in the Science Museum carryall.

We went out to a rancher, Dalton Trumbo on Hell Creek. The reason we went there is that Barnum Brown had found over 200 skulls of *Triceratops* in the eight miles of Hell Creek. And as such, this looked like an awfully good bet. So on June 13th, we piled into the van--Bruce Erickson and I, Paul Lukens in the Zoology Department and Delwin Olson who was Bruce's prospective brother-in-law.

JC: [Bob is referring to notes from his collecting trips at this point.]

RES: We were met out there by my partners in crime, John Hall, who was at this point a major student of Cretaceous macro-fossils and pollen and two of his students, Robert Melcher and Norman Norton. When we got there, we set up camp and within five minutes of setting the tents up, Paul Lukens managed to find a *Triceratops* skull. We found a lot of other fossils as well, measured a detailed section, collected pollen samples from Norman Norton's Ph.D. thesis and made a wide variety of collections. I have got seventeen fossil vertebrates that we found and four measured sections in a period of about ten days. With a very small party, this is doing very well. We came home with a plastered *Triceratops* skull complete and it was enough so that the Science Museum was very excited and we had more than enough so that even though we still could not yet get any NSF funding, we were able to convince the Hill Family Foundation in St. Paul that they ought to fund us at a low level. So we then went out in 1961 and 1962 to do more extensive work.

We needed a bigger 4 x 4 vehicle so we purchased a Dodge truck formerly owned by Erie Mining Company. They had greatly added to the springs of the thing, so it was sprung as if it were two-and-a-half ton truck even though it was basically a three-quarter or one ton truck. The fenders and the cab were basically similar to the Dodge three-quarter ton truck of World War II and so was the basic machinery. It didn't have a box on the back so we found a war surplus body from a Dodge model 1953 three-quarter ton GI truck. It was actually a Marine Corp truck, not that it was any different from the Army ones that I had driven in the Guard. And we carefully repaired the rusted-out holes, bolted it onto the truck, frame and cab that we had, added a plywood box to make up for the fact that there was about eighteen inches between the front of the box and the cab where there wasn't anything, and then rebuilt all the top bows so that we could make a canvas top. Harris Machinery took regular GI twelve-ounce tenting canvas and made a tarpaulin type top--a covered wagon top just like GI trucks. We painted the thing OD because we got five-gallon buckets of OD paint free from war surplus as an educational institution. We had no money for anyone to do this so Bruce and I and our students literally rebuilt the truck to what we needed to have. It was basically mounting the truck body, patching any holes in the floor that had been poked and rebuilding the top bows and painting the thing. Not a great deal of trouble, but it got us a very, very strong and sturdy four-wheel drive. Bruce's dad welded up a major trailer hitch for the back end of it and also a flat-bed trailer to haul Bruce's World War II vintage antique jeep, which he still has, and it too was painted OD so here we were. We didn't paint a white star on the door. We painted a white *Triceratops* head on the door instead. So we looked like an Army outfit going down the road. We packed all of gear for six weeks for six people in the back of this thing and rode off to Montana. Without a load, it was pretty rough. By the time you loaded all the dunnage and gear for six people for six weeks, it rode pretty well. And we always had four people sleeping in back and a couple of people up in front.

In `61, we went out with a much larger party and carefully collected about two-thirds of a *Triceratops* skeleton. This really made the Trustees of the Museum very excited because this was a major mount--a dinosaur--a complete dinosaur as opposed to just a skull. And it was beginning to put the Museum into a big-time mode. This, in fact, was the final impetus necessary to build the St Paul Arts and Science Council on the strength of the Science Museum having a major dinosaur skeleton, the Arts and Science Council then proposed a joint building to handle the Shubert Club, the Civic Opera, the Civic Orchestra, a gallery or two and the Science Museum. And this is the eastern building of the Science Museum complex today.

JC: So this clearly was a big deal for them?

RES: This clearly was a big deal, and the presence of an articulated large dinosaur meant that the powers that be in St. Paul interpreted the Arts and Science Council as something that was off and running. This was a big-time Museum as opposed to the dinky little [set of] storehouses that had existed before.

JC: How did finding this material affect your status in the department?

RES: Not much initially. They knew I was working on the problem of dinosaur extinction. I found in 1961 my first fossil mammals of Cretaceous age and, for the first time ever, I went to a convention of the Society of Vertebrate Paleontology. I had been a member for a long time, but I didn't really have anything to contribute and I couldn't afford to go. This time I thought I ought to go. So I did. The convention was held in Denver and there was a photograph which [you have] that shows the people of this particular convention.

JC: What was that convention like when you gave your presentation?

RES: The convention was very unlike the highly structured conventions of the Paleontological Society, the Geological Society of America, and the Society of Economic Paleontologists and Mineralogists. Simpson and Romer had broken away from the old Paleontological Society because they were basically zoologists looking a fossils as opposed to geologists looking at fossils. They also wanted to get away from the highly structured fifteen-minute or twenty-minute papers into sort of a show and tell [system]. [There is a break in the recording at this point. It continues:]

JC: So here you are at the Denver meetings of the SVP. You've just found some material. You're in front of a rather loose and informal group where everybody reports on their stuff--and what do you say?

RES: Well, I described the *Triceratops* skeleton that we had collected. It was basically the back two-thirds of a triceratops plus a skull from another one, and we had a large number of fossils of all sorts of things besides the *Triceratops* including a half a dozen Cretaceous mammals. And nobody had been working on Cretaceous mammals except for Simpson for a very long time. Simpson wrote one paper [and] Albert Wood wrote one paper and that was it since Simpson's 1929 thesis. So we caused a little stir, and particular, it caused a stir with Bill Clemens.

Malcolm McKenna had rediscovered the washing technique. Malcolm was, of course, a graduate student while he was still in high school. He was independently wealthy, a millionaire a couple of times over, but a very serious paleontologist who was going to make paleontology his life. And he found a series of localities in the earlier scene in northern Colorado and he had visited Claude Hibbard and his washing operation down in the Plio-Pleistocene of Meade County, Kansas, and adapted and developed the washing technique so that much larger volumes of material could be processed even than Hibbard was processing. Hibbard was using a half dozen screens. Malcolm was using fifty or sixty at a crack. And while he was doing this, he was also poking his nose into everything else and he took two young graduate students from Berkeley, Bill Clemens and Richard Estes, out to Lance Creek. They visited some of the classic localities where Hatcher had collected *Triceratops* skulls and Cretaceous mammals, were able to relocate the old *Triceratops*' quarry. Its got a sizable hole in the ground and showed that this was going to be doable. So Estes and Clemens collectively had a project in which Clemens worked on the mammals and Estes worked on the lower vertebrates. Now Clemens had completed most of his Ph.D. thesis at this point. It was not yet published, but would be as soon as he could get it revised and Estes had made major progress in his. And suddenly I show up with Cretaceous mammals for a new place--God, nobody had ever heard of--and naturally the first mammal I found was the biggest chunk of Cretaceous mammals I have ever seen. It was the back half of the jaw of a Cretaceous opossum in which just the back half of the lower jaw was about two-and-a-half inches long--very big and massive bone.

JC: Did you have it on your possession there?

RES: Its in the collections.

JC: I mean did you take it--

RES: Yeah, I think I took it. I don't remember. It doesn't matter.

JC: A show and tell technique is real common in the SVP.

RES: Right. So on the strength of what I reported when Malcolm got back to the American Museum, he mentioned to a graduate student at Columbia and the American Museum, Leigh Van Valen, that this might be a solution to the problem of some weird mammals that had been collected by amateur collectors under the long distance direction of Walter Granger and that Walter Granger had brought back to the American Museum. Unfortunately, Granger died of a heart attack on a trip back from the field to the Museum and these collections were made just one county east of where I had made my collections. So Leigh wrote me and asked if he could go out the next year. And, I said sure, come along. And so we knew that this locality which ultimately became Harbicht Hill was close to the Cretaceous Tertiary boundary in terms of biostratigraphy, these were extremely early Paleocene mammals mixed with Cretaceous mammals and dinosaur teeth. They were slightly older mammals than those in the oldest Tertiary mammal collections, those from Mantua quarry, that had ever been made before. This was a Puercan locality, from the Big Horn Basin. So in 1962, after starting off in Hell Creek and getting the crew going, Leigh showed up by what was called the mail stage, and took the train into Miles City, Montana, and then you paid a small amount of money and you rode in the mail truck from Miles City to Jordan, which is about 100 miles. Its called Mail Stage. So Leigh showed up. We picked him up in town and by this time, we had started a new *Triceratops* quarry for the front part of a dinosaur. This one had a good skull and the front two-thirds of the skeleton so there was significant overlap and we could come up with a good composite mount. This is the dinosaur that is now in the Science Museum.

So also showing up there was a graduate student in zoology named Bill Nelson. He was a student of Sam Eddy, and mammalogist, great artist, and bachelor. Bill was driving a Rambler two-door sedan so Leigh and Bill and I took some of the equipment, piled into the Rambler, and drove what is by air about 50 miles east. And by the route we had to go was about 200 miles, camped at the shores of the Fort Peck Reservoir on the opposite side of the Big Dry Arm of the reservoir, and started looking for this locality. All there was in the way of information was the fossils, the information that it was one mile from Dr. Cases dinosaur and a photograph of the locality--period--that was it. And we only had something like fifteen townships to search. We knew it was in a school section. We knew it was close to Dr. Cases dinosaur, but nobody at the University of Michigan knew precisely where Dr. Case collected his dinosaur. So we were looking in school sections. We were checking with old-timers in the area to see where schools were in the `30s because this was initially collected in 1935. And we started looking one school section not too far away from one of the places where Dr. Case collected some bones, and there on the top of a very steep hill about 150 feet above the Cretaceous -Tertiary boundary, Leigh found the biggest single tooth of a Paleocene mammal we have ever found. And what it was was the upper canine of an *Eoconodon* which is a arctocyonid and as such close to the mesonychidae which is the ancestral family for the whole whale clade.

JC: What did you think when he pulled it out?

RES: Oh, we rejoiced. And then we spent two or three days on the top of the hill, all three of us. It is a very steep hill. And we picked up a scrap of jaw and we picked up lower molar of a small hyposodontid and a multituberculate tooth--I forget which one. And we were really rejoicing so we went into town to get some groceries, and while we were in town, we met with the head of the Corps of Engineers at the Fort Peck Dam, Donald Beckham, who was a civilian engineer in charge of this Army Corp of Engineers project--the dam and all of its associated things. Well the Corp had the only money that was to be had. The Corps was doing all the work that should have been done by the Montana parks department and everything else. They had been out prospecting a couple of weeks earlier with Newell Joiner from the part service. Newell Joiner was a graduate of Nebraska, who had been called in to organize the collections the amateurs had made back in the `30s that were stored initially in the theater building in Fort Peck and later in the power house at the Fort Peck dam. As there was a lot of overlap and there was a lot of stuff that didn't need to be soared and some was worth displaying and some wasn't so they found this locality on Bug Creek.

Don had written us because he knew I was looking for Cretaceous mammal localities. We already had some. And he said, well, we can take you out to Bug Creek. We'll go now, except we have to be back in town by 6:00 because there is a party tonight that we're hosting. So we drove back to Rock Creek Park and we parked the Rambler there and they drove us on out to Bug Creek and showed us the place. And when they showed us the path where they had picked their fossils up, the side of the quarry, all I could see was three monster ant hills made by the harvester ant *Pogonomyrmex.* The harvester ants for years have been known to carry small bits of gravel for up to fifty yards to form a hill mantling their tunnels to shed rain. And so Hatcher collected ant hills. Horace Wood collected ant hills. Simpson never did. Well, he may have but he never wrote about it, but it was a standard way of prospecting for mammal localities. You took a look at every ant hill. This meant you had to plug the exit holes of the ant hill and look quick before they came out and stung and bit because they have the same stinger glands that their ancestors, the wasps, have and they're still nasty.

Anyway, I saw three ant hills sitting right on top of the hill with Big Creek samples in the side of the same hill. And so Leigh and Bill and I each took an ant hill and a film can and our tweezers and plugged the ant hill and furiously started collecting teeth off the ant hill and putting in the film can. We were dragged kicking and screaming off the outcrop to go back to our camp after five or ten minutes--a very short period of time because they had to go back to town.

JC: And why were you kicking and screaming?

RES: Because when we got the cans back and started looking at the material we had collected, we had found 130 mammal teeth in five or ten minutes among three of us. This was a bigger collection of Cretaceous mammals than those present in the American Museum of Natural History which had the biggest collection of Cretaceous mammals in the world!

JC: A small find, huh?

RES: A small find! Well, we continued to mine our amateur friends for localities. Don had also found the classic locality in Bug Creek and other places where Roland Brown had collected fossil figs, so he showed us that locality. And he showed us Bug Creek West which is slightly higher and slightly later than Bug Creek Anthills, and then we started prowling around the area and we found more localities.

JC: And you knew you were overlapping the K/T boundary?

RES: Yeah.

JC: You knew you were working the Cretaceous sites early on?

RES: This was really very obvious. There were articulated dinosaurs below a particular level. That is clearly Cretaceous by any standard anyone was using. In modern terms with the K/T impact layer, it was below the ?? layer and above the ?? layer. We were still faced with the problem of locating this fossil locality that Harbicht had found in 1935. Finally, one day, one of our friends in the Corps, Richard Erickson, showed us a map that a doctor, whose name I can't think of but its in my autobiography because I got it out of the field notes, had made in about 1940, showing all of the localities that he knew of in the Fort Peck fossil field, which was actually stretched from the Fort Peck dam down south to McGuire Creek. And here we found a little dot labeled Mammal Butte, and one mile away Dr. Case's dinosaur. So we piled into Don Beckman's jeep--we just drove around in the Rambler--you can't go across country in a Rambler--and Don figured out a way for us to drive to Harbicht Hill and what became known as Harbicht Hill--and there we were able to duplicate in the next two or three hours--exactly the specimens that Harbicht had collected for Granger in 1935. And we actually found a screen that Darwin Harbicht's kids had been using while they were camping on the locality and screening fossils for teeth. The screen consisted of the bottom of an old cane chair across which someone had tacked some galvanized iron screen wire. The screen wire was rusted through, but there was enough on it so you could still see it, and there was no doubt, this was the place.

JC: He knew you were there?

RES: We knew we were there. The hill matched the photograph exactly. We duplicated the specimens and got a lot more and we were off and running.

JC: Now while you were doing this, of course knowing that you were doubling and tripling the worlds supply of Cretaceous mammals, were you also talking with Leigh and the other people there about the paleoecology, were you also collecting material?

RES: We certainly were. Leigh and Bill and I stayed camping at Rock Creek Park for quite some time. Let's see just how many days...

JC: [Bob is looking through his research notebook from the field.]

RES: 1962--we left the Jordan Hell Creek camp. On the third of July, we moved to Rock Creek the hard way and stayed there collecting until Bill and Leigh had to go back.

JC: And all this time, were you talking about interpretations of the material that you were finding?

RES: Oh boy, were we ever! Leigh had been making extensive studies of Paleocene mammals and was exceedingly familiar with all the Paleocene ungulates. And I was talking to Leigh and got Leigh to talk about this kind of information on the basis of actual comparisons of specimens because the American Museum had the major collection of Paleocene mammals. There was a similar collection at the National Museum, but the one at the American Museum was actually bigger because Simpson had spent from `29 until about 1940 doing mainly Paleocene of the western United States and Paleocene and Eocene and Miocene of Argentina.

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nterview with
Robert E. Sloan
by Joe Cain
Session 5

A series of interviews with Professor Robert E. Sloan regarding his career, the recent history of paleontology, and the life of an American scientist in the second half of the twentieth century.

These interviews were made possible with a generous Mellon postdoctoral fellowship from the Department of History of Science, University of Oklahoma.

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#5 Tape 3, Side 2
12 March 1996

RES: Professor Robert E. Sloan, Department of Geology, University of Minnesota
JC: Dr Joe Cain, interviewer

JC: [continuing discussions from Tape 3, side 1] In your interactions with Leigh in that first season of collecting with him, clearly you were exploring possibilities as you were picking up things

RES: Yes.

JC: What was your immediate impression of Leigh and how did you two hit it off?

RES: Leigh is an extremely interesting guy who is very shy and speaks in a very soft voice. He's very difficult to understand at meetings because he speaks so softly. And he tends to pause a lot and think before he speaks so getting him to say something is an interesting chore. He's one of the slow talkers of America, but very powerful thinker. He had been through all the ungulates and he had worked out many kinds of ancestral descendent relationships. I got him to diagram for me some of these conclusions that he had made. He never did get around to publishing them so I published them instead. I added more because by this time, I was accumulating every single Paleocene reference in North America, either by picking up old reprints and old but still in print publications at the American Museum. Jepsen sent me copies of his papers. I photocopied Patterson's papers, those that were not available. So essentially I had a complete corpus of all of the Paleocene literature by the time I got home that summer.

Leigh and I worked out these things. We got into the idea of--were all of the stages in the radiation of ungulates present in museum collections at that point? No one had ever considered the possibility, but Leigh began to realize just what was there. We started talking about general [evolutionary] problems, brought up the problems of whale origins, and I said, Brian Patterson, my professor in mammals once said that the mesonychids looked like the best possibility for the origin of whales. And Leigh had already documented the transition between primitive Arctocyonids and primitive mesonychids to very slightly more advanced mesonychids. His thesis was actually on the rest of the Deltatheridia, everything but mesonychids but he was very familiar with all this stuff. And he said, that does indeed sound like a good possibility that he hadn't considered before. And that ultimately led to write a paper in which he proposed that mesonychids were, in fact, the origin of whales. And its been brought out now.

Once the hypothesis had been made on the basis of specimens in the field with gaps in morphology, then it was possible to pinpoint a place to look for that morphology. The initial primitive mesonychids that are to be found, are to be found in North America. But the radiation of mesonychids, is just basically Asiatic and at some point in our terrestrial middle Paleocene which is in the first third of the Paleocene, migrated from North America to Asia and then underwent a radiation in Asia. And the earliest whales had been found in Middle Eocene rocks in Egypt, in the Gulf Coast of America, and in the Himalayas, all in Tethyan marine rocks. Once people realized that it was the Asiatic mesonychids that were the probable origin of whales, which was ultimately based on this chance conversation of Leigh and I over the picnic tables in our Rock Creek Park, then it was possible for someone to physically go look for the transition between mesonychids and whales. The transition clearly took place in that part of the Tethys Sea which is now folded into the Himalayas in Northern Indian and Pakistan. Well, for political reasons, Pakistan was the place to go and Gingerich went there and other people subsequently have gone there and we now have multiple stages in the transition between primitive mesonychids, carnivorous hoofed mammals and whales.

So there is now no longer any doubt and there are all the transitional fossils one might need. This is an example of just how things progress in paleontology. First you have to get an idea about phyletic relationships and they will be things that involve major transitions. Then you have to work out the morphology of the transition and the paleogeography of the transition and then you go look in a very limited time frame in a very limited area, there you will find the transitional fossils. The delightful thing that we realized before the summer was over was that we actually had in the specimens we had together with the previous Paleocene collections that were available, the complete radiation initial North American phase of the radiation of hoofed mammals. And there weren't any gaps and everything was filled in. And the big reason there weren't any gaps was because I had pinpointed a very specific, very narrow, stratigraphic interval only about 50-feet thick in which the search should be made. Actually, more like 150-feet thick, 50 meters thick. This short interval out of 2,000 feet was the place we really needed to look for the initial phases of the radiation, and we had it.

JC: And you knew you had it at the time?

RES: And we knew we had it at the time. So, at this point, we were on the second year of the Hill Foundation Grant. Bruce was very upset that I had spent so much time away from the dinosaur expedition which was the real reason.

JC: When you were co-opting the research money for other--?

RES: Yes. We were co-opting the research money for other sorts of things. And so we left for Rock Creek on the 25th of June and Bill and Leigh had to leave on July 3rd. So--eight days.

JC: Not a lot of time, but a--

RES: Not a lot of time, but a tremendous amount of work. Leigh was never able to return to the field.

JC: Why was that?

RES: Because by this time, he had accumulated sufficient ultra-violet exposure in his previous field work in the San Juan Basin and in the week he was out in Montana so that he developed skin cancer. And his eyebrow has been rising ever since. Skin cancer is an occupational hazard among vertebrate paleontologists. You are out at high altitude in very bright sun in reflective surroundings, and you need all the protection that you can. I, so far, have managed to escape and I think I'm probably in pretty fair shape. I always wore long sleeved shirts, full jeans, boots, and a hat with a 3-1/2-inch brim to shade everything.

JC: To stay covered up? What do you think the consequences were of Leigh not coming back out to the field with you?

RES: Not much in the long run. Leigh enjoyed field work, but his real mint here was analysis of museum specimens, making comparisons between specimens, then ultimately writing them up. Leigh had great facility with writing. He was very good in the field, but it wasn't important that he do the field work. It was important that he do the museum work. So, in the long run, it didn't make a great deal of difference. Leigh and I continued to collaborate co-writing papers until about 1986.

JC: And what I would describe as a really robust research.

RES: Very robust research program and with very great synergy. We would play off each other's strengths.

JC: But what were those comparative strengths?

RES: My strength was a visual memory which I've already talked about at length at one place or another. But I could literally remember precisely the illustration of a single tooth and match it to another illustration done in a totally different mode of illustration or to a specimen found in the field. Leigh once made a comment--I pointed out that a badly drawn line drawing in one of Simpson's papers from 1935 or thereabouts was, in fact, the same species as one in another Simpson paper where it had been named and was the same species in a Jepsen paper. Leigh made a jocular comment, "R. E. Sloan identified this specimen from a distance of 1,200 miles." Or maybe it was 2,000. Whatever! But I was right.

There are other examples of that facility. Simpson, of course, in 1945 I guess, had done a spectacular paper on the middle Paleocene mammals--primates. These were beautiful shaded pencil drawings by Chester Tarka, the most magnificent illustrations of these teeth that had ever done. The previous illustrations had been very, very tiny, crude line drawings. So there were these magnificent illustrations. And, of course, by that time I had a copy of the paper and I steeped myself in the morphology of these mid-Paleocene primates. The oldest primates then known in the world. I knew them cold. I had also done what would now be called a cladistic analysis of the features that were present and absent in each of these things and essentially represent the synthesized morphotype--to use a word that is not always well regarded--of what the probable ancestor might look like and what size it would be.

By this time, in 1963, we were actively working what came to be known as Purgatory Hill--the site where Leigh had discovered this big arctocyonid canine. And by dint of long effort, we got 600 mammal teeth out of 10 tons of rock from the top of this miserable hill. I knew that if there was ever going to be a place where I would find early Paleocene mammals, it was going to be at Purgatory Hill and so we had improved the collecting from one tooth every three person days of crawling over the outcrop to twelve teeth a day which is a significant improvement. And I had this morphotype in mind. This was six months after the collections were made. I was finally getting around to looking at them because we removed everything that wasn't teeth.

That was quite a chore and it was extremely exciting to be sitting down in the basement of Pillsbury Hall in Room 19 poring over these loose teeth and suddenly finding the precise size and shape that I had predicted. The whole point of this operation was one of providing continuity. The big gaps in the geologic record are usually at extinctions and radiations. These are always the gaps that pose the biggest problem for fundamentalists because they stick out their tongues and stick their thumbs in their ears and bite on all their fingers and say, ya, ya, ya. What's with this gap. Well, the real reason for the gap is that there aren't enough paleontologists to go around and look at everything. You have to budget your time and your collecting and for the most exciting kinds of paleontology, the thing to do is to concentrate on these gaps. Develop techniques that will let you collect materials in quantity and then examine them in bulk rather than depending on just casual collecting, which is what most paleontologists do most of the time. And the Bug Creek collections tied together the entire mammal radiation of the Cenozoic for at least the ungulates. And Purgatory Hill provided a way of looking at early Paleocene mammals that none else had because none else had large collections. This was 600 loose teeth. And the biggest early Paleocene collections were those from the San Juan Basin where the smallest specimens were the size of raccoons and bigger.

But none of the little ones were present. And here I had the little ones.

JC: So not only did you have the specimens but you had not only ecological range but also enough within of a single species to do biometric work.

RES: To do biometric work within a species and to talk about the changes from one species to the next.

With the changes actually being minimal so that none would ever be concerned about there being a gap between them. Within five major sequential localities in the Bug Creek or McCone County area, we could watch them evolve, go from one species to three species to five species to seven to eight and now even more. And if you include Purgatory Hill to about 60.

JC: So here you are in about 1963 and your department had sent you out in the late `50s with the call to go find something sexy and exciting, and you had managed to scrounge some money up through the Science Museum to go out and dig up dinosaurs. Then you used that money to come across a lot of mammal material and you presented that material at SVP meetings.

RES: Yeah.

JC: And here it is about 1963--

RES: Here it is at 1963. It is now the first year after a whole summer--six weeks worth of field work with NSF money, enough to do the job the right way and with a long, continued party instead of just stealing time from other things.

And I got up and described the sequential faunas that we had at the base of the Cenozoic--some of them in the Cretaceous, some of them in the Paleocene. We now know that some of the things we thought were Cretaceous because they had dinosaurs in them were, in fact, early Paleocene, but the base of the thing is still in the late Cretaceous.

And we have a true continuity of deposits right across the K/T boundary with significant changes in ecosystem. In addition to my own work, I had students work on the mollusks. I had students work on the pollen. I had students work on the leaves. I had students work on the lower vertebrates. And, of course, Dick Estes immediately was given a very sizable amount of Bug Creek material to work with so that he could extend his Lance lower vertebrate studies to Bug Creek and the entire sequence.

The result was a neat package. In November `63, about the time that Kennedy was killed, I was allowed somewhere between 40 minutes and an hour of yakking away at the SVP convention describing in detail the bio-stratigraphy, the paleoecology, the evolutionary phylogeny of this sequence of faunas we had worked up in the course of `62 and `63.

JC: And that length of time, of course, is--just for the record--is an extraordinarily long length of time--15 to 20 minutes--20 minutes being the maximum is quite a tradition in the SVP, so forty minutes is non-trivial. And so what was the reaction at a meeting like that?

RES: The reaction in the department was, is I was approved for a promotion to Associate Professor. I already had tenure, and a few years later I was approved for promotion to a Full Professor.

JC: And would you say that's on the basis of this material?

RES: On this material and the things that I generated from it. Because in the process of analyzing the base of the radiation, I also took a look at all the rest of the Paleocene rocks of North America and the radiation, worked out the physical chronology of these things in terms of partitioning of available Paleocene time into these early and middle and late intervals. They're not equal. Early Paleocene is about a million years. Middle Paleocene is about two and late Paleocene is about seven. Which is, of course, precisely what anyone who read Simpson's *Tempo and Mode* would anticipate.

But nobody did, not even Simpson. Its a little surprising to me that Simpson didn't, but he had--say if there really was unconformity that everybody had been putting in all over the western United States because there weren't any fossils between the dinosaurs and the late Paleocene rocks, and there wasn't enough space to cram the early and middle Paleocene in. Well of course there was.

JC: Of course, Simpson didn't have much in the way of geology training.

RES: No.

JC: He definitely was a zoology oriented paleontologist.

RES: Yes.

JC: So you clearly had found a sexy series of projects.

RES: I had found a sexy series of projects, and Leigh and I continued to cooperate.

JC: You personally, you were getting grant money.

RES: I personally got two years grant money from NSF. And then we had serious family difficulties because my eldest child was diagnosed with schizophrenia, and it was literally tearing the family apart. And I had to reduce the amount of field time I was spending and reduce the time I could spend on research, and I had to essentially slow back for awhile until all of these family problems were solved. And a schizophrenic child produces a lot of problems in a family.

JC: What are the sort of year ranges for that? Can you recall that?

RES: 1964 to 1967 or 1968. In any event, the net result was that I would propose ideas to Leigh. Leigh would propose ideas to me. We would each run with a thing and then generate them. One of the things I did was immediately go around looking at all the Paleocene collections. Well nobody had worked on the multituberculate mammals since Jepsen in about 1940. And this was now 24 years later.

A fair number of collections of multituberculate mammals had been made by all the people who collected Paleocene and there weren't many, but there were a lot of specimens. And, at this point, I knew what the stratigraphy and chronology of these things was from the ungulates and I came up with an independent chronology based on the multituberculate evolution. It turns out that there were about a dozen described species of multituberculates. And between the undescribed specimens that other people had and my own collections, I had over 40 species to describe.

It turned out all the previous generic assignments were troublesome. This had interesting consequences. Simpson's eyesight was not as good as it might have been and apparently he had very terrible microscopes to work with. And so Jepsen and Simpson argued back and forth in the literature about Paleocene efforts, and particularly multituberculates. It wasn't obvious unless you read all the papers of Simpson and all of the papers of Jepsen in chronological order. But, of course, I had done this. And it became apparent that part of the problem was that Simpson had seen all the specimens. They had been badly illustrated. Jepsen's specimens were well illustrated and well described, but there were all these other specimens to be described and I could sort out the multituberculates. Well I had a multituberculate phylogeny now. It was a primate phylogeny and an ungulate phylogeny. It became possible for me to generate a sequence of time order of all the known Paleocene localities in North America. By that time, it was up to about thirty. Previously, they had been simply grouped into these major categories--early, middle and late Paleocene, and a little bit of Clarkforkian.

But I could sequence them within those things and come up with more precise chronology. And so I did. And I worked out the phyletic arrangement of these multituberculates, demonstrated for myself that phyletic gradualism did, in fact, exist because up until that time, although I had heard about phyletic gradualism since I was a very young undergraduate, I had never seen personally any prime examples of it. Well, here I had worked it out in multituberculates. Fairly regular changes in these species with time. I wound up reclassifying all the multituberculate of the Cretaceous and Paleocene of the world, and their fuliginous, and became the multituberculate specialist. When any graduate student had a Paleocene locality, he would bring his fossils to me and I would look at them, and I would tell him from his multituberculates exactly where in his sequentiated list, what part of the late Paleocene or middle or early Paleocene they came from. a whole series of graduate students from all over the country who made their pilgrimage to Minnesota to see these things. Jay Lillegraven came while he was working on his thesis, Dave Krause, Ken Rose, a gentleman from Alberta who ultimately didn't finish his thesis, but it was finished by Dave Krause later, and most particularly, Sophia Kielan Jaworowska.

Sophie and I have literally changed positions. She started out in life as a trilobite paleontologist working in Ordovician trilobites in Poland and was assigned the job of being Director of the expedition to the Gobi Desert of the joint Polish Mongolian Expedition. And she did, and she found a lot of multituberculate mammal skulls and decided she'd better work on multituberculates. So she came and spent a week with me and I trained her in multituberculates, pointed out everything I knew about multituberculate anatomy including the cranial anatomy of *Ectypodus*, and at the same time--well, later on--as she became a foremost student of multituberculates, much later on my injuries to my leg became sufficiently disabling so that I could not longer work on these strenuous vertebrate paleontological collecting trips, and so I switched to Ordovician trilobites. So there has been a complete reversal of Sophie and I.

JC: You developed a multituberculate training into something of a short course.

RES: Yes.

JC: What was that like?

RES: Basically, it was a matter of stressing what kind of anatomy you could get out of a very detailed analysis of the blades, upper and lower blades, which are peculiar teeth in these animals. To a lesser extent, the upper incisors and the molar teeth. But also the precise arrangement of muscle facets on any skull fragments they might have and certainly on the lower jaw.

And it was simply detailed morphology and I pointed out what kind of morphology each of these groups had, gave them Xeroxes of my drawings. My standard treatment of these things was to carry around a gridocular, 10 x 10 ocular grid with a grid in the focal plane of the eye piece so that when I put it in a microscope, I would have the optical grid and the specimen in focus at the same point. I would then take a spiral bound cross-grid notebook, course graph paper and literally draw the tooth in a standard orientation, square by square--and at this point, its a very simple thing. You estimate what position in tenths of a square the edge of this tooth crosses the right hand border, the lower left hand border of the square, or whatever borders happen to be involved. So you're never doing very much at all.

[end of tape side]

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Interview with

Robert E. Sloan

by Joe Cain

Session 6

A series of interviews with Professor Robert E. Sloan regarding his career, the recent history of paleontology, and the life of an American scientist in the second half of the twentieth century.

These interviews were made possible with a generous Mellon postdoctoral fellowship from the Department of History of Science, University of Oklahoma.

#6 Tape 4, Side 1
12 March 1996

RES: Professor Robert E. Sloan, Department of Geology, University of Minnesota
JC: Dr Joe Cain, interviewer

JC: [continuing from previous discussion] We're talking about the short course on multituberculates that Bob was working with. So you were talking about...

RES: About drawing techniques. The drawing techniques basically involve carefully surveying and the very specific orientation of the landmarks of these teeth. At first sight, when you look at them, they seem to be very simple. When you look at them in detail, they are not. They are very complex. They're probably something on the order of thirty genes in operation in the development of any individual mammal tooth, cheek teeth in particular. And so these architectural features are very significant and tell you a lot of information about the developments of species. Its possible to get some convergence, but it is much more difficult.

JC: And so when these graduate students or young professionals would come and basically take this short course, you'd have them repeat that?

RES: I'd have them repeat that technique. I would draw their specimens. I would show them exactly where in the sequence of multi-teeth phylogenies of their specimens came and I would give them Xeroxes of all my drawings of multituberculate specimens. At that time, they weren't published. They didn't get published, all of them, until about 1987. But I published several papers with individual things along the way.

JC: And this is going to sound like a strange question, but why did you run this short course? Why did you make this material available to graduate students?

RES: (Laughs). Because I'm a ham teacher. I'm a teacher. There's no doubt about it. And I will teach at the drop of a hat, and if someone needs to be taught, I do not necessarily ask for the University of Minnesota to receive tuition for it. I would usually put the students up in my study. On a day bed.

JC: Good accommodations.

RES: Yeah.

JC: Now, that raises an interesting question.

RES: It's a standard tradition in vertebrate paleontology. Malcolm would put me up every time I went to New York. And it is a tradition in vertebrate paleontology. You take care of visiting students.

JC: I've always understood that the field is so small that everybody knows everybody else.

RES: Everybody knows everybody else. The amazing thing was the number of vertebrate paleontologists who show up in Lusk, Wyoming on one Saturday night for a beer bash at a strip joint, and we had a rump meeting of the Society of Vertebrate Paleontology in which I..., well, it got very drunk that night and I am told I was making comments such as you could put dynamite in Bug Creek Anthills and catch the fossils raining out of the air in catchers' mitts and you'd still get the fossils.

JC: (Laughs) You've had different experiences with Simpson and Jepsen.

RES: I never had any interaction with Simpson at all because by that time, Simpson had retired from the American Museum, had taken a position at Harvard. Harvard didn't really have many Paleocene multi's at all and so I only met Simpson a couple of times at conventions despite the fact he was probably my major mentor other than Olson, Weller and Lowenstam. And to a lesser extent, Charley Bell.

JC: Right. So when you went to the American Museum, you'd stay with Malcolm?

RES: I stayed with Malcolm, went in with Malcolm, worked on the American Museum specimens, drew everything at hand. I would take my micrometer occular with me and use whatever microscope they had.

JC: And then when you went to Princeton to look at the collection that Jepsen had, you had some interesting experiences.

RES: Yes. Jepsen was a troubled man. He did not get along well with anybody. He divorced his wife or his wife divorced him and I don't know if he ever had any children. I never met any. They didn't have anything to do with him, I think, if he did. He was in part of the old autocratic school, like Osborn, who ran things. He was, after all, the William Berryman Scott Professor of Paleontology at Princeton University. And he had his own research funds, the Scott Fund, that he didn't have to account to anybody for. He had hired Don Baird, who had been one of Al Romer's students, to work as a subordinate with him. Don did his own research, but he also had to do whatever Jep told him to do. And he abused his students to the point where he never had many graduate students. He had undergraduates who would go along on his field trips with him. He would have a senior, usually non-degree paleontologist running the field crew and then he would have undergraduates from Princeton and a number of other places for a wild summer of collecting. He abused them mercilessly, and he abused his colleagues in the profession to a lesser degree.

JC: What do you mean by that?

RES: The junior colleagues who wanted to work on problems that had Princeton specimens were not always shown the specimens they needed to be seen. Jepsen had a safe in his office, and he kept his most valuable and rarest specimens there. I had him buffaloed, in part because just before Leigh and Bill and I were introduced to Bug Creek Ant Hills and then exploded in this wealth of localities, he had had Don Baird and his summer's crew out working in the valley of Bug Creek. I don't know why he picked Bug Creek, but he was very upset with the lack of progress they were making and he jerked the party out of the field before they had a chance to explore it, and before they had a chance to find Bug Creek Anthills.

[Here there is a disruption in the tape. The discussion continues:] And in very large part, Jepsen's major important 1940 paper on multituberculates is a matter of ripping to shreds the work that Simpson had done on the comparable multituberculates of the Crazy Mountain Field in 1937. And he did not say so in so many words, but from that point on, you can see this tension between Simpson and Jepsen.

JC: (Laughs)

RES: He was a very good paleontologist, but he was just a very difficult man to get along with. I liked him. I could appreciate his good points and I had him buffaloed enough so that I didn't have to worry about the bad points. In fact I got to see all the treasures that the whispering network of graduate and young professionals in the field had been talking amongst them. Hey, what did you see? I saw this. I got to see every single one of them and, in fact, even got to photograph some of them. In large part because I think I blew him out of the water with Bug Creek material.

JC: I'm sure you have.

RES: There are three paleontologists who provided the foundations that I had for my major work in the Paleocene. They were Simpson, Patterson, and Jepsen. And, of course, I should add William Diller Matthew whose monumental work on the San Juan Basin Paleocene was organized by his son-in-law, Ned Colbert, and to a lesser extent, Simpson. And they provided the foundation and I was able to synthesize everything from all of those on the basis of the conversations that Leigh and I had had in which I worked out with him the interrelationships of all of these evolving species of ungulates and what I worked out in primates and I what I had worked out in multituberculates.

JC: So even by the mid-sixties, you were publishing articles in very high profile journals, *Science*, for example.

RES: Oh yes. The *Science* issue, the first one, the major paper on Cretaceous mammals from Montana, had a strange history. By this time in 1964, I had had roughly $20,000 NSF Grant which supported two years of field work and massive collections, massive stratigraphic work. I had bounced around all the western United States and examined all of the major Paleocene localities. I had worked out the multituberculate phylogeny in considerable detail. We had the ungulate phylogeny going.

[There is another disruption in the tape. The discussion continues:] Now, we wrote this paper and there were major problems. The major problems were several. One of them was that I should not be given an NSF Grant to do this work. Some senior professional should do it instead. And, in particular, Lew Gazin, of the U.S. National Museum.

JC: Did he suggest that or did other people suggest that?

RES: He suggested that.

JC: I see. What was the justification for saying such a thing?

RES: Oh... because I'm not a vertebrate paleontologist.

JC: You're an invertebrate person and...

RES: Who has lucked into this thing and I'm not really qualified to do the job.

JC: And because it's such a big deal, you need to have a real person come in and do it basically, was that?

RES: Right. Exactly. You have the point. And the same kind of blocking was taking place about this paper we submitted to science. It was clear, this was a major blockbuster paper. Science and Nature were the only places that it ought to be done. And the paper, in fact, covers a very large number of topics. It talks about the detailed biostratigraphy, the detailed evolutionary trends of the ungulates, the replacement of dinosaurs by mammals, the major changes in the classification of multituberculates, the description of major new species of multituberculates from rather spectacular materials including a first attempt at an articulated skeletal drawing of a multituberculate because we had all these spare parts and yet again, there was a road block submitted by some reviewers of the paper. This work ought to be done by some major professional.

Now fortunately, Al Romer was President of AAAS at the time, the nominal owners of the journal *Science*. It's semi-independent but not completely so. And Al put his foot down and said this paper should be accepted and it was. Al, of course, was one of my intellectual grandfathers because Al Romer's first graduate student was Everett Clair (Shorty) Olson of Chicago who was one of my major professors there, one of the three in Chicago who had a major impact in my initial training.

And who gave me my first vertebrate paleontology field work? [JC: It was Olson.] I don't know whether this kind of relationship played any part of it at all, but Al stood up for us and our paper was essentially accepted without change, and published, and it made a big splash. And while that was in print, I found the loose teeth of this earliest primate.

And I had the teeth drawn. At that point, I was in these family difficulties and so Leigh joined me and we wrote the paper, The Earliest Primate, which was a complete joint proposition. I continued to work on the multituberculates and worked out the multituberculate phylogeny and the sequentiation of Paleocene localities in North America the first time this had ever been done. And I ran across a paper, I forget where, in which a Stewart Landry, who was a specialist in modern rodents of some sort, or at least rodents, said there could be no possibility of competition between squirrel-size multituberculates and early rodents, which were mouse sized, despite the fact Jepsen had this major paper on competitive exclusion.

Well, this burned my soul because it was just in error. He had a terribly inadequate understanding of what the size and taxonomic diversity of multituberculates might be, what the degree of ecological overlap between rodents and multituberculates was, he was simply arguing competitive exclusion did not exist. And he did not know what he was talking about. So I wrote a first draft of a paper in which I had the multituberculate phylogeny, quantitative changes in multituberculates and rodents throughout the late Cretaceous Paleocene and early Eocene. Leigh added some more and the paper was ultimately published in *Systematic Zoology*. It was a shell shocker.

JC: This is your 1966 paper: the extinction of multituberculates? Right.

RES: And there has never been any serious doubt that competitive exclusion is involved since. David Krause re-did the paper with exactly the same results some years later. But our's was much more thoroughly documented than Jepsen's.

And so I was extremely pleased when Bob (I can't think of his last name) [Schrock] did the book in which he took the twenty-two best papers in vertebrate paleontology of all time and included our paper as one of them along with Olson's chrono fauna paper and several papers by Cope and Marsh and other notables of the whole history of vertebrate paleontology and here we are.

JC: And clearly that's a big compliment for your work.

RES: Yes, it really is.

JC: You've said that when the paper came out it made a big splash.

RES: It was one of the papers I had for which I had many requests. Another one was one I did for a North American Paleontological Congress that was held at the Royal Ontario Museum -- I think it was the first one.

And I was invited to give a paper and so I proceeded to give a paper, the second paper in the sequentiation of faunas in North American in which I talked about the ecology of the Cretaceous - Tertiary transition, the ecology of dinosaurs, their extinction and the evolution of mammals. And this paper, I got over 800 reprint requests for. I had to keep reprinting it every so often in great galloping batches.

JC: So that's how you know you make a big splash?

RES: That's how you know you make a big splash. When you know you get a lot of reprint requests or you're honored as one of the most significant paleontology papers in all of time, that's a big splash!

JC: When did you start feeling like you were getting accepted as a vertebrate paleontologist?

RES: As of the 1963 session at the American Museum, the week that Kennedy was assassinated when I gave that 40-minute talk. It did not cover all of them, but it did cover a lot.

JC: But you got the sense there that people, not only were they willing to listen, but they were aware you getting the sense that they were looking at you as, yes, okay, here's somebody who knows what he's doing?

RES: Yeah. And ever after, every time I have given a talk before the SVP, no matter how sparse the auditorium is in the paper before, everybody comes charging in for my paper. And the same thing happens at GSA Conventions when I give a paper in paleontology sessions. People come to hear my papers. Its very encouraging to see suddenly the swell, and then after my paper they all go out. One of the things that I developed early on

JC: [the tape pauses owing a telephone call.] We're back after a telephone call. Bob was talking about the SVP meetings and his activity there.

RES: Okay, my presentation always involved very complex graphics. An awful lot of paleontologists simply produce bad slides from typed script or fossil photographs, and the type is never really large enough to really see from anything but the front row. One of the things I've always done is make certain that if I do have to have bad or complicated graphics, I also provide the critical graphics in the form of a single sheet, 8-1/2 x 11 handout printed on one or both sides with all the critical graphics so that they can, in fact, take away the information from my talk.

JC: That's one of your trademarks. When did you start doing that?

RES: Oh, I don't know the exact date. But anytime you have specimens that a lot of people are going to be interested in, the thing to do is to, I think, provide preprints of what it is you're doing, with a copy of the abstract, so that what it refers to and the key slides. Key stratigraphic diagrams, things of this sort.

JC: So they can take that away...

RES: So that they can take away and are not just trying to remember scribbled notes in the dark about the exciting things that happen because I've spent a lot of time on my graphics getting them ready for a presentation. I do my graphics myself. I do them in all sorts of ways. At first they are simply hand drafted with Leroy lettering sets and then usually I would have complicated things in which I would have the equivalent of a four-dimensional diagram displayed with pie diagrams colored to show all sorts of things. That became fruitless so I concentrated on single color ones and at one of the conventions I was introduced to Polagraph. Polaroid makes instant slide material for 35mm cameras.

They are a bit more expensive, but then on the other hand, the processing isn't. And I got a processor and the associated material at one convention, and that convention literally produced a slide in my hotel room in Reno that I presented in a session the next day. And ever after, I do my graphics in part on computers and in part hand drawing, increasingly more on computers since its easy to do on a Macintosh and then print them out and simply photograph them in Polaroid slides. And this way, I can wait until the very last minute before I leave for a convention before I make my slides. It only takes a matter of fifteen minutes to photograph a dozen slides and then a total of five minutes to process them. So it really can be a last minute proposition and still have workable materials.

JC: You're suggesting that the kinds of presentations that you give have had an important role in getting people excited and having people remember what you're doing?

RES: I think so. And I always try and stress all the aspects of paleontology rather than just one. I talk about classification. I talk about phylogeny. I talk about stratigraphy. I talk about paleoecology all the time. They're not separate things and I try and make each talk I use a model presentation that will literally let the graduate students who are watching this thing for the first time see what a good talk ought to be. Never read my papers. I hate read papers. Instead, I get my slides organized and essentially talk from the podium based on the slides in particular order that I have them.

JC: Can you remember in this period while your presenting the K/T mammal material, any serious criticism, any prolonged discussions during your presentations on lets say as speaking?

RES: No. Gazen would sometimes carp afterwards. In one case at the 1962 meeting, I had had an edentulous jaw of *Eucosmodon kuszmauli* and I showed him, and a lot of others, that this incisor and molar teeth and blades fit the lower jaw exactly. And he was then carping to other people, I was making composite specimens. Well this just happened to be a very good edentulous jaw. I had lots of other scraps of jaws that showed all the teeth in the right place and I would just flip these teeth into the jaw. They would fit just beautifully. Yes it was a composite, but it didn't stay a composite.

JC: One of the things I was hoping in this tape series that we could get you to reflect on is what do you see as your legacies in paleontology and other aspects of your life and I wonder if you are to describe the K/T boundary material and think about it in terms of a legacy to paleontology, what would you say?

RES: Well, one of the legacies is you have to be very, very precise in your stratigraphy. This is something that was drummed into me at my Ph.D. oral exam by old John Clark who was a visiting faculty member at the time. I never forgot it and it was stressed later that summer when I spent a week with Charley Bell looking at the geology of Southeastern Minnesota.

JC: That you can't rely on other people's work?

RES: You can't rely on other people's work. You really do have to check it and do it yourself as well. You can trust people but check! The second thing is the notion that rates of evolution are extremely variable and that there is this inter-relationship between extinctions and radiations. And that many of the problems that we have in the geologic record are essentially associated with the very rapid evolution that takes place immediately after an extinction during the recovery phase.

Because many extinction events are catastrophic or semi-catastrophic. The stress on detailed analysis of morphology and on detailed mapping. You really do have to go out and look at the rocks. You can't just look at the critters in the museum. You have to see where they come from.

And see how they're related.

JC: And one of the things I would -- if someone asked me what you like see out of this material is that you've made -- you insist that this problem be a sophisticated problem, that there are many aspects to the K/T boundary that need to be explored and tied together. Would that make sense to you?

RES: It sure would. There is a very large population of geologists who are content to say, oh there is a single reason for the extinction of dinosaurs and its the asteroid collision as that's a gross oversimplification.

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Interview with

Robert E. Sloan

by Joe Cain

Session 7

A series of interviews with Professor Robert E. Sloan regarding his career, the recent history of paleontology, and the life of an American scientist in the second half of the twentieth century.

These interviews were made possible with a generous Mellon postdoctoral fellowship from the Department of History of Science, University of Oklahoma.

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#7 Tape 4, Side 2
13 March 1996

RES: Professor Robert E. Sloan, Department of Geology, University of Minnesota
JC: Dr Joe Cain, interviewer

JC: On this tape, we're going to start talking about Bob's time at the University of Chicago in the late 1940's. Bob began his career there as an undergraduate right after the war and that's what we're going to start talking about.

RES: Actually, my University of Chicago career began in the spring of 1945 while I was still a sophomore in high school, the first year in senior high school, because the high school I went to was not sufficiently large for the number of people who had to go there. So the Freshman class was disbursed to several branches. I did my Freshman year work in high school at Morgan Park and then transferred to Christian Fenger for the sophomore year. When I got there, I had a reputation that had followed me starting with my IQ tests that were given in about the seventh grade in which I was -- as I was later to find out ranked with an IQ of about 152.

JC: But you didn't know this at the time?

RES: I didn't know it at the time. I was just there doing all the normal high school things. I was in ROTC, in the ROTC Band and was also in the concert band, both of which were led by Captain William Burnham who had been a band master in World War I and carried that as his title for the rest of his life. I played the "C" melody saxophone. We played concerts. We played all the football games. We were still playing, marching at championship football games as late as March because Fenger was one of the winning-est football teams in the city. I have been to exactly two football games since my sophomore year in high school. That is not my favorite sport. In the spring of my sophomore year, one of my teachers and I don't know who it was I can't remember, approached me about trying out for entrance and a scholarship to the University of Chicago.

So the two of us from Fenger went to the University of Chicago for a day-long comprehensive examination. It was a model of all of my undergraduate course examinations for the rest of the year. The standard practice was you may have had quizzes, they didn't count. The only thing that mattered was how you did on the day-long comprehensive examination at the end of the term. This examination had a lot of general questions. It gave us some specific paragraphs to read and then asked us to write paragraphs about them. And, as I say, it took a full day. A month or so later, the two of us found out that we had been admitted to the University of Chicago for the fall quarter, and in my case, I was given a scholarship. So there was the inevitable discussion in the family: should Bob go to college now at sixteen, or should he wait? And the deciding factor was that our family was definitely lower middle class. We were always scratching and this was an opportunity to get a fully paid-for college education.

So having gotten the scholarship, I then went through a series of placement examinations. And this was a three or four-day affair, comparable to the later comprehensive examinations and the others. This was essentially to determine what courses you would place in or out of. In the course of this, I placed out of the first two years of Natural Sciences which was an integrated program in all the natural sciences and I went into Natural Sciences III which was biology and, most particularly experimental discussion of the problems of experimental embryology and a few other things, but very heavily based on Speman's developmental anatomy of the frog. I placed out of two of the three years of English and wound up in English III. I took the full mathematic sequence, Mathematics I and Mathematics II. Mathematics I was essentially the basis of postulational systems in which we independently developed postulational mathematics, comparable to postulational geometry but working up as far as the Peoni postulate. It was basically a study of group behavior, in mathematical groups, and a whole series of operations and inverses. At first, one which was not specified, just a general operation that we did not discuss on its inverse and then later, everything we learned there would automatically transpose into the normal arithmetical operations, a whole series of Algebra operations.

RES: I got the Humanities series and the Social Sciences Series. I think I missed Soc. I. I certainly took Humanities I. And this probably has to do with which ones I placed out of depended in large part on what my independent studying by my voracious reading as a sickly kid had prepared me for.

JC: So very strong in the sciences?

RES: Very strong in the sciences. I still was faced with a language. I had had two years of Spanish in high school. I never took another foreign language. And, in effect, by the time I was through with the placement exam, instead of proceeding with the normal four-years of Hutchins' program to a Bachelor of Philosophy degree, I had only one course to take to do the whole thing in three years. And so I chose to do just that.

JC: Let me ask you about the time of your just getting started into that program when you were taking the placement tests and those things. You were sixteen?

RES: I was sixteen or fifteen when I took the initial scholarship exam.

JC: You talk about your parents discussing whether or not you should be in college: what did you think?

RES: I thought it was appropriate. I was really spinning my wheels and wasting my time in high school. I was never being pressed in any subject at all. If I was interested in it, I did well. If I was not interested in it, I did well enough.

JC: What did *you* think about going to college?

RES: Well, I knew I was going to and if they thought I was ready to do it, well then by gum, I was ready.

JC: Okay.

RES: It was as simple as that. Hutchins' program is one that he developed starting about 1935 or 1936 in which extremely bright sophomores were invited to go to the University of Chicago and given an intensive set of broad-based programs. Some of these are truly spectacular, as some classical textbooks were written for them. I'm sure you remember Ralph Buchsbaum's textbook on Zoology that was really written as a major introductory course. And it was a delightful book, very unlike the classical zoology textbook, Parker and Haswell, that I had read and thoroughly absorbed when I was ten. So these were integrated courses. These were not taught by the standard departments. They were taught in a separate part of the University of Chicago called the College. And there were divisions in the College and these divisions, in effect, functioned as departments separate from the Graduate School so there was the Division of Social Sciences, the Division of Physical Sciences, Division of Humanities, and so on. And so instead of getting the traditional kind of instruction that you get say at a standard state university where you pick and choose which introductory science course you're going to get, and you're only going to get that one.

This was a specifically planned program in which the faculty looked for the great ideas in that subject and then presented them. Usually by reading extracts from the original literature, not just textbooks written by some hack textbook writer.

And we would discuss the development of the ideas. This was very definitely part of the Great Books program that I'm sure that all of you have heard about that ran at the University of Chicago from the `30s until the `80s.

In any event, this was the program that I placed out of essentially a year's work and started in at the ground level in the subjects in which I was less prepared and

JC: What did you want to be about that time?

RES: I knew I was going to be a scientist or mathematician of some sort. That was obvious. That had been made very clear to me. I had made that decision while I was still in elementary school, before 1943. This was what I wanted to do. I didn't know what aspect. I had several possibilities I had been thinking about. I had been thinking about...I was fascinated by several things. I was fascinated by non-Euclidean geometry, particularly after I took geometry and realized that this postulational system was not the only one possible with slight variations and definitions and postulates, you would have equally consistent kinds of geometry and we really didn't know which kind the universe really followed. I was much influenced by a series of popular books on science written, among others, by George Gamov. I can't remember their names now, but I had been exposed while I was still in high school and getting library books out of the Roseland Public Library, to the basis of the development of quantum theory and all the peculiar differences from what you normally think of as how the world is put together. The same sort of thing in special relativity so quantum theory, special relativity, nuclear physics, were all and mathematics were all things that I was considering as potential careers.

JC: That would be a time, when you had started your classes, when the cult of the atomic scientists was just going.

RES: The cult of the atomic scientist was definitely going. And I read rather deeply on these subjects.

RES: We lived about eight miles from the campus of the University of Chicago. My dad, by changing his route from the southern side of Chicago roughly 102nd and Wallace near Halstead Street up to his employment at the Chicago Avenue and River Plant of Montgomery Ward & Co., which is a mile-size building, could go right through the University of Chicago. So throughout my college career I would get up early in the morning, my dad and I would drive to the University, he would drop me off usually next to Hutchison's Commons in the Reynolds Club, and I would go in there and read and study in a nice ox bridge type of study with leaded windows and gorgeous leather arm chairs and dark oak tables and leaded glass windowed book cases with books you never touched. They hide them. And I would study there in the Reynolds Club.

JC: That must have been an exciting place.

RES: Oh it was!

JC: Especially for a sixteen-year-old.

RES: Yeah. The most delightful thing about it was that my colleagues were a mixture of extremely bright sixteen-year-old from all over the country, so then I was no longer isolated in a standard Chicago bunch of teenagers, but really bright teenagers from all over the country. Thoroughly seasoned with somewhere between a third and fifty-percent returning World War II veterans of the Army and Navy, and most of them, probably not by accident, turned out to be Army and Navy pilots.

JC: Why not by accident?

RES: Because, in general, although bright people could get ahead in any branch of the service, there were probably more bright people flying than any place else.

JC: Oh, I see.

RES: It took much greater in the way of technological skills and training to be a pilot than it did to be in infantry or a tank offices. And so all of them wound up there.

JC: And so here you are at the University of Chicago with a group of...

RES: A peer group finally of my peers. Instead of being an isolated nerd, always treated like a nerd.

JC: Now, you were in a pack of 'nerds', right?

RES: I was in a pack of nerds. Bright nerds that were interested in everything teenagers were interested, it's the scurry and find as in Ricky-Ticky-Tavy (the mongoose in Kipling's stories).

JC: Now what influence did the GI's have on you in terms of, I guess of wondering did you see them as role models or as older brothers or just as other people?

RES: They were other people. And they formed an important part of the peer group. They provided a very rapid maturation that I would not have had otherwise. I did not miss in any way my junior and senior years of high school. Had I been forced to go through them, I would simply have been marking time and I would not have been faced with the intellectual challenges I was faced at the right time.

JC: But also in terms of what you call rapid maturation...

RES: Mental maturation.

JC: Right. You weren't expected to be an adolescent. You couldn't be a sixteen, seventeen, eighteen-year-old, you were expected to perform like a adults?

RES: Yeah. Oh, they would lean back and snicker when you were being a teenager. But education was a serious business. These people had had their education postponed for as much as four years. And they weren't about to waste any more time. It was time to get about the business of getting themselves into whatever their careers were going to be, and it rubbed off on many of the teenagers. Not all of them, but it certainly did on me.

JC: They also must have had some great stories.

RES: Oh yes, they did. Although most of them did not really talk very much about their war experiences.

JC: Why do you suppose that was so?

RES: I really don't know.

JC: Okay.

RES: I wouldn't care to try and put words in their mouths. But they didn't spend much time talking about their war experiences although they would say something if they were asked, but they would not automatically bring it up. They were here for the same thing I was to go as far as they could as fast as they could. The normal Hutchins Program started kids at the end of their sophomore year in high school at about sixteen and brought them through to a Bachelor of Philosophy degree in four years. There was the inevitable problem of what was the worth of a Bachelor of Philosophy. Some schools considered it a Bachelor's degree. Some schools considered it a touch above the Associate of Arts degree. In my case, I didn't care because I was clearly going on to Graduate School by hook or crook very early on. I just hadn't decided what I was going to be.

JC: Why did you have that clear momentum?

RES: Because even at Chicago, among these bright teenagers and World War II vets, I was still leading the pack. Except now it was a much stronger pack of scholars. So comparing myself to everyone around me, it was very obvious that I should not stop at a Bachelor's degree that I really should go on.

JC: And scientists have Ph.D.'s, so it seemed only natural, isn't that I mean I presume that that played a core part of your thinking that that was just a standard part of the training.

RES: Yeah. I was not yet familiar with the ordinary kinds of degrees that were offered other places. I was immature in many ways, and quite mature in others. And so I came through my first three-quarters and I took my comprehensive examinations in spring for the entire year's work in my four subjects which were mathematics and social sciences, humanities and Natural Sciences III which was the biology.

And since I realized that I could get my Ph.D. if I were to get that language out of the way during the summer, I spent the summer of 1946 taking German. I took an entire year's German in one course. It was a bit much and I didn't do as well in that as I had been doing. In fact, I routinely flunked the quizzes throughout the entire quarter. I did take the final examination of the course and squeaked through with a "D" and was not happy with that at all. So I then spent spare time in fall quarter boning up on German pushing German vocabulary and studying German grammar.

And retook the examination at the end of fall quarter and raised it to a "C" and stopped. This left me with two languages, essentially a collegiate year of German and the equivalent of that in Spanish done in two years in high school. Those wound up being the two languages I used for my language examination for graduate school. And in each case, I studied the examinations before I took the language examinations in graduate school. The examinations for the languages in physical sciences turned out to be usually about readings in the language in non-Euclidean geometry or relativity or quantum physics. So I was already familiar with the subject. I simply set up the special vocabularies for those subjects and learned them cold and passed both the Spanish language examination and the German language examination adequately.

JC: What else did you do in the summers? I remember reading some place that you had spent a summer working at Montgomery Wards. Is that right?

RES: Yes, I did. It was in the customer return department where my mother had worked early on in her work career. My dad had never worked there, but of course he was in the same building. So I simply went down there and worked in customer returns. A very strange thing, I did not do well in that job because I kept getting interested in the stories that the customers would write about why they were returning these things. And some of them were real and some of them were fairy tales designed to replace worn out stuff with good stuff at no expense.

JC: So there was nothing like Martians came down and used it?

RES: No, but this was a sweat shop atmosphere in which you were expected to produce so many cases handled per hour and I never was at a sufficiently high level to satisfy my supervisors so I only did it for one three-month period.

JC: What else did you do on the summers while you were an undergraduate?

RES: Well, I of course joined the National Guard.

JC: Right. And we'll talk about that another time.

RES: Yeah.

JC: When we can get together.

RES: And so I would have the two-week summer encampment. Which was at Camp McCoy, Wisconsin for the first year, Camp Greyling, Michigan for the next two years and then at Camp Ripley, Minnesota. And those tended to occupy a lot of my summers. I don't remember anything else special about those summers except that I spent a lot of time in the summers going to outdoor prone small bore rifle matches in which you shot 22-caliber target rifles weighing from ten to thirteen pounds at distances of fifty yards, fifty meters and one-hundred yards. Under very, very precise conditions. And, of course, I was doing well on that.

JC: Yeah.

RES: But I don't recall that I had other serious employment during the summers.

JC: Okay. Well let's move up to studying graduate school.

RES: Actually, we need to talk about the last year before I got the Ph.B. In order to complete all my degree requirements for the Ph.B., I only needed to take three courses for the whole year. This left me enough tuition money for one additional course, an elective. Anything I wanted. And, of course, as those of you who read my autobiography know, I have been fascinated by fossils starting at age three and just going on and on and on. I practically lived at the Field Museum for many, many weekends during my life. So I thought I would take the introductory geology sequence.

A full year's course of geology, this time from a department rather than the college. And so I took this course. It was taught by Ninian Allen Riley who had just completed his Ph.D. at the University of Chicago. The teaching assistants were Larry Van Vlack and Jerry S. Olson. Jerry Olson was the one I was closer to of the two teaching assistants of the course. Jerry was an ecologist and a geologist and his specific master's thesis which he was working on at the time involved an analysis of what happened in the Indiana Dunes which were very close to Chicago. And most particular, how you could re-establish the vegetation on a sand dune in order to stop a sand dune blowout when every time the wind blew, the seeds would in fact be exposed and blown away.

These blowouts would usually get established when someone unthinkingly set a campfire on top of the sand dune and literally destroyed the core of the grasses leaving only the deep roots that couldn't reproduce more grass. And with the depth of the grasses, there was nothing holding the sand in place and it would simply blow away.

JC: We should pause. We're going to pause for just one second to change the tape.

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Interview with

Robert E. Sloan

by Joe Cain

Session 8

A series of interviews with Professor Robert E. Sloan regarding his career, the recent history of paleontology, and the life of an American scientist in the second half of the twentieth century.

These interviews were made possible with a generous Mellon postdoctoral fellowship from the Department of History of Science, University of Oklahoma.

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#8 Tape 5 - Side 1
March 13, 1996.

JC: We're talking about Bob's last year as an undergraduate at the University of Chicago.

RES: Jerry's thesis was an interesting mixture of ecology and geology. His thesis research was sponsored by some poor soul who had a house that was probably worth $50,000 then and would be a half million dollars today. In the Indiana dunes area, and there was a blowout right in front of his house, so the sand dune was migrating and about to cover his house. He needed help now. So ultimately, Jerry, after working through the ecology of the particular plants that would control the windblown sand elaborated further on a specific curative measure for this. And basically what he did was pick the appropriate seeds, buried them under burlap bags staked out across the dune, and then watered religiously until the seeds germinated and the plants started. By the time the burlap rotted away, the plants were well established and the dune was stabilized.

So, to a certain degree, Jerry, while talking about his research, gave me my first introduction to ecology.

JC: Who was he a student of?

RES: I don't know. Jerry went on to a long teaching career at the University of Tennessee--Oakridge. And he's still there today. He's not much older than I am. I suppose he's retired now, but not by very many years.

JC: And so here is somebody who you're developing a professional and personal relationship with in doing geology and ecology. And you had Jerry for a year, first semester and for your second semester.

RES: First, second and third quarters.

JC: And third?

RES: He was a T.A. for the course all through the year just as Larry was. And they sort of shared the duties, but I spent more time with Jerry than with Larry.

JC: Would it be correct in saying that he made quite an impression on you?

RES: I think he probably did. One of the neat things about this, of course, was that it was not a traditional physical geology/historical geology course. A considerable amount of mineralogical chemistry was indulged in because they talked at length about mineral stability, particularly during metamorphism. They talked about a wide variety of topics. There was a very cursory treatment of historical geology, but in fall quarter, before the storms came, the entire class took a week-long trip by bus to the classic Cincinnati, Ohio area where in a radius of about a hundred miles, there were major classic sections of Ordovician and Silurian rocks.

JC: And that was your first camping experience?

RES: No, it wasn't my first camping experience, but it was the first camping experience with the geology department. I camped before with my folks.

JC: Oh, I see.

RES: This was the first geology field trip in which I went to see truly fossiliferous rocks. All of my previous exposure had been to limited to see gravel pits in central Illinois, which are not the most exciting in terms of what kind of fossils you would find.

And I was struck by these Ordovician marine rocks with the alternation of shale, ultimate source, the Appalachian Mountains or their ancestor the Taconic Mountains, and limestone grown on the sea floor by the marine organisms. And this was fairly close to sea level so that the storm waves would stir up the bottom and concentrate the shells into massive shell beds. And I was suddenly struck by the possibilities of working out ecosystems by looking at these shell beds that had all these critters that were living under the same conditions and that they were different from time to time. I was immediately faced with the physical evidence of evolution because the Ordovician ones were different from the Silurian ones. There was a major extinction in between the two. And by the fact that we were looking at a wide variety of kinds of lithotypes with very, very different kinds of ecologies. I was immediately faced with the notion that different kinds of bottom sediments had different kinds of faunas growing out of them.

Though it was an extremely critical field trip. And long before the year was out, it was very clear that I wasn't going to be a physicist. I wasn't going to be a mathematician. I wasn't going to be a chemist or a plain biologist. I was going to be a geologist. And probably going to become involved in paleontology.

Well, that was the end of the third year and so I immediately entered into graduate school. Well, not really graduate school. I then went through the department for a departmental Bachelor's degree.

JC: It's because your degree in philosophy came out of the college?

RES: Yes. My Bachelor in Philosophy was non-designated college.

And so I spent a year and accumulated enough additional courses in the Geology Department to get my BS or SB as they called it in Chicago in geology. And these courses basically consisted of introductory mineralogy with D. Gerome Fisher.

Whom I enjoyed immensely even though I knew I wasn't going to be a mineralogist. I did all my mineral identification with grains and oils in a micro petrographic microscope--not hand specimen mineralogy. I never really had been very good at hand specimen mineralogy, and a series of other courses. I cannot tell you at this point what order I took the courses in because I haven't looked at my transcript in thirty years. It might useful to do so.

JC: Sure. One of the courses that I think you did take as an undergraduate in this contact was a course of Weller's?

RES: Yes. Right off the bat, I started in paleontology because I wanted to be a paleontologist. Weller had a year-long sequence--no, the first course in the sequence was fall quarter of--well, lets see, that would be 1948--and it was taught by Heinz A. Lowenstam. Heinz had gotten his Ph.D. at the University of Chicago, had worked for the Illinois Geological Survey for some time and was called back to the University of Chicago to teach the Introductory Paleontology course. This was truly spectacular course that thoroughly convinced me that paleontology was what I wanted to do. He talked about all the usual basic principles of paleontology and he talked about these in context of the local Niagaran Silurian rocks which are horrible rocks. They were carbonates which were thoroughly dolomitized shortly after they were deposited in which most of the fine detail of the fossils was destroyed so the fossils frequently are casts or molds. Once in awhile, they would be replaced with silica.

JC: Sure.

RES: Heinz took us on field trips--not just one field trip, but several field trips and encouraged us to do some research of the local rocks. And Richard Bader, who was one of my colleagues, a former Army Air Force pilot was working very specifically on certain paleo ecological problems in the Niagaran as a result of this. He got into this during this introductory course. And I went along with him. So I was exposed to a little bit of stratigraphy, a considerable amount of collecting. Dick Bader was making compass rose analyses of nautiloid shell orientations at different levels in the Niagaran in the areas that we were looking at. Heinz pointed out the main points of his thesis. There was not one middle Silurian fauna in the Chicago area, but two. There was the big reef fauna which consisted of all the organisms that lived on massive skeletal carbonate mounds as big as four or five miles in diameter and as tall as six-hundred feet above the sea floor. And then the quiet water inter reef deposits that were in the deeper water in between these reefs, and pointed out the problems that had come earlier when Ulrich tried to create a strange paleo geography with a mythical group of islands separating the Chicago area from Southern Indiana. The rocks were the same age, but they were totally different faunas, therefore there had to be some sort of barrier to keep them apart. Well, the barrier was simply the depth of deposition of the particular sediments. So in this introductory paleontology course, we got all the usual things in the way of paleontological principles and we got a far greater foundation in paleoecology than was being supplied to anyone else in the country anywhere.

It was limited. We were not faced with any geosynclinal sediments at all because there weren't any fairly close to Chicago where you could go see them. The closest ones would have been in Oklahoma and Arkansas, and there were rocks that Heinz wasn't interested in and never made a trip there.

JC: Sure. It sounds like the first-hand experience to the exposures is important here.

RES: It really was. And it was quite spectacular. Following that, I then got into Weller's advanced invertebrate paleontology courses. This is J. Marvin Weller, the second Weller in the history of the department. His father, Stuart Weller, had been one of the major movers and shakers of invertebrate paleontology in the country from, oh roughly, 1900 until his death in around 1940 or so. Marvin had been one of his students, although he had worked elsewhere as well and had worked independently for many years for the Illinois Geological Survey, had been a President of the Paleontological Society, so it was not sheer nepotism that brought Marvin back. The department was undergoing a major upheaval at the time. And this is discussed at length in the uncitable history of the Department of Geology at the University of Chicago by J Harland Bretz.

J (no period) Harland Bretz. There were bitter machinations in the faculty. The department head at the time was Walter Newhouse who had been brought in because he had a major reputation as an economic geologist and was extremely neurotic and really did not pay much attention to how the department was going, and would make arbitrary decisions and, in general, kept the department in a continuous boil for ten years. He wanted to produce a stress on igneous and metamorphic processes, reduce the emphasis on sedimentary geology and paleontology, and the department was divided into several armed camps. Departments in such conditions are not always happy places to be. Fortunately, it didn't affect me very much. Although it did affect Marvin. It did affect Heintz. Heintz did not get tenure at the University of Chicago and it was the most inopportune tenure decision that department ever made. He then went off to Cal-Tech and became a biogeochemist and totally abandoned his magnificent work on Silurian reef paleoecology which was cutting edge research.

Nobody--he never came back to it and to this day, Lowenstam's work is the premier work in the Niagara and Chicago area. There have been some things following them. A fellow named Ingells [??] at Northwestern did a spectacular job on Thornton Reef at one stage of the reef Development and some others have been in and done-- Don Micalik is now working on the problem, but what was really needed was a set of--somebody on site who was interested in these things and actively kept tract of what was happening in this monster quarry called Thornton Quarry in the monster Thornton Reef. A tremendous amount of ecological information about the development of Silurian Reefs was lost forever simply because there was no one around at Chicago who was interested in this project once Heintz left.

If Heintz had been given tenure, the result would have been a tremendous advance in Paleozoic, particularly Silurian, ecology.

JC: Do you think your career would have changed if he had been there?

RES: No, it wouldn't because I had moved on to other things after I left Heintz. Weller's advanced invertebrate paleontology course, was a course in which Weller covered in a very modern, very precise, up-to-date way from original research in all the groups of marine organisms, the evolutionary history of marine organisms. It was a very different course from those offered by anybody else. It was mainly morphology, anatomy and evolution, not much paleoecology. Weller also was forced by the department to teach the stratigraphy course. That was not as satisfactory, and I took it of course.

JC: That was not his strength?

RES: That was what he did well. He did not teach it well.

JC: I see.

RES: And so I had no real hands-on training in stratigraphy. I had to develop it for myself as I went along. Marvin's course in stratigraphy was basically the historical development of stratigraphic nomenclature, and not much more. He didn't enjoy teaching the course. It was a course he was forced to teach by departmental edict.

And he didn't put his best efforts into it.

JC: I see. But the course that he did teach well--

RES: The course that he did teach well was this course in morphology and evolution of the invertebrates. And Marvin's course was far in advance of the textbooks in invertebrate paleontology that were available at the time. The textbooks that I bought for the purpose was the small original Shrock, or Twenhofel and Shrock, and it was not until I nearly completed my work that adequate modern textbooks had come along. These were the revision of Twenhofel and Shrock, now called Shrock and Twenhofel, and Moore, Lalicker, and Fischer. Moore, Lalicker, and Fischer basically came closest to being the course that Marvin gave me. Ultimately, Marvin did write an invertebrate paleontology text based on this thing, but he published it after he retired and by that time, the *Treatise of Invertebrate Paleontology* had advanced so far that it was obsolete when it was printed.

And so it never had much usage. I have a copy somewhere downstairs, and its delightful to look at because I can hear Marvin talking about these things as I read it. Marvin gave me the appreciation for a detailed analysis of morphology and how it changed with time which was not something I had gotten from Heitz. Heintz, because of his stress on paleoecology was basically looking at big picture and not really looking at precise morphology.

So the two courses complimented each other very well.

JC: Can you say something about Marvin's teaching technique?

RES: Marvin's teaching techniques were relatively simple. He had a large number of white poster-card cards, about 8-1/2 x 11 on which he had carefully drawn original illustrations documenting all of the things that he was talking about. These were line drawings. He actually made photographic prints of these things available for people who wanted them. Unfortunately, I didn't buy a set. Bob Miller did. I looked at his set. But basically he would put a half a dozen or so of these cards upon a poster board in the front of the paleo lecture room, each lecture, and they'd stay up for awhile. And you were expected to look at these things and maybe make copies of them--some of them.

JC: Hand draw them yourself?

RES: Yes. Drawing in the department was stressed by Lowenstam, by Weller, and by my structural geologist, who was Robert Balk.

Robert Balk had a major influence on me, not only through giving me all the structural geology that I ever had, but also by stressing the point that you increase your skills of observation by drawing. He would give us a polished slab of igneous or metamorphic rock or sedimentary rock, and require us to make a drawing of it indicating by different patterns exactly which mineral was where. And he was not happy with crude illustrations. He would also have us draw what we would see in a microscope field.

JC: You've incorporated that into your own teaching.

RES: Oh yes, I certainly have. Sketching was one of the crafts that I learned as a kid. I taught myself perspective long before I left elementary school.

And I, in the process of making models of airplanes and cars, boats, I learned to interpret three-view plans, learned the orthographic projection and learned perspective so while I was utterly bored out of my skull, in late elementary school and in high school, I would spend a lot of time drawing things. And I found if some of these drawings, I would draw perspective sketches of the favorite allied war planes of the time. My favorite was probably the Curtis P 40, in all of its various versions.

JC: In this year while you were pursuing your B.S., it sounds like you were introduced to a lot of the people who would in your graduate time there, be important.

RES: Yes. In fact, the boundary between the Bachelor's degree and the graduate training did not exist.

I got my degrees at the University of Chicago more than anything else based on the state of the Korean War. I had joined the National Guard early on just before it was first started and the draft was reinstituted after World War II. I was bound and determined that if I was going to have to go into service, I would go into service with some training rather than depending on a quick basic training and then being sent directly to the front lines which was happening to a lot of draftees at the time. So I joined a local National Guard unit close to the University of Chicago which meant that it was still relatively easy for me to get there from the University of Chicago and then take a streetcar ride home.

JC: So the question of why you--

RES: When I got my degrees was essentially determined by whenever the Korean War hotted up.

JC: Now when you were in that transition between undergraduate and graduate, did you ever consider going somewhere else?

RES: No. There was never any doubt. This would be the place where I would continue. I had started in Chicago, I was going to stay there. It was convenient for me to live at home and to ride to campus with my dad. He didn't mind driving me to campus in the morning and picking me up at 5:00 or 5:30 or so on the way home.

It was only a minor change in route to his state of employment so it was very clearly a very reasonable thing to do from any point of view.

In that last year as an undergraduate, I registered for four courses a quarter. I also joined a fraternity. The fraternity took far more time than it should have, and so each quarter I dropped a course in time so that it would not count against me. This was the only quarter or the only school year in which my parents had to pay my tuition which was a relief for them.

My brother had a much more difficult time with his education because he had to work all the time. He too wound up living at home. He went to DeVry which was a trade--an electronics trade school for awhile. He's bright. He wasn't quite as bright as I was. And essentially got no help other than just the family resources. Despite having dropped a course every quarter all year long, I finished the quarter with a 4.0 average and, of course, made the Dean's list, which I've always thought was utterly ridiculous under the circumstances. But I'm there on the Dean list for that year.

JC: What possessed you to join a fraternity?

RES: Some of my--one of my colleagues, David Krinsley--was already in this fraternity and I was rushed. Another student in the department joined the fraternity at the same time or slightly earlier, and so they thought I was a plum. It took a lot of time. Long before the year ended, I had decided that this kind of raucous partying behavior was not doing me any good, that the expenses of being in a fraternity were far greater than any benefits that I was getting out of it, and so essentially I went inactive after that year. I had to be active the first year.

JC: What was the typical--well, what kinds of things did you do in that fraternity for this behavior?

RES: I spent a lot of time paying my fraternity bill by doing maintenance work on the fraternity house. I do recall one problem. There was one time when I cut the oil line from the oil tank to the furnace and the heat went off. It was a major mess in the basement. We had to call professionals in, but I did a fair amount of minor carpentry type maintenance work around the building which was old and dilapidated.

And as a group, did the fraternity do the standard sort of pranks that--not that the pranks were that serious--it was more just a party atmosphere. They had a party every weekend. I stayed at the fraternity house once in awhile, but on the whole it was not a useful way to become a professional anything.

JC: Do you think it was an important year for you as a person growing up?

RES: Not really. I could have done it equally well without it.

JC: We should pause right here and change the tape.

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Interview with

Robert E. Sloan

by Joe Cain

Session 9

A series of interviews with Professor Robert E. Sloan regarding his career, the recent history of paleontology, and the life of an American scientist in the second half of the twentieth century.

These interviews were made possible with a generous Mellon postdoctoral fellowship from the Department of History of Science, University of Oklahoma.

#9 Tape 5 - Side 2
March 13, 1996

RES: Professor Robert E. Sloan, Department of Geology, University of Minnesota
JC: Dr Joe Cain, interviewer

JC: It's a little after 3:30 and we're in Bob Sloan's Winona house. We're talking about Bob's tour at the University of Chicago, and I wanted to have Bob start talking about his career in graduate school.

RES: As I said earlier, the boundary between the Bachelor's degree and true graduate work is not really recognizable. I just continued taking courses in the department and filling out my general background in all aspects of geology. I was clearly going to be a paleontologist so I took every single paleontology course that was offered as well as Francis Pettijohn's sedimentary petrology course which was far more useful than Weller's stratigraphy course. And--

JC: More useful in the sense that it was better organized?

RES: It was better organized and it turned out to be a much more comprehensive treatment of the subject so that by the time that I had completed this course, I had been through the entire corpus of what Pettijohn later published as his major textbook *Sedimentary Petrology* in which he talked and wrote at length about all the different kinds of sedimentary environments and the kinds of sedimentary rocks that are deposited in them and how they differ so that you could recognize sedimentary environments from looking at the rocks themselves. It was a spectacularly good course. Again, Pettijohn was one of the people who gave up in disgust and left the department before I got my Ph.D.

JC: Because of the political environment?

RES: Because of the political machinations. Again, these are thoroughly discussed in the published, but uncite-able, history of the Geology Department at the University of Chicago.

By this time, I was working part-time in the Library in Rosenwald Hall. Rosenwald Hall served as the housing for both the Department of Geology and the Department of Geography. These originally had been closely related departments with shared faculty, but over the years they had separated and the general feeling of the geologists was that geography was the study of the irrelevant by the incompetent. Actually, it was considerably better than that. Geography at the University of Chicago was a truly prime department and it was one of my colleague's from Geography who ultimately wound up being the best Dean that the College of Liberal Arts at the University of Minnesota had for many years, and this is Fred Lukerman. Fred never did get his Ph.D. from the University of Chicago, so he was not Doctor Lukerman. Nonetheless, he ran the finest Geography Department in the country and went on to be the finest Dean that CLA had ever had, in my whole career. The Library was joint geology and geography. I became very familiar with that library, and for awhile in fact, was engaged to one of the librarians there who was thirteen years older than I. That did not ultimately work out, but I did work at length in that library shelving books, making money to help support my graduate career and undergraduate career because I was actually doing it in my last year of undergraduate work.

But I was exceedingly familiar with that Library and, of course, was thoroughly involved in it. Earlier, before I was in the Department of Geology, I spent my time mostly in Harper Library, which was the general library and since I had a lot of time to kill between classes and after classes when I wasn't rifle shooting or working for the Athletic Department or running the range, I would read in Harper Library. And in the reading room, they, of course, had Britannica.

I spent a lot of time in my undergraduate career before I went into geology simply reading articles on a wide variety of topics in Britannica.

Great editions of Britannica--so much so, that in 1956 or thereabouts while we were living at 984 St Paul Ave. in St. Paul, we got an offer to buy at discount price down to what the price really was, a copy of the Encyclopedia Americana. I didn't want the Americana, I wanted the Britannica. So I called up Britannica to see if they would make us a similar offer. They were happy to do so and so we took the 1955 Edition of Britannica and ten year's worth of the Annuals starting in 1956. We had the option of getting a bookcase to hold Britannica as a premium or the two volume dictionary that goes with Britannica, and we chose the dictionary. I could make my own bookcase.

And I continue to use Britannica to this day. I personally prefer this edition of Britannica to the new ones that came along starting about 1965 or so in which they greatly changed the structure of the thing and horror of horrors substituted one of my graduate students for writing encyclopedia articles rather than using the classic articles of W.K. Gregory.

JC: (Laughs) So your days in the early part of your graduate program were pretty packed between--

RES: Yes. Yes. The one thing that happened was in the end of that first year of study in the Geology Department, a note came up on the bulletin board to the effect that there would be a thousand-dollar fellowship. The Chicago Museum of Natural History Fellowship available for graduate students, nothing further was specified, but it was suggested there might be some need to work at the Field Museum in return for the Fellowship. Well, I applied for the Fellowship immediately and come spring, I got it. And this took care of my tuition for the balance of my academic career because it was renewed every year thereafter.

JC: Why do you think you won it?

RES: I may have been the only one who specifically applied for that one. In the long run, they never did ask me to do anything special equal out there, but of course, I continued to spend a lot of time in the Museum. After having been through the year's sequence in paleontology in that first year in the department, the next year I took the three quarter sequence in vertebrate paleozoology for which the nominal professor was Everett Claire Olson, generally known as Shorty to his colleagues and to his students. And this course was team taught by Olson and the Curatorial Staff of the Field Museum. Then they had changed the name from the original Field Museum named for Marshall Field who donated a lot of money to it to start with, to the Chicago Museum of Natural History. And then some years later, it changed back to what everybody still called it, the Field Museum. Its the Field Museum now. But these curators consisted of Robert Dennison, whose graduate work had been at one of the New England universities on Eocene mammals, but whose research had been entirely on early Paleozoic fishes, specifically Devonian ones.

So Dennison taught us the fish part of the course. Ole taught the introduction, Dennison taught the fish, Ole taught the lower tetrapods, the amphibians and Paleozoic reptiles, particularly the mammal-like reptiles. Rider then came in, Ranier Zangerl, who was at that time the curator of fossil reptiles, and taught us the rest of the reptiles, essentially archosaurs and lizards. And the last quarter was taught by Bryan Patterson and involved mammals. So Ole covered essentially Mississippian, Pennsylvanian, Permian and Triassic lower vertebrates including the mammal-like reptiles and Pat took over from the origin of mammals and went on. Pat was an extremely interesting character. I loved all of these men deeply. They were at the absolute peak of their professional careers. They were specialists in their own fields, but they were also extremely broadly trained, so they could cover everything they needed to cover. This was probably the best single course in vertebrate paleontology that was being offered in the country at the time. And Ole always thought that my particular group of students was probably the best group of students that he ever had in his whole career.

My colleagues in the class consisted of Gordon Thurow, who did not continue after the first quarter and who gave a non-descript oral presentation on Paleozoic fishes. I forget the name of the man from zoology. I'd have to look it up. He went on to be a zoologist and not a paleontologist. Ernest Luther Lundelius, who had worked or who had gotten his Bachelor's degree at the University of Texas and who had worked as a student, both before and after this course, as one of Jepsen's field crew, Glenn Jepsen whom you've heard about before and will hear about again, James Richard Beerbower, who is always known as James professionally, but we called him Dick, who was then interested in amphibians but went on to do a very spectacular textbook in general paleontology at about the same time Moore, Lalicker and Fischer came out and it was always a terrible choice--do I use Beerbower which is great on principles and poor on morphology or Moore, Lalicker and Fischer, great on morphology and poor on principles--and I figured it was probably easier for me to talk about the principles myself and let Moore be the reference on morphology than to do the other way around. So that's why I usually use Moore.

JC: What were the classes in the vertebrate class like?

RES: The vertebrate class was organized in a very different way than any of the other classes. They were held down at the Field Museum of Natural History. We all somehow got to the Field Museum. They were held in the third floor on tables. In between the storage cabinets, there was a small blackboard available. We simply all sat around a table. Ole was always sitting in the course whether he was lecturing or not and we simply went through all the distinctive features of each group in a fairly synoptic fashion.

JC: Using specimens?

RES: We looked at specimens. There were major diagrams, phyletic phylograms drawn on the blackboard for us to copy. You have my notes so that you have some feel for what was going on. It was a fairly free-flowing class. Any of us could and did interrupt to clear up a point at the slightest provocation. In addition to Ernie Lundelius and the others that I mentioned, there was Robert Baker, who was totally unrelated to Richard Baker who had been working with Lowenstam, Sloan, Beerbower, Lundelius, and Bater, all went into professional paleontology.

We also had Ralph Gordon Johnson, come to think of it. Ralph was going to be a vertebrate paleontologist and switch to invertebrate, in part because when Weller finally got fed up with the problems in the department and left after I got my Ph.D., the department then hired Ralph Johnson to take Weller's place. And ultimately, Ralph became a head of the department.

JC: Is this when--during this course, was this when you met Bob Miller?

RES: Yes. Bob Miller had gotten his Ph.D. about this time. He had worked extensively with Ole in vertebrate paleontology. He worked as a colleague with Ole developing the book, Morphological Integration, which is a functional anatomy book that was a very, great pioneer in the subject, and also had specialized his statistics under Kruskal. With the disappearance of Pettijohn, someone had to teach sedimentary petrology and Bob Miller was drafted. The department at Chicago has always been very ingrown and this was certainly the case. Ralph Johnson and I shared the invertebrate paleo course the year--the last year of my graduate career and Ralph simply stayed on as the paleontologist teaching the Introductory Paleo and when Marvin finally left for good, Ralph simply became the invertebrate paleontologist at Chicago. He had been through Lowenstam's course with me and had had a fair amount of training in ecology at Chicago. So he worked extensively on paleoecology.

He spent a lot of time at Marine Biological Stations working with modern marine ecology, which is also something that Richard Baker had done.

In any event, there is this blurry line between graduate student and faculty member at Chicago. And a very large number of their faculty have been in house productions. In the long run, I don't think this was good for the department, although somehow they managed to make it work.

JC: What was the--you say in your autobiography that all the graduate students in geology and paleontology hung around together.

RES: Yes. We really did. In fact, all the graduate students in the department hung around together.

JC: In the Geology Department?

RES: Yes. There was an active "Geological Fraternity". It even had Greek letters, Kappa Epsilon Pi (KEP), and KEP sponsored weekly seminars and a subset of KEP mostly consisting of the male graduate students (there were some female graduate students) belonged to a Luncheon Club. This had been founded by Bretz who was in my mentor in many ways, and he found space in the fifth floor of Rosenwald, up in the attic, to put a small kitchen consisting of two two-burner gas hot plates and a sink and a place, cabinets, to store dishes and pots and canned goods. We belonged to the Hyde Park Cooperative, and we had a regular rota. Everybody in the Geology Luncheon Club had to either cook, shop or wash dishes, and this would change from quarter to quarter. But we routinely provided hot meals at noon for members of the Luncheon Club at a minimal cost. We're talking about maybe $2.00 per week.

It would be the equivalent--well, it may have been less than that, come to think of it. It was very inexpensive and it certainly was an obvious thing for me to do.

JC: And it sounds like in addition to feeding you, it was a place where there was considerable camaraderie.

RES: Yes, there really was. As most graduate departments do, there were desks tucked away in the collections in various places in the two buildings, Rosenwall and the adjacent Walker Museum where graduate students could store their books and study. So I had a home, physical home, other than Reynolds Club and the Rifle Range. In fact, even when I was an undergraduate and doing the lab work for William J. Plumley's thesis on River Transport of Cobbles involving tumbling barrels, I had a desk in the basement next to the tumbling barrel where I could study. That result was that there was a very close relationship in the department. At least once a year, the department would go on a Tri-State field trip. This was a loose amalgamation of the Geology Departments of the Schools in Iowa, Illinois and Wisconsin. Minnesota was conspicuously omitted for reasons I found out later. But at least once a year, there would be a big Tri-State field conference in which all these schools would pile into cars and buses and go off and meet somewhere and go on a three day field trip.

And the host institution would come up with a guidebook describing the geology and the region and lead us through the observations. So there was a very--I had a home. I had always had a home at the University because I was down there twelve hours a day, but now I really had a home.

JC: And did you get the sense that you were moving through the program together?

RES: Yes, we really were and nobody was paying much attention. There were several of us in the department who had come up through the college and we were treated exactly the same even though we were much younger than the veterans who composed the bulk of the Graduate Program.

JC: And did you find yourself helping out each other out on research projects?

RES: Oh, routinely we did that. And, of course, we studied together. When Dick Bader needed some help in the field measuring orientations and stuff, we all went off and measured several [inaudible portion of the tape]

RES: We would all go on field trips together. I can recall three of us at the end of our first year sequence in geology while I was still a real undergraduate, hiking ourselves off to the Blue Mounds just south of Madison, Wisconsin and seeing for ourselves on a three-day camping field trip all of the things that Bretz had described about this magnificent cave, Blue Cave of the Mounds, and the local stratigraphy and paleontology and we turned a report in on the subject. I had taken a field course in geology at Caribou with about twenty of us. Instead of the usual four-week course this year, it was abbreviated as a two-week course. But yes, we were all in the swim together.

And some of us were petrologists and some of us were sedimentary people and some of us were paleontologists, but we were all in the swim together. The women were excluded from membership in the Luncheon Club because of Bretz's tradition, even though he had retired, he was still around.

And eventually they broke into the Luncheon Club as was appropriate.

JC: Now one of the things I wanted to ask you about is I don't see it is as all inevitable that you become an academic paleontologist. From what I see early in your undergraduate career, you could have become a physicist or a mathematician or other things.

RES: Yes. And later, I could have become an industrial paleontologist working for the petroleum industry. In fact, when I was involved in research and paleontology for the first time in 1950, I went in to see Marvin about a research project--I talked about it yesterday.

JC: Right.

RES: And, among other things, Marvin suggested it was time for me to join a Society. And so I joined, since he belonged to it the Society of Economic Paleontologists and Mineralogists. This was--the Journal of Paleontology at that time and for many years after, copyright was held by SEPM, but it was a joint publication of Paleontological Society and SEPM. SEPM people were for the most part micro-paleontologists working in the oil industry or sedimentary petrologists also working in the oil industry. And there were two--Journal of Sedimentology and the Journal of Paleontology. And since Marvin had come from an industrial paleontology background, he pointed me in the SEPM--

I didn't know the difference. I just did it. And so that gave me my subscription to the Journal of Paleontology which may also have been the reason that I joined it.

JC: So why did you become an academic paleontologist and not industrial paleontologist.

RES: :Because right from the word go, I was a teacher. Even in elementary school, although I was a nerd to everybody else, when they had problems, I was their nerd. I was the person they came to to see why things went. And I was teaching at some level from the fifth and sixth grade up.

JC: And you had begun to formally teach as a graduate student?

RES: Yes, I had begun to formally teach as a graduate student, but it was quite apparent on that two-week field course, for example, that I was the person who was going to provide explanations of why we were doing what we were doing to the bystander who is asking, "what the hell are you doing"? So it was quite clear that my metier was in teaching in some way.

And besides, I didn't really want to be an academic bear, an industrial paleontologist, I wanted to work on these exciting things that I had read about in Simpson and Roe [1939] and in *Tempo and Mode*.

And in all the rest of the new systematic books that I had carefully gone through during the year that I was taking the vertebrate paleontology course from Ole and Dennison and Zangerl and Pat. The next year, the same group continued with only minor changes in the only comparative anatomy course I ever took which was cranial morphology and this was taught by Dwight Davis and Pat and Ole.

JC: Right.

RES: And so that essentially brought me up to the end of my formal course work.

JC: And by that time, you had decided--

RES: Oh, I had decided I was going to be an academic paleontologist.

I really couldn't decide whether I wanted to work in invertebrates or invertebrates and most of the examples that I had seen in my reading had been examples created by vertebrate paleontologists. There hadn't been any ground-breaking examples done in invertebrate paleontology since Alpheus Hyatt in the 1890's.

JC: So why then did you choose a research topic that was--

RES: So I chose to move into this direction where I could make a bigger splash. If I can find examples of these things in invertebrate paleontology.

JC: I see. So, did you get the sense then that vertebrate work was in some sense--

RES: Vertebrate sense was theoretically far more advanced than the invertebrate. There was a foundation for what was going on.

Vertebrate paleontologists routinely did functional anatomy, paleoecology, the exciting kinds of phylogenetic analysis.

What they normally didn't do was stratigraphy. Very few vertebrate paleontologists did anything more than the most cursory kind of stratigraphy.

JC: Right.

RES: And I thought that I would have a very exciting time if I were to do the same kinds of things vertebrate paleontologists were doing, but in invertebrates.

JC: Because your sense was that those are wide open.

RES: That was an absolutely wide open area. So that's why I chose to do it. And so I was looking around in 1951 for a potential thesis topic.

JC: Let me just change the tape here real quick.

[end of tape side]

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Interview with

Robert E. Sloan

by Joe Cain

Session 10

A series of interviews with Professor Robert E. Sloan regarding his career, the recent history of paleontology, and the life of an American scientist in the second half of the twentieth century.

These interviews were made possible with a generous Mellon postdoctoral fellowship from the Department of History of Science, University of Oklahoma.

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#10 Tape 6 - Side 1
March 13, 1996

RES: Professor Robert E. Sloan, Department of Geology, University of Minnesota
JC: Dr Joe Cain, interviewer

JC: Bob and I are talking about his thesis work for his doctoral dissertation.

RES: Ole routinely took off during spring quarter to go down to Texas to collect his fossils. And the prime reason is Ole worked in a continuation of what his professor, Romer, had started in the Texas red beds. Well, Texas is not habitable outside in the summer. So Ole would always do his field work during spring quarter. And in the spring quarter of 1951, Ole went on a six-week trip to Texas.

JC: And he would take his class?

RES: And he would take a selected number of students, depending on how much money he had available.

As a result, I have talked about this to an extent in the autobiography, and Ole has talked about it also in his autobiography, *The Other Side Of The Medal*. And I had been working, of course, in Pennsylvania in marine shales, and particularly on the snail *Glabrocingulum* at length. This ultimately became a publication which was the equivalent of my master's thesis, although it was done long later. And I was familiar with a large number of collections that had been made in North Central Texas, in and around Palo Pinto County.You have the complete middle and late part of the Pennsylvanian preserved in this area. There is a large amount of relief because of the sea level changes of the Pleistocene, major canyons had been carved across the region. The exposures were great on the walls of these canyons. They have been mapped very thoroughly by Frederick J. Plummer, who was both an industrial paleontologist and stratigrapher and actively working for the Texas Bureau of Economic Geology, which was the equivalent of the Texas Geological Survey. We were collecting vertebrate fossils two counties, northwest, in the early Permian. And so we took off for a weekend, and I drove down to Palo Pinto County. I had a county highway map. There were no topographic maps available. I didn't even have my copy of Plummer's geologic map of the county available. And we simply drove into the region, found some spectacular exposures of Pennsylvanian marine shales on the walls of these road-cuts and canyons, in this heavily dissected region, and in a period of several days over the weekend made extensive collections.

JC: Bob, here's your field notes taken from 1951 and `52 in this region that he's describing.

RES: I think it must have only been a day or two. I may have collected from about seven localities. I precisely located them geographically on the county highway map and which had actually been produced by a local gas station for the use of fishermen and hunters, and carefully recorded the location by using the speedometer on my car. So these represented the first collections of what were to become my Ph.D. thesis.

JC: While you were making these collections in 1951, were you thinking that this was going to be a dissertation or were you--?

RES: I was thinking I might just be able, particularly after I saw this stuff, to develop a dissertation out of it.

JC: I see. So you were looking for a topic.

RES: I was looking for a topic.

JC: You were exploring this area--

RES: Yes.

JC: --this particularly collections are convenient because you are already in Texas--

RES: Yes. Yes.

JC: But at the same time, your scouting for something anyway.

RES: I was scouting for something that I could turn into a thesis topic. I had seen collections from many of these localities before that were in the Walker Museum, now in the Field Museum. And we simply made as clean a sweep of the outcrop as we could literally getting down in the traditional belly-on-the-ground, eyes ten inches from the ground, and picking up every scrap of fossil. I had seen enough of this county and the surrounding counties to see that I should be able to develop a thesis. The question now was how to go about doing it. I had read an interesting paper by Maxim K. Elias, who was a real renegade, and wild man in paleontology and in evolutionary theory. Joe met him more as in evolutionary theory. I met him more as an invertebrate paleontologist.

One of the things that Max pointed out, in a single diagram in one of his papers, and I'm sure you can find the citation to it in the bibliography, was a single illustration showing an inferred spectrum of environments of deposition across the submerged continental shelf offshore of the coastal plain of the Appalachian Mountain system which is now buried under the present Texas coastal plain. In this diagram, Max showed, on the basis of many, many observations, a progressive change in the character of the fauna in which the inshore environments were dominated by clams and snails and the farther off-shore you went, the more brachiopods and bryozoans and corals and crinoids there might be. And looking at the wealth of materials that I made on this first field trip, I then decided that an appropriate thesis topic would be to test this hypothesis of Max Elias and see that if there was a change in character of the faunas as you go from more molluscan to more brachiopod, crinoid and coral, and if particular species were present in particular places and if there was any affect on relative maximum size and a whole series of other variables in marine ecology. By this time, I had already been exposed by Lowenstam and by Dick Bader to the idea that one of the most significant gradients in faunal composition among invertebrate marine species, was related to depth which is a composite of on-shore, off-shore and also amount of photo synthesis and amount of sediment agitation. And further more, that there would be a corresponding association with sediment type, and Dick Bader on the basis of his studies at Marin

[The tape is inaudible here. It continues:]

as many outcrops as I could, identifying their stratigraphic position from Plummer's very excellent geologic map and description of the geology of the county complete with a geologic column, and seeing if within individual units, the more on-shore eastward, south, south eastward, towards the Appalachian Mountains under the present Texas coastal plain, would be molluscan rich and those more northwestward in the same stratigraphic unit would, in fact, be more brachiopod rich. And I would be doing this not just as a single stratigraphic unit, but rather in this same environment which repeats itself over and over again in these Pennsylvanian rocks in this area. It was great set of exposures, a great set of rocks.

It was doable once I had the theoretical mechanism for it, and the biggest problem was getting large enough collections. And so my field experience basically consisted of identifying stratigraphic position from the preexisting works and I didn't have to mess with that, making very large collections from the precise localities and then pooling the data for a single stratigraphic unit to see whether the eastern ones were more molluscan than the western ones at the same stratigraphic unit were more brachiopod -coral- crinoid rich and they were. Many years later, officially my thesis advisor was Marvin Weller. In 1951, I had the one day in April and then several weeks later in the summer after National Guard encampment before classes began again. And that got me fairly large collections. And then in the academic year 1951-1952, Weller was utterly fed up with the department and took a position as--in the Philippines, acting as an Advisor for the Philippines under the sponsorship of the U.S. Geological Survey, which meant that in my entire final year of my thesis, I did not have an invertebrate advisor.

By that time I didn't need one. I was an invertebrate paleontologist. I was at the level where many graduate students go away and get hired as an instructor to finish their degree. I still had my fellowship. I was still basically living at home so that was clearly what I ought to be doing. So in the fall of 1952, I made another collection. Marvin wasn't there and I wound up in the academic year 1952 through spring of 1953 finishing my thesis. It was nearly finished, very clearly essentially done except for the writing by January when I got hired.

JC: At Minnesota?

RES: At Minnesota. And I have an extensive documentation of that in the autobiography.

JC: Now during the time you were working on your thesis, who were you interacting with, say on a daily basis?

RES: On a daily basis, I was interacting with Richard Konizesky who was Ole's preparator of fossils and was also a graduate student, a colleague of mine with Ole, with John Clark who was a visiting professor with an extremely checkered background. He had been working for the OSS in China during the war in occupied China and did a major reconnaissance solo throughout China.

Which I described a bit of in the autobiography. And there are lots more stories to tell about John Clark, and unfortunately I can't remember the details of all of them.

JC: We'll get them down at some point.

RES: Yeah.

JC: So how was you interacting with Clark in that period?

RES: We would simply talk about all of the various aspects of research that each of us were doing. By this time, I had met Sal. Sal was introduced to the department along with Judith Weise who later married Lundelius, became essentially the female members of the Department of--the Walker Museum paleontology coffee clutch. And so the people that we interacted with every day for coffee, were Bob Miller, who was still working into sedimentary petrology to replace Pettijohn; Ole, Beerbower, Lundelius, Ralph Johnson and Konizeski.There were not any other invertebrate paleontologists around except Keith Chave. Keith Chave was a maverick kind of paleontologist who was following a direction that Lowenstam had established just before he left, and that was working on the biogeochemistry of shells. Keith ultimately went on to have a career in the Department of Oceanography at the University of Hawaii.But Keith was not particularly sociable with the rest of the paleontologists. He had been crippled by polio. His left leg, as I recall, was withered. He limped around. He was always prickly and even though the two of us were clearly the brightest of the young graduate students in the department, I could never get close to Keith. He would not get close to people.

JC: Now during this period, did you feel comfortable bouncing your ideas off on the people in the Walker or did you keep your ideas for your research to yourself?

RES: Oh no. We were continually talking over our research. We were always--it was a gee whiz, look what I found now!That was the feeling which everybody was poking their nose into everything that was going on. We all would lean over Konizeski's shoulder as he was preparing fossils that Ole had collected in the Texas Permian or even some that Romer had collected in Africa--South Africa before he left and that had been left around unprepared.

JC: And what about--since your work was in invertebrates, did people take the same amount of interest in yours?

RES: Yeah. They really did. This was--it was quite clear that I was a paleontologist who happened to work in invertebrates. Ole, I think, was a little hurt that I wasn't going directly into vertebrate paleontology, but in the long run, he took it very well. He served as my thesis advisor that last year officially and the committee consisted of Bob Miller and Ole as chair, and John Clark, and I forget who else.

JC: And did you also interact with people at the Field Museum about your research?

RES: Yes, I did. I had spent a fair amount of time with Eugene S. Richardson, who was the curator of invertebrate paleontology. Gene was also interested in the Pennsylvanian. In fact, Ranier was switching from being a student of fossil reptiles to a student of Pennsylvanian sharks. One of the things I have always stressed to my students was certainly going on here. A Ph.D. is a license to train yourself in new subjects as you need them. And Ranier was busily doing this. He and Gene had developed a spectacular quarry near the town of Mecca, Indiana, which of course led to all sorts of good and bad jokes, about going to Mecca.

It was at precisely the level as the Mason Creek faunas at Braidwood, Illinois, which are so important as a strip mine area where there are spectacularly preserved fossils. And it was the same interval just about a particular coal, the same coal, except in this case it was a black marine shale in an isolated paralic basin separated by a brachiopod bank from the on-shore marine shale. It was an environment which was isolated and the total thickness of the Mecca Shale was only about three feet, and so far as they could tell, it was deposited in four years flat. The spectacular thing about it is because it was black shale and there was no bacterial decomposition, the black color is essentially due to plankton being buried and carbonized. The soft parts of all the organisms living in this lagoon were preserved as carbon films in articulation. And so Ranier and Gene had this spectacular research program going on in Mecca.

At the same time, Gene was also directing the amateur investigations in Mason Creek that ultimately led to the classic Mason Creek studies. It had been known for a century, but most of the work essentially was the result of the elaboration of the amateur collector's work by Gene Richardson, and to a lesser extent, Ralph Gordon Johnson, who had been with us in the invertebrate paleo course. Johnson was also a member of the Walker Museum paleontological coffee clutch.

JC: So, then Gene--did you take some of your research problems to Gene for advice?

RES: No, not particularly because at that point, I considered myself as thoroughly trained as Gene. At this point, I was on my own.I was working as a practicing invertebrate paleontologist. Gene was working in a totally different environment and had never even considered the kinds of things I was doing in my Ph.D. thesis. He was valuable in many other ways, but not really for discussion. He stayed at the Field Museum and in that year, I was seriously working on analyzing the data I had accumulated in a grand total of about eight weeks of field work, maybe less.

Thorsen's paper in the marine ecology part was my first introduction to why this was working and I simply had had not run across it at all because, of course, Emerson didn't talk big about marine ecology. And Lowenstam had not paid any attention to this aspect of marine ecology. This was essentially based on the Petterson's School of Biofisheries which got started during WWII as a means of predicting what the Danish Herring crop would would be for year by year. You essentially looked at the food chain for the Danish Herring crop in order to predict what its going to be like next year and the year after based on things you can find.

And none of that was yet available.

I was in fact extremely excited when this volume came out and I got to read Thorsen's description of these marine bottom communities and it obviously fit very closely with what I was doing. When I came to Minnesota, my whole direction of research had to change. How much time do we have? Do we want to continue here?

JC: I think we should pick up that at another time--in another tape, but I think there is one last bit of things I'd like to ask you about here.

RES: Uh huh.

JC: You say in your defense that your defense went reasonably well except for John Clark had some issues.

RES: John Clark had some issues, mainly some questions that made me consider whether using Plummer's stratigraphy was enough and I was very shortly thereafter as a result of being hired by Minnesota, and being told I would teach Geology 115, the geology of southeastern Minnesota which is the course Charley Bell invented. Charley introduced me to very detailed stratigraphy in these rocks of southeastern Minnesota in a single week. And that was the first really decent training in stratigraphy I had had. I knew the technical vocabulary and I knew the principles, but I had not seriously looked at biostratigraphy until Charley showed it to me on that field trip. So Charley was the last of my professors for one week about a month after I got my Ph.D.

JC: So he gave you what--

RES: What John had been talking about at the oral examination. I managed to get through very nicely because it was quite apparent that I did have the knowledge but it could have been better.

JC: It would have been better in--what you mean is, if you had done the stratigraphy yourself?

RES: If I had done the stratigraphy myself and if I'd paid a bit more attention to precisely where my collections had been made. Some of these shales are 60-feet thick, and it would make a difference where in the shale they came from, what the character of the fauna was. But this was still enough to show that it works.I had to drop it because at Minnesota, my teaching assignments were historical geology which I had never really had, invertebrate paleontology, and southeastern Minnesota. And my summer employment was basically going to be working on Minnesota rocks so I had to work on problems of Minnesota rocks.

JC: One of the things, when you started teaching your paleontology courses, you've told me in other contacts that you adopted some techniques that you had as a student. Can you elaborate on that a little bit?

RES: Yes. The first ones were to look over all the examinations that Charley Bell left behind and see which ones would be useful as texts, quizzes and examinations. I had to develop a historical geology course all by myself because I had never had one. I had had all the components, but I had never put it together into a coherent package. This is a standard course that is taught in most places, but was not taught at Chicago. So, in effect, I organized a course based on Moore's *Historical Geology* which was a really very satisfactory book. And also on the Schuchert and Dunbar *Historical Geology* which was in the long run not nearly as satisfactory. The *Invertebrate Paleontology*, I modified Charley's exercises using some of the materials he had and some others that were in the collections and ultimately I added a biostratigraphy course, an advanced invertebrate paleontology course, which I went off in my own direction on, and the vertebrate paleontology courses.

As long as I did the other things, I could do the vertebrate. And ultimately these boiled down to Introduction to Paleontology, Historical Geology taught very frequently because it was one of the money-making courses in the department, and the Vertebrate Paleo course.

The vertebrate paleo course I used precisely the system that had been used by Ole except that I had only me to give the lectures. I used the same text, Romer, which was the only text available that really mattered, and I would lecture. I would put specimens out. I would pass out illustrations produced by a ditto machine and supplement the lectures with specimens that I had. This meant I was in a steady process of trying to improve the collections. They were in pretty sad shape. I got some from the Field Museum. I got a lot of skulls from hither and yon by a wide variety of techniques, a lot of plaster casts from my colleagues in vertebrate paleontology here and there and developed a respectable course.

JC: You also used the presentation technique?

RES: I used the same presentation in the vertebrate paleo course, again, there were no examinations. There were lecture presentations with specimens pointing out all of the various aspects of paleontology involved with different aspects for different groups and the grades were based on an oral presentation of a term paper topic that had been researched by the student during the course of the quarter. Inevitably, this meant that between a fifth and a third of the students in the course would take incompletes because they simply hadn't done the work. They have not had the discipline to do it even though they had been told right from the very beginning. The grade was based on the oral presentation, class participation and the term paper.

[end of tape side]

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Interview with

Robert E. Sloan

by Joe Cain

Session 11

A series of interviews with Professor Robert E. Sloan regarding his career, the recent history of paleontology, and the life of an American scientist in the second half of the twentieth century.

These interviews were made possible with a generous Mellon postdoctoral fellowship from the Department of History of Science, University of Oklahoma.

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#11 Tape 6 - Side 2
March 13, 1996

RES: Professor Robert E. Sloan, Department of Geology, University of Minnesota
JC: Dr Joe Cain, interviewer

JC: We're almost at the end of our discussion about Bob's teaching. What was the goal of having the presentations?

RES: The goal of the presentation was to introduce the student to first library research because routinely students at the University of Minnesota, whether they were undergraduate had not done much library work.

They needed to pick a topic and research it in detail and then solve the problems of the inevitable conflicts between the various authors involved and do it in a reasonable basis. And many students simply threw up their hands in horror because they couldn't resolve the conflicts.

JC: But that was in some ways the point?

RES: That was the point. The people who could were the ones who were doing well. And the people who could not got incompletes which wound up being an incomplete on the record. Much later an "F". But initially there was no penalty to getting an incomplete. You just dropped the course.

JC: And you learned that technique from Ole?

RES: I learned that technique from Ole. It was very, very satisfactory. I learned a very great deal from the term paper topics that I did. One of the term paper topics I did was the evolution of the Ceratopsia and, of course, this was a prime factor ultimately in choosing to go after a *Triceratops* for the Science Museum. This was a group of species I knew something about and loved dearly.

JC: That's interesting. It all comes around full circle.

RES: Everything comes around full circle. There isn't a thing I've done that does not have inputs into it from throughout my entire career starting as a three-year-old.

JC: It keeps us busy. I think its time to take a break.

RES: Okay.

JC: We'll pick up on this tape tomorrow.

[tape continues March 14, 1996 - 1:00 P.M.]

JC: We're going to start another topic of conversation here. We're going to start talking about Bob's family and Bob growing up in Chicago in the 1930s. Bob, tell me something about your parents.

RES: My dad was a gentle man who loved to hunt and fish, but after I was ten or so, he never really could take the opportunity. The reason was, like his father before him, he was a craftsman. Well, officially he was an electrician and he spent his working career as a house electrician for Montgomery Ward & Company doing every kind of electrical construction and maintenance. He was also a carpenter. He could do plumbing and clearly had a sense of planning what kinds of operations he was going to do. After we moved into our house in Fernwood, we were able to get it because it was a very tiny house of very low market value. And he bought it from a friend. But it was a terribly cramped little house and he wound up doing many, many things to that house completely redoing it for the rest of the time they stayed there. The first thing they did, I guess, was to dig out the basement.

The house had been simply built on wood posts sitting on concrete or rock slabs sitting on the Lake Chicago Glacial Lake Deposits, which was a very, very stiff blue clay that you didn't dig, you shaved. Much different than most clays. The house had a small dugout area for the furnace--nothing more. And it had simple hot air duct distribution with floor grates made of wood. And that wasn't very satisfactory and he needed more space. So my Uncle Norman, who was not at that time married and was doing casual work, simply came up one summer and lived with us. And my dad and Uncle Norman proceeded to jack the house up off its foundation using building jacks and then dig out a new basement, lay, they themselves laid a concrete block foundation and then they had a concrete mixer come in with ready-mix and essentially pour the basement floor. So we had a floor. In the process, he had to put in the sewage lines and everything else. He was a consummate builder.

There was, of course, a tremendous amount of scrap wood left lying around from this operation and it was in piles in the yard, and after the basement was built and he had his work bench set up and his tools organized, one of the things that I was allowed to do--so was Norm--was to use any of the hand tools he had. And occasionally the power jig saw. The jig saw was about all he had in the way of power tools. There was one tool that we were restricted from. Its a draw knife. And basically it is a very, very sharp blade, sharper than most swords with a handle on each end that you pull towards you for quickly rounding big timbers. And we weren't permitted to use it and I have never wanted to use it.

JC: Because they figured it was an accident waiting to happen?

RES: Yes. In any event, we did not have a great many toys because my dad was--this was the heart of the depression. Pop was just glad to have a job, and he spent all of his time out there, essentially rebuilding the house. Once in awhile, he would go duck hunting. He never did again go squirrel hunting after we moved to Fernwood. He was forever promising to take Norm and I hunting. He did once. He let each of us let fly at a fence post with a 410 gauge double-barrel shotgun and we perforated the post--not very deeply. Four-ten's are pretty mild and it made a big satisfactory bang and that was about it. Having gotten the basement built, my mother fell down the stairs from the attic. The attic had relatively low sides. He managed to box it in and produce an attic room with a new floor and sloping walls.

The roof pitch was about 45 degrees so the side, he moved the sides of the room in so that the sides of the room were about three-feet tall and there was a very narrow flat section in the ceiling so it was basically a modified octagon in shape, rather bizarrely shaped. But it did provide additional living space and the stairs to the attic were extremely steep and there was a door right at the bottom of the stairs. The risers were about eight-inches. The treads were about six-inches, so it was steeper than 45 degrees by quite a bit. At one point, my mother had been upstairs and she missed a step coming down and she rolled down the entire flight of stairs and collided with the door at the bottom.

RES: She wasn't permanently injured, but she was surely black and blue and sore and so the next step in remodeling was to tear out the partitions. There was a small bedroom and a living room and a closet or two with a chimney going through the middle of it in what was to become the living room area. My dad built a flight set of stairs around the corner of the room to get up to the attic. He took out the old chimney, put a new chimney in a different place, added a kitchen at the back end of the house, and made a big attic bedroom still with the low sidewalls for my brother and I.

My brother, Norman, and I. Norman is two years younger than I am. So we slept up there in bunk beds that Dad made. Not bunk beds, these were simply pine boxes with a plywood bottom to hold a mattress and beneath the plywood bottom, he built in drawers so that each of us had three drawers underneath our bed for storage or clothes or toys in hopes we would keep it organized.

JC: No such luck?

RES: No such luck. And--

JC: Your father was a carpenter by training and--

RES: And my grandfather was a carpenter by training and, of course, dad had learned from him throughout his whole life. As a boy, he had been a teenager when electricity widely distributed was new and my dad played with electrical motors and circuits the way modern kids play with computers. In fact, when my brother and I were playing with our erector sets, we would power our magnificent constructions with one of the small six-volt motors that he had used as a boy. A little cast iron magnet--no winding motor that provided all the power that was necessary to run the thing. It had a little pulley on it so you used a rubber-band to connect it to the pulleys. On the erector set, you could run whatever contraption you built and we built lots of things.

JC: Did he let you play around with his electrical materials or did he work with you?

RES: No. He didn't work with us. He really didn't have to. He always bought every month two magazines and occasionally a third--Popular Mechanics, Popular Science and Mechanics Illustrated which had at one time had been called Modern Mechanics. These magazines were very different from what they are now. In fact, Popular Science originally was the same kind of publication as Scientific American is today. Then it shifted essentially at about the time of World War I to a magazine for home handy men and it would have information on repair of automobiles, on craft projects ranging from summer cabins to model airplanes and to all sorts of things. I remember in particular one issue of Popular Science that fascinated me greatly in which there were complete plans for building a model of a New York Central 4-6-4 Hudson locomotive in HO scale which was brand new, and giving precise dimensions for how you should cut--and patterns, how you should cut every piece of brass and solder this locomotive together, and you would have a model superior to most of those that were readily available.

So these were basically construction magazines. There was a little bit of news about exciting things. For example, the special pieces of equipment that were used on the (Admiral Richard) Byrd and Arctic expeditions were always described including Byrd's worst failure which was an eight-wheel-drive machine designed to travel across the Antarctic ice cap in style, basically an eight-wheel-drive and steering truck with a boat bottom and wheels the size of LeTourneau earth movers that are used in highway construction. The biggest tires anyone could get. The thing was not a howling success and was abandoned in Antarctica where it sits today. But this was described in glowing detail with all the things that Byrd was going to be able to do with this thing. It was not the modern kinds of Popular Science and Popular Mechanics where the machine basically describes all the neat features that are going to be in next year's car.

These magazines had some very interesting things for me. I remember vividly a series of plans for water line models of typical naval ships, Liberty ships, LST's, destroyers, battle ships, aircraft carriers, so that with also with cardboard and scraps of wood, you could make your own three to ten-inch model of the latest thing in the Navy.

And descriptions on how to carve a half size replica of an 1860 Colt Army revolver or an antique revolutionary war musket. Always done at half size. Oh, and there was also a Scmeisser FP-43 machine pistol, the first of the really good sub machine guns other than a Thompson. And these were fascinating and I read them avidly. They always had a photograph of the finished object as well as a major side view, and in the case of the ships, deck view as well.

With cross sections marked so that you could figure out what the three-dimensional shape of this object should be. And having done a few of these things, and carved an 1860 Colt revolver into the lid of an all cedar cigar box, the kind you can't get anymore, but were absolutely delightful. I ran across a drawing of a 1903 Springfield, so I carved a roughly fifteen-inch long, reduced size replica of the 1903 Springfield. In this case, I didn't have the cross section so I got the butt stock wrong. But I even set it up so that the bolt would work. Whittling was something I did starting very early on in pine and balsa wood. When I was about eight, there was a program of building model--rubber-band part model airplanes down at one of the local parks. And this was supposed to be for nine-year-olds or better, but I got in. And I started to build my first model, and it was a model of Winnie May, the plane that Wiley Post and Will Rogers were flying around the world when they were killed. A very famous airplane.

And it was going very well until I thought I would add something that wasn't in the plans, movable control surfaces so I just cut through the thing and it flopped because there was no framework moving, so that one was scrapped. Ten cents down the drain. I was able to convince my folks to get me one of these ten-cent model airplanes every so often, and ultimately, it got to the point where I built a torpedo bomber of about two-foot length span when I was ten using scrap balsa left over from other projects, finished or unfinished, and broom straws and a variety of other things. It was a nice, fine model. Some of these flew. Most of them were over-weight by the time I got them covered and glued so that they would have a very, very steep glide angle.

JC: Now these projects that you worked on, were you doing them by yourself or were you--

RES: I was doing them by myself. The first one I got started in with at Park program and ever after, I was doing them on my own.

JC: Was your brother involved or your folks?

RES: No. He didn't make as many models as I did. There was the building of them that was important, not the flying of them. And we both spent a lot of time with our erector sets. We had started out with a very little erector set and when they saw how much pleasure we got out of them, how many things we built with the very small parts in the beginner kit, my dad found one of the steel chest ones that had been returned in the employees bargain room at Wards. And it was missing a few pieces, but we just added the pieces from the starter set. And the motors we had around and everything else and sometimes we'd get the lid closed if we got everything in exactly right, but we made a tremendous variety of things with that erector set.

JC: You said that building these things are the most interesting part of the job.

RES: Yes.

JC: What is it about making model airplanes, model boats, models in general that appealed to you so much?

RES: Making something out of nothing. Or very little.

I had gotten very good at it, both flying models, the ones with rubber bands and tissue paper covering, and also solid balsa models. During World War II, very early on, the Navy set up a program in which the skilled model makers around the country would make and donate to the Navy pine models of aircraft from all over the world for identification purposes. These were always done with retracted wheels unless they had fixed gear and they were always painted black. And the object was to have these models available in squadrons so that pilots would see, no matter from what angle, and identify the airplane so they wouldn't shoot down their friends.

JC: The friend and foe exercises.

RES: Yeah.

Ultimately, they went to plastic injection models, but of course the plans were very readily available. Again, you had to be fifteen to do this and I wasn't, but I managed to acquire the plans and I built so many of them. One of--I had a couple of failures. One of them in particular, one of my friends, Billy something and I can't remember--Smith, maybe--had picked up a model of a Curtis Hawk Biplane and he was having trouble building it and he asked me if I couldn't carve out the fuselage for him. And there wasn't quite enough information in the plan. There were no cross sections available and so I botched it and he was very upset and I was as upset as he was when I realized what had happened because the accuracy of the models was something that was important.

This quickly spread into trains. A friend of mine a half a block away on another street, Carroll Repacey, had HO trains and this was back before any plastic models. The HO train manufacturers had, of course, moved into war production so you were only left with the prewar scraps. One outfit would make tenite plastic trucks and wheel sets so you could build freight cars and put these plastic trucks on it. And I didn't have a locomotive, couldn't afford to buy a kit for one of the locomotives--why they started at $15.00--

JC: Which was a lot of money.

RES: Which was a lot of money. I couldn't even afford much. I did ultimately manage to acquire a single electric motor suitable for putting in a locomotive and some drivers, and then tried to build one out of tin can metal. It should have been brass, but I didn't have brass. I did have tin cans. Tin cans you could get. And that was a failure, but I always learned from my failures--things that didn't work right. In the process of working from these magazine articles to build things, I learned about the importance of three views--the three orthographic views that normally cover what's need to build something, and in the process, I also wound up teaching myself perspective.

I have written in the autobiography about my sickly childhood and how I read everything in sight. And my folks would go to this used bookstore and buy used books of all sorts. It didn't matter, whatever it was, I digested it. There were, of course, my dad's college texts and some things left over from my grandfather, and among them were articles from manual training courses involving mechanical drawing.

And from art courses describing perspective and so I drew things as well as making them solid, carving them out of wood.

I had the usual accidents. I got a neat scar on my thumb that I got at about eight when I was carving balsa wood with a knife, and the balsa wood suddenly split and the knife went into my thumb. There was this big flap so they put a band aid on it and it healed. But the scar is still there. It's been awhile. That was about the same time I was building that first rubber-band powered airplane. So the three-dimensional thinking--the spatial thinking.

The spatial thinking was something that I was thoroughly trained in by the time I was ten.

Not just seeing things in orthographic projection, but developing perspective views as well and being able to go from a perspective view, such as a photograph, to an orthographic projection.

JC: And when you say that you were thoroughly trained in it by ten, is what you mean you--

RES: I had learned it--

JC: Yeah.

RES: --for myself.

JC: It sounds like you------

RES: Manual training, technical school books and low grade college textbooks. It was in this same period of time that I read and digested the entire contents of Parker and Haswell's college zoology book. Which basically covered all of anatomy and comparative embryology.

JC: One of the things about your descriptions of when you were growing up is you played with a lot of scraps and you made things out of the scraps.

RES: Uh huh.

JC: This is going to be a leading question, but do you think that because your family didn't have a lot of money to buy a lot of things, do you think that's important in your development?

RES: I suspect it really was. They spent it on the right things for me.

All those books, technical books and fiction in every possible subject--it all came through and I read it all. And while we were not without toys, our wishes for toys were really not well met most of the time. The thing we really looked forward at Christmas was the appearance of the 128-page Ward's Christmas catalog with toys.

Because then we could ooh and aah and wish we could have--it was a wish book. We could wish we could have this and this and this and sometimes we got something close to it.

JC: Did you ever make some of the things that were on your wish list?

RES: With the erector set, yes.

And Pop did get us some trains. Again, they were bargain room returns of the least expensive train sets available in individual pieces. We had one American flyer locomotive in O-gauge--a 3/16 scale that was to us super detail. But the rest of them were all the Marx models which were lithographed tin plate, punched out and then formed into shape and twisted together with tabs with a very basic mechanism inside it. Sometimes it was clockwork mechanism or often it was a four-wheel electrical mechanism. And the cars were all Marx cars which were gaudy, but a very simple construction--quite sturdy. And we did put together a--my dad put together a train table for us made out of one by threes and quarter-inch plywood.

And we had a permanent layout set up on that. These weren't the elaborate super detailed models that Lionel made or American Flyer made for the most part, but they did the job. They moved around.

And we did build buildings from straps of wood and things of this sort.

JC: We should change the tape now.

[end of tape side]

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Interview with

Robert E. Sloan

by Joe Cain

Session 12

A series of interviews with Professor Robert E. Sloan regarding his career, the recent history of paleontology, and the life of an American scientist in the second half of the twentieth century.

These interviews were made possible with a generous Mellon postdoctoral fellowship from the Department of History of Science, University of Oklahoma.

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#12 Tape 7, Side 1
March 14, 1996

RES: Professor Robert E. Sloan, Department of Geology, University of Minnesota
JC: Dr Joe Cain, interviewer

JC: It is about 1:30 or so. We are continuing our conversation with Bob Sloan and we are talking about Bob's family. Bob, we have talked a little bit about your father. Why don't you tell me some about your mother?

RES: My mother was given a normal school education that is the equivalent of the modern associate of arts degree and came out of normal school with a teaching certificate. Her work career consisted of two terms of teaching and then some part-time work at a bank in Champaign, Illinois. After that, she never worked. Initially she drove, but she had given up driving for all intents and purposes by the time I was ten. Probably because my dad was an iron man driver. He would just drive and drive and drive and drive. She was not a very good cook. She was a terrible cook. She could burn water. And she was well read. She did a lot of reading. After I was 13, and our youngest brother, James Richard, was born, by that time she was nearing 40, let's see, 1898, she was 43. And she did not go to the doctor very frequently and she had a very difficult pregnancy and ailed from it for the rest of her life. So she was never active after that.

JC: What was your relationship with your mom when you were growing up?

RES: Reasonably good until I went to the University of Chicago. And then she started feeling the pressure of my having surpassed her and we got into some head to head arguments. She couldn't believe that Einstein was right in the special theory of relativity and she couldn't be convinced of it.

JC: What do you mean by that?

RES: Well, there are a lot of peculiar things that take place that don't follow normal activity when you are dealing with objects moving close to the speed of light. And of course, I had read all about them. I knew about them very thoroughly. I knew the tests that had been made to demonstrate that this was correct. At low speeds, it collapsed into typical Newtonian physics, but these were monstrous corrections at high speeds. And she couldn't believe the world was put together that way. I really don't know why. It may have been, as much as anything, a matter of rejecting the notion that I was now surpassing her in terms of what she knew.

JC: Your mother worked quite a bit through your childhood?

RES: No, she didn't. She did not work after we moved to Fernwood, about 1935 or so, 1937, somewhere in there.

JC: I thought she had picked up a job at Wards.

RES: No. She never did. She had worked at Wards very early in their marriage, or before their marriage. But she did not work after we moved to Fernwood. She managed the house and not very well. In her defense, I might say that Norma and I never really did learn to keep things well organized and neat and clean. She was continually railing at us. I think she just gave up trying.

JC: You did a lot of errands even as a small boy.

RES: Yes. She was particularly, after Jimmy was born, she didn't move very much. She sat around all day. And so I routinely went the two blocks to the grocery store and the four blocks to the coal yard and paid bills at the coal yard. The bills were always late. We were, my family were not good money managers. In part because my dad's employment at Wards in the depression was great, but it was pretty low and with 2 boys in the family, the expenses must have been quite a bit. My dad had a problem. He loved the bargain room at Wards where broken or returned merchandise was sold at great reductions in price. And he was forever thinking of projects he would like to do with these oddments that he would bring home from the bargain room. And he could charge them. And so he did. And so we always had things that someday would become something else and never did. I am afraid it is a habit I learned too.

JC: The curse of a tinkerer.

RES: The curse of a tinkerer is right.

JC: You mentioned a few things about your brother, Norman. Tell me about Norman.

RES: Well, Norm was close enough to my age so that we were best friends. We almost never got into even squabbles, let alone fights. There was one time when we had a wrestling match, as I recall. He was very furious because my behavior was simply to hold him so he couldn't do anything. Which only made him even more mad. But our relationship was always one of very, very close and loving care for each other. We played with Donnie Matthews across the street, Eston and Lois Ellis next door on one side. And Stewart Woodward on the other side. So there was this cluster of six kids essentially playing together all the time. Eston had a gelatin duplicator. For those who don't know of it now, it is comparable to the ditto machines that are thankfully now being gotten rid of in most offices.

Basically, there was a tin pan, very low and flat, at about 8-1/2 by 11, filled with gelatin. And there was a special hectograph carbon paper, and you would put a sheet of paper down, put the hectograph carbon paper on top of it, and then another piece of paper on top of that. You would draw on the top carbon and you would produce a reversed image in hectograph carbon paper on the other one. You then put the hectograph carbon paper down on the gelatin. The hectograph pigment would transfer to the gelatin. You would then get 10 or so copies by just putting a slightly damp piece of paper on top of the gelatin. And you would get a right side print. It was a very, very simple version of lithography.

JC: And you made little newspapers, is that right?

RES: We made our own newspapers. Eston taught me to print in block capitals and lower case letters because of course, I had learned cursive in school. And somehow there wasn't much block printing in school. I learned it from Eston.

JC: About what age were you?

RES: Oh, 8 or 10, somewhere in there. So we did that. We played alley baseball. One of the standard city kids' modifications of baseball. You change the rules until they fit what you have got, where you are playing. We did have a bat. We had a monster softball which had lost all shape and form. It was about the consistency of a sofa pillow. So when you hit it, it deformed all out of sphericity, and of course, did not have very good ballistic properties. But that is all right. We were talking about a baseball diamond with maybe 15 feet between home and first base. And you could touch first and third base from the pitcher's mound.

JC: And so if I was driving down your street in Fernwood in the 30s I would most likely see you and Norm palling around.

RES: Yes. We would be doing all sorts of things. Occasionally we played football. Football was never my favorite game. I wasn't good at it. I didn't like it. I didn't like the roughhousing involved. I still don't to this day. I think it is a thug's game. Baseball, on the other hand, ... is a game of skill, like pool. We did have a pool table.

JC: You did, at your house?

RES: A miniature pool table with pool balls about one inch in diameter. And the table might have been 3-1/2 by 6 feet. With cushions. And short cues of course. We had a lot of fun with that. And we played cards. Most usually we played pinochle. Because pinochle was a game that our parents played with Grandma and Grandpa Schwartz, my mother's parents, and also played with my dad's cousin, Mary Richards Roland. And we would go over to Aunt Ida Richards' place where Mary lived, her husband had been killed in the navy on the Murmansk run. This was `42 or `43. And we would spend the night there. And playing games continuously the whole time we were there.

JC: When your folks socialized, did they socialize with neighbors, or relatives mostly?

RES: Mostly with relatives. Until my mother became an invalid, we went to the Fernwood Methodist Episcopal church at the other end of the block and after mom was, essentially became a semi-invalid, we stopped going there. Later on Norm and I picked up with the Morgan Park Baptist Church, which belonged to the Southern Baptist Conference. It was relatively close to the school I went to for my freshman year, and we joined the youth group there. And I was active there until about a year after I started at the University of Chicago and then I just dropped it.

JC: How important was religion in your younger ...?

RES: It was important in terms of developing a sense of morality and ethics. And since I was a voracious reader, by the time I was 12 I had read the entire Old and New Testaments. Plus the Apocrypha.

JC: Would you describe your parents as devout or casual?

RES: Casual I would say.

JC: And would you say grace at the table?

RES: Not ordinarily. Maybe on special occasions or when someone more devout was there. But we very seldom had people over for dinner because my mother was a terrible cook.

JC: And so would it be fair to say that going to church was more of a social function?

RES: I think probably so. And there was the feeling that it was something that children ought to be brought up as. Both of their parents had been much more religious than mom and pop were.

JC: You have described elsewhere, your parents being rather conservative in politics. Can you expand on that?

RES: Oh, yes. Despite all of the good that Roosevelt had clearly done for the country in the early phase of the depression, they were devout conservative Republicans. And they were continually muttering about the evil things that Roosevelt had done, despite the fact that they weren't part of the group that was being injured by any of Roosevelt's activities, and in fact, had gained considerably by his behavior. We normally read the Chicago Tribune that arch conservative newspaper. For the most part, that was the only paper that was delivered. Later on we got the Herald Examiner and still later, after it was founded, the Sun Times, which was somewhat more liberal. But we always got the Tribune.

JC: In the late 30s and early 40s, the Tribune was strongly isolationist.

RES: Yes, it was. And so were my folks.

JC: Really?

RES: One of the things that happened at the University of Chicago is that I was very quickly made aware of a more liberal viewpoint, and it agreed much more closely with the sense of ethnics and morality that I had developed in my church. And so at that point, I became a liberal. And most of the time a democrat.

JC: Do you think, did that play a role, do you think, with the tension that you were experiencing with your mother?

RES: No, because I really didn't talk to her about politics. It was quite apparent that her mind was made up on the subject. It was not a subject that was open for discussion.

JC: Your brother, Jimmy, then came along. You described him a little bit. Can you say some more things about him?

RES: He was a neat baby, but somehow, he was not a stable child. He was hyperactive and my folks simply, particularly my mother, gave in to his every whim. As a result, throughout his entire childhood, he was allowed continuous access to pop. We didn't have pop very often. We had water, we had milk. That was all. Pop was a very occasional thing. Jimmy drank so much pop that it literally decayed his teeth.

[The tape has an inaudible section here. RES adds in edit: "I took him along on a four-week geology field course to the Black Hills in 1956." It continues:]

RES: But it was not for want of trying. He was continually running up steep slopes, without any thought to was he going to get stranded. He dislodged some boulders accidentally, big boulders, I can recall particularly one case, on Whitewood Peak, where boulders started rolling down the hill. It was about 4 feet in diameter. And I was down there, so all I could do was just wait until the boulder was within 15 feet or so and then step aside. There was no point moving beforehand, because you could be moving into the path as easily as out of it. I have often wondered whether Jimmy might have been the result of the reason for that boulder starting down hill. He was, of course, up there. He did find a complete *Oreodon* skeleton in a concretion in the Turtle *Oreodon* bed of the Brule Formation in the Big Badlands, which we carried home and ultimately became a mounted exhibit in the Science Museum. It is still on display. And every time my nieces and nephews come up, they go over and take a look at Uncle Jimmy's skeleton.

JC: Did he take pride in that? Or did that matter to him?

RES: Just the thrill of discovery is all. Nothing more. He was, he continued to be spoiled rotten and hyperactive. Somehow he convinced my folks that he ought to have a motorcycle so he had a motorcycle. And while riding it along one of the major streets in Chicago, a car drove into him and he suffered a severe concussion and died of internal bleeding in the head. A real tragedy.

JC: How did that affect your family?

RES: Well, I don't know. Because at that point, I was here in Minnesota and they were in Chicago, and we didn't see them that many times a year. Two or three times a year. And so I just don't know. Ultimately, my dad shifted from being a house electrician to being in charge of setting up catalog order stores in all sorts of small towns around the Midwest. And he essentially ranged from Minnesota to Ohio and was driving first in the family's 49 Hudson and later he had about a 55 Ford Station Wagon that the company gave him, it was just too wearing on him. By that time, when he finally quit it, he was about 57. And he simply couldn't cope with both the extended driving and the business of completely remodeling and refurbishing a store to Wards' standard patterns. And so he finished off his career at Wards, again a house electrician for the most part, working on the elevators.

JC: Your folks lived in Fernwood for a long time.

RES: They lived there for a long, long time. They finally disappeared during the white flight from the area. By this time, my brother was working for Motorola, and had a home in Schaumburg, in Hoffman Corners, in the northwestern suburbs of Chicago. When they sold the Fernwood home, they moved into the basement of Norm's house. In fact, they used the proceeds from the sale of the family home to help Norm and Romaine buy this house that they moved into a basement apartment.

JC: What kind of effect did moving out of Chicago have on them?

RES: At that point, they were pleased to leave. It was not the peaceful, quiet small town neighborhood within Chicago that it had been while we were boys.

JC: What did your folks do in retirement?

RES: My dad played with lapidary work and the other things. He never did go back to hunting. And my mother just sat and read.

JC: And you have always been very close to your brother?

RES: Much closer to my brother than to my mother. I loved my dad deeply, I tolerated my mother. Particularly after we had those knock down drag outs when I was around 17, family arguments.

JC: And you and your brother have done quite a number of things over the years.

RES: Oh, yeah. I joined the National Guard just before the beginning of the Korean Emergency when the draft was re-instituted, and my brother joined exactly the same unit a week after I did. We did it together. For the first part of my enlisted career, my brother was in exactly the same section that I was running. Later on, he shifted into communications. He had gone to the DeVry Institute, which was an electronic electrical trade school, in Chicago. And ultimately he wound up getting a bachelor's degree out of the Illinois Institute of Technology. He did not have the same kinds of easy access to scholarship money that I did. By that time, my folks were concentrating on Jimmy, and so Norm got stuck having to pay his own tuition.

JC: You have done a number of things to help him out, giving him cars and such.

RES: Oh, yeah. My first car was a 1935 Ford that I bought for $35. It had some interesting shenanigans about the title which I finally managed to clear up with an ironing board and paper towels.

JC: I don't understand.

RES: Well, it seems that all of the signatures on the title which I was given when I bought the car, were covered with carbon paper. You couldn't read the title. And the state would not give me a title to it. So somehow, I had to remove the carbon paper from the license enough so that they could see it was indeed a valid title and had been signed. And so the solution was to put it on a table with a damp cloth on top of it and then cover it with paper towels and literally iron it and transfer the carbon paper from the title to the paper towel. Once I had gone through several cycles of this, you could then read the title again. Finally, I got the title transferred.

Anyway, that car was the car I learned on. I completely rebuilt every aspect of it except the one thing that desperately needed, and it needed a new crankshaft. It didn't get one. And in the process of rebuilding it, I completely redid the wiring, and added sealed beam (headlights). I completely redid the distributor, the carburetor, I completely redid the upholstery. I patched the body and changed the brakes from the horrible Ford mechanical brakes to hydraulic brakes. I had done a major amount of conversion. The car ultimately cost me about $600 including the tools necessary to do the job. But in the process, I learned what I needed to know about automotive mechanics, and the basic principles of automobiles and how they work, and how you fix things when they are broken and so on.

After having made the trip to Texas in 1951, two trips to Texas in 1951, in connection with Ole's trip, and my thesis, I then, after I got home, found a car that had great allure for me, a 1939 Ford Coupe, with a bashed in trunk lid. And a rusted out floor board. And I got it for $85. This was a much more modern car. It already had hydraulic brakes. It was the deluxe 1939 Ford, so it was exactly the same as a 1940 in most respects. I could add sealed beams simply by going junkyarding and getting 1940 headlights to put in the body. I found the Coupe in another junkyard, from 1940 that had a good trunk lid. I picked up the trunk lid for $10, including the sheet metal below it that included the latch, and a friend of mine brazed the pieces together, and I rebuilt a fine engine to put into it. I didn't spend $600 this time, because it was somewhat better. I took care of the rusted out floorboards by getting some 1/16 of an inch thick steel, and literally having flame cut new floor boards, and then bolted them in place on top of the old ones and put the seat back in.

That problem was solved. And since I now had 2 cars, and didn't need one, and Norm did, Norm was at that point, going to the University of Illinois in Champaign, no he wasn't, he was still going to IIT. But no, I'll take it back. He was at Champaign. And he was drilling with a unit down there. And he had to come back and forth. Well, that unit was federalized. So he came back to our unit, but he still had to go back to Champaign in between drills. So he was commuting from Chicago to Champaign, and to pay for this, he was bringing crates of eggs up and selling them around the car, making a sizable profit in the meantime. So I gave him the 35 Ford and that was what he was using for commuting. He stopped at an accident, and while he was out of the car, helping people in the accident, they were just off the road, somebody rear-ended him, and smashed the back end of the car, split the gas tank open.

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Interview with

Robert E. Sloan

by Joe Cain

Session 13

A series of interviews with Professor Robert E. Sloan regarding his career, the recent history of paleontology, and the life of an American scientist in the second half of the twentieth century.

These interviews were made possible with a generous Mellon postdoctoral fellowship from the Department of History of Science, University of Oklahoma.

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#13 Tape 7, Side 2
March 14, 1996

RES: Professor Robert E. Sloan, Department of Geology, University of Minnesota
JC: Dr Joe Cain, interviewer

JC: We are continuing our conversation with Bob Sloan and Bob's family.

RES: Anyway, the car was rear-ended. Norm wasn't hurt, but the gas tank was ripped open. And ultimately I went down there and re-covered the car. The way we got the car back home to dismantle it and to sell off parts, was I put a bucket of gasoline in the front seat, cut off the gas line, and simply shoved it into the bucket of gasoline and drove it home that way.

JC: Yikes!

RES: Well, I wasn't smoking, so it was not a totally unreasonable thing to do. Anyway, we got the car home. Norm and I have always had a relatively close relationship. For example, when Sal and I got married, we had traded in the 39 Ford on a brand new Ford 1953 Ford station wagon. We needed the space because we were going to be hauling a lot of stuff between our parents' homes in Chicago and our new home up here in St. Paul. And we had been promised, "Oh, the car will be there before the wedding". Well, the day of the wedding, "the car will be here before the reception". At the end of the reception, when the reception was almost over, and we called to check on the car, "Oh, it is in Minneapolis." It had been built at the Ford plant in St. Paul. And we were due up here the next day, because our honeymoon was basically going to be moving up here and getting ready to start fall quarter in about 2 months. And making sure we had a place to live and all the other things of establishing a family. So we were wondering what we were going to do when Norm suggested that we borrow our folks 1949 Hudson Sedan and he and his fiancee would drive us up here. So the four of us spent our wedding night driving 14 hours from Chicago to St. Paul. We pulled into the motel we had arranged for, at around 8:00 or so in the morning, and then Norm and Romaine got in the car and drove back to Chicago.

JC: That's a long day to be in a car.

RES: That's a long time to be in a car. And of course, we have been very close over the years. I am the crazy Uncle ??, not the crazy Uncle Bob. I am the outrageous Uncle Bob to my nieces and nephews.

JC: And you always considered them children of your own.

RES: Oh, yes, I am very close to them.

JC: I want to change gears slightly here and talk about in all your descriptions of things today that you did as a child and in the discussions we have had before, you have emphasized working with your hands, working with tools, making things, 3 dimensional thinking, basically what I summarized here before as a craft skills.

RES: More than craft skills. My brain works in a way that is different from most of the kids I see in my introductory course for the great unwashed. I am sure you have seen these pictures of images that are rotated in space on a computer screen.

This is not something that most people can do. I have been doing it in my brain since I was 10. I can literally look at a three view and construct a mental perspective image of what the three view plan represents, or the way I got through structural geology and field geology, was a way very different from most students, which meant I understood the structures very quickly, much more quickly than most of the students. I would literally construct the equivalent of a mental flat sheet representing a bedding surface. And then as I was going through an area such as Baraboo mapping dips and strikes, I would superimpose on that flat surface, the north, south, east west coordinate grid and I would locate each point as I did it and literally crimp and deform the flat sheet representing a bedding surface, according to the dip that I had just measured at a particular spot. And by the time I had roamed over much of Baraboo which was the first time I did this, I had a complete mental image of precisely how these rocks had been deformed. In three dimensional perspective. Then I could go through and erode it and ultimately wind up with a massive four dimensional model. The three spatial dimensions plus the changes in time which I could see as a movie any time I wanted to. And I could go back to a particular point in this file in my brain, and literally use it to reconstruct any kind of cross section or map I needed. This is not something most people can do.

JC: Where did that skill come from?

RES: I am quite sure that it came from this bit of making models with successes and failures between the time I was 10 and 15. 8 and 15. The failures were as important as the successes.

JC: So what, for a child who is playing around, translates into technical training as an adult.

RES: Yes, it really did. And technical training the kind most people don't get. And today, there are of course, computer programs that will do this. But my brain was simply doing it for me. I felt this is what everybody could do. I couldn't understand why other people couldn't see it.

JC: And not only that, but you also are a tinkerer.

RES: I am a tinkerer. I learned all the skills that my dad had, that my father-in-law had, and some I developed all by myself. I don't forget things. I don't forget much. And the net result is if I am in a situation where I had better fix it or I won't get out of this problem, I fix it. And it can mean some truly remarkable fixes. I recall when I was having really serious problems because the heater duct in my Scout had broken, couldn't be repaired. And so I fixed it on the spot with some bailing wire and the cardboard from a cocoa can. I can recall in the middle of the night, on one of our trips, taking one of the by this time, old departmental vans, to Montana, for the Bug Creek trip.

We were in the middle of North Dakota in a drenching rain and the total rainfall that night was 4 inches. And we were out in the middle of Interstate 94 and suddenly while the engine was running, the car stopped going. And pressing on the throttle didn't change the speed of the engine at all, it just dropped to idle. It stayed there all the time. So I knew it was an old vehicle and I had thrown a traveling kit of tools, a few wrenches, some screwdrivers, a hacksaw blade, cutting pliers and some other stuff. And there happened to be, for some strange reason, a coat hanger in the bottom of the van. Always strange things floating in departmental vans.

And so I was driving at the time. And we simply pulled off to the side of the road. We opened up the engine cover, which was on the inside of the van, threw it in the back seat. And there was the trouble. The throttle linkage had worn through. The ball joint was not connected. And so there was nothing between the gas pedal and the carburetor to open the carburetor. So I got out my big, heavy, side cutting pliers and the coat hanger, and proceeded to bend a piece of coat hanger wire to serve as a band aid to keep the tattered remains of the throttle linkage back together, and we finished the trip with the band aid.

Or the time when the Duluth International Harvester carryall had a rock thrown into the gas tank just where the feed line was, and all the gas was leaking out on the ground on this road 60 miles from the nearest gas station on the west side of Salt Lake, in the Lakeside range. And "what are we going to do? How are we going to get things? We can't cram everybody into the other vans because there isn't enough space. We have 65 students and 12 of them were in this vehicle. We had to somehow get it back. And well, I had just given up pipe smoking and I was chewing gum at the time, and I had a fresh pack of Beeman's Pepsin Chewing Gum, so I chewed up a stick and I had several other people chew up sticks of chewing gum and the first stage in the process was to cram the chewing gun in the crack in the fitting at the front of the gas tank where the gas line came out. That was where all the leak was. Well, that slowed the leak but it didn't stop it because the gas was literally dissolving the chewing gum as we watched. So it wasn't going to do the job completely. There was a roll of electrical friction tape which is the old black cloth electrical tape with a really gooey adhesive on it. It was not quite as sticky as duct tape, but fairly close. And not as wide, either.

So I then wrapped the gum and the fitting with the friction tape. That slowed the leak considerably, but it was still there. And we found a red rag lying in the back of one of the vans. I ripped it into strips about an inch and a half wide, and literally tied it around the friction tape so that the gas wouldn't dissolve the gum on the friction tape, and hold everything in place with a splint. When they finally junked the van 10 years later, they were still running on that patch.

JC: (Laughter) Your craft and technical skills are obviously important to a lot of the different things that you do. And they help you out in the field tremendously sometimes. And you have a lot of stories about the quick fixes that you made. I want to ask you a slightly different question. How does having these skills affect your fit in the community of paleontologists? Are there a lot of people like that?

RES: Yes. There really are. My old fellow student, Ernest Luther Lundelius, went to Texas the year after I did. And Ernie was out with the Model A Ford and the distributor rotor broke. Well, a car won't run without a distributor rotor. So what Ernie did was take this broken Bakelite distributor rotor which had metal pieces in it, to conduct the electricity, and he literally carved a new rotor out of a well weathered piece of fencepost and sat the metal pieces from the broken rotor into it and covered the whole thing with shellac to glue it together, because these were the things he had, and he then limped it back into camp, at town, where he could get another rotor. And still later on, when Koniszesky was down there, with Oley, they had a great big rip in the top of the Model A and it wasn't keeping the rain out. And while it doesn't rain very much, when it does rain, it rains a lot. So they put a new top on with burlap and plaster using exactly the technique used for bandaging fossils. Morris Skinner, who was a Frick collector during the depression, invented a tractor. He made it out of Model T parts. Because Model Ts were really cheap at that point. They were obsolete. And nobody wanted them. You could get Model T parts for free. And what he did was literally put two transmissions in this thing. One transmission driving the other transmission, so he could have really low ratio with lots of power. No speed, but lots of power, and use this as a tractor to help pull stuff around.

JC: How does the ability to do these things affect the things that you can do as a paleontologist? Affect the things that you think about doing as a paleontologist?

RES: It greatly affects the people who are field paleontologists. There are a lot of paleontologists who are strictly museum paleontologists that depend on collections that others make. The field paleontologist has to be able to cope with any kind of problem that comes along and solve it in order to get back to business. And you have to do it with what is available. And most vertebrate paleontologists are fairly bright people, but those who are involved in field work, frequently absorb craft skills such as this from what they do so that they can solve. It would be great if we didn't have to be continually patching your four-wheel drive vehicle, but on the other hand, only someone like Malcolm McKenna can afford to go out and buy a new four-wheel drive whenever he wants to. And the rest of us usually wind up getting vehicles that have had a long and arduous service and then keep them running. The Scout that I had, my personal Scout, was a 1963 model that Malcolm had used for long and arduous service. He replaced it then with a Land Rover, but allowed students doing field projects to use the thing. Well, they weren't as well skilled as some others, and the vehicle suffered. So I finally bought it from him as a ten-year old vehicle for $200. It was in pretty sad shape. The seat was broken. The windows were sitting in the back seat. The tires were bald.

The battery was flat. It really needed major work. It had had at that point over 75,000 really hard miles on it and that is a bit much for a four-wheel drive. So I then kept it until about 1991 when my nephew came home from the Navy. He didn't have anything for a car. He was going to finish his college degree and in the process, I gave him the Scout and he still has it. This thing, at this point, is 33 years old. And it probably has 120,000 miles on it. It continues to get patched, and it will continue to run indefinitely. At this point, it is an antique car.

JC: You mentioned that there were different kinds of paleontologists.

RES: Some people who spend most of their time in the field, and some people who prefer not to go into the field.

I wouldn't say most; there are a few who spend most of their time in the field. The Frick collectors were of that sort, although most of them came back to the American museum in the winter and worked there.

JC: I guess I mean people who want to go out to the field.

RES: Yeah. There are certainly people who desperately enjoy field work. I definitely am one of them.

JC: Aside from the obvious of getting new material and understanding localities better, what is it about being in the field that makes it worthwhile?

RES: It is never boring. You are solving problem after problem after problem at every possible level. The problems of just plain surviving the weather. And the places where I have done much of my field work have been places where weather gets to be severe. Seven inches in 25 hours when you are camping, is an awful lot of rain. Temperature changes from freezing to 120 degrees in the shade in the same week, are major changes. And you have to be able to cope with them. You do it because the only way the problems that you are really interested in are going to be solved if you collect the materials and collect all the kinds of data that are necessary at the same time you are collecting materials. That is the stratigraphy, the lithic associations, the paleontological aspects, the taphonomy, all of these things have to be done simultaneously in order to get materials that you could use to solve even the simplest kinds of problems. It is never any great problem finding new species to describe. I could go out this afternoon and drive 15 miles into the middle Ordovician and I would be willing to bet I could find a describable specimen by 5:00 tonight. But the real problem is finding specimens that are worth spending time on. And this is really what I have done most of my career. I have had a hypothesis first, I decided what kinds of specimens, of what age, and of what sedimentary facies would best solve this problem, then I go there and do whatever it takes to collect those specimens.

JC: We are going to explore that in the next tape, I think, and I want to ask a little bit more about being in the field. Is there something about being with, I guess the question I want to get at is, is there a sense of camaraderie with the people that you are out in the field with?

RES: Oh, always. You become a family. I have had a few people who did not join in the operation and who were essentially totally miserable and the best thing you can do at that point is send them home as quickly as possible. Both for them and for you. They totally destroy the morale of the camp with their whining and complaining. And you could jolly most people along, but people of that sort you can't. And the thing to do is find that out as quickly as possible and then get rid of them.

JC: You have not only worked with your professional colleagues quite a bit in the field, but you have also taken quite a number of student groups out.

RES: Oh, yes. This of course, goes all the way back to the first time I taught Geology 115 for the University of Minnesota in 1954. The year after Charlie introduced me to the region. And what you really have to do is train the students in several subjects at the same time. You are training them in Geology. You are training them in how to get around the hills safely. You are training them in how to be of good cheer even though the weather is miserable. You are training them in the skills necessary to be comfortable in outrageous conditions. Because you don't have any control over the conditions you are going to deal with. The best you can hope to do is go to Texas in the spring or to south China in mid winter.

JC: What is the difference between having a field party made of students and a field party made up of colleagues?

RES: You don't have to do as much training when you have a field party made up of colleagues. But the students really need to be trained. Some of them may have done some camping, but they have not done scientific work at the same time. And so most of them normally aren't capable of cooking or don't know how to set up a tent in a way that will survive, or don't know what clothes to take to be comfortable. And don't have a feeling for what kinds of foods are reasonable in a camping situation. So my 20 years or so of class field trips out to Montana always did two things. It got me some more materials, but more importantly, it provided essential training in paleontological techniques and just plain how to be comfortable and happy even though the climate is awful. You need to introduce them to cooking methods in the field, which are not completely the same as those in a well-equipped kitchen. To the necessary sanitary things that you have to indulge in like spending all your time heating water so that you have got plenty of boiling water to get good, clean dishes. To what sorts of things that are really suitable for field lunches, and how much liquid you really have to drink and what you have to do to make certain that you don't get sunstroke. What kinds of clothing are appropriate. Students are used to wearing T-shirts, shorts and sneakers. And that is not appropriate for field work. You need armor. You need good, tough jeans with no holes. And you need cotton shirts with long sleeves to keep the sun off. You will burn to a crisp in half an hour.

JC: You took student groups out to Montana for quite a number of years.

RES: Yeah.

JC: Did it ever get routine or boring for you?

RES: Never boring because the problems were never the same. They were always solvable and the students always learned how to cook, how to cope with weather, why I insisted that they get not straw hats, which they always wanted to get, but rather a white felt cowboy hat with a big brim, why you always wore long sleeve shirts and why you never wore shorts except maybe for going in swimming. Why you wanted boots rather than sneakers or sandals.

JC: Did any of your student groups ever find anything truly spectacular?

RES: Oh, sure. I can recall Ashok finding a complete articulated soft-shell turtle. Missing one foot, but otherwise, everything else present from the tip of the tail to the tip of the snout. Quite a find.

Completely articulated. And all sorts of stuff was always found.

JC: You stopped going to the field in about 1990. And that was because of mobility problems.

RES: Yes, it really was.

JC: Had field work lost its luster for you at that point? Or were you truly sad not to?

RES: I was sad. I missed it. My spring quarter field trips, end of spring quarter field trips were always a way of getting rid of cabin fever which I had in a serious way by that time of the year.

JC: But you have continued to take students along the southeastern Minnesota trip?

RES: Uh huh.

JC: And that trip, in a lot of ways, began with Charlie Bell's ..?

RES: Yeah. It began with Charlie Bell's work and now it is down to essentially training how to collect fossils. But I don't have to go farther than road cuts or quarries. I am not hiking a mile across the badlands and climbing 200 foot hills.

JC: Are you taking students to some of the same sites that Charlie Bell used?

RES: Oh, yes. Some of the sites that Charlie showed me on that first week's trip are in RI 35 and I take them on every trip there the best localities that existed. Charlie knew what was good. And so do I.

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Interview with

Robert E. Sloan

by Joe Cain

Session 14

A series of interviews with Professor Robert E. Sloan regarding his career, the recent history of paleontology, and the life of an American scientist in the second half of the twentieth century.

These interviews were made possible with a generous Mellon postdoctoral fellowship from the Department of History of Science, University of Oklahoma.

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#14 Tape 8, Side 1
March 14, 1996

RES: Professor Robert E. Sloan, Department of Geology, University of Minnesota
JC: Dr Joe Cain, interviewer

JC: There were a few things that we missed when we talked about Bob's time at the University of Chicago, and we are going to pick up on that. In particular, Bob wanted to talk about his experiences with two instructors, Sewall Wright and Alfred Emerson. Go ahead, Bob.

RES: The professors that I was told I really needed to take courses from by my colleagues were Alfred Emerson and Sewall Wright. Emerson taught an ecology course, and Sewall Wright taught the genetics courses and a course called Evolution, which was essentially an analysis of his mathematical studies of population genetics.

JC: Feel free to go on. Who told you that taking these courses would be to your benefit?

RES: Well, Ernie Lundelius had already taken it. And Ralph Gordon Johnson, of course, had taken Emerson's course in ecology, and it was quite apparent that after what I had gotten from Lowenstam I needed more ecology. To put it very bluntly, I don't remember very much of Emerson's course at all. The textbook we used was The Great APPES, Allee, Parks, Parks, Emerson and Schmidt, which was a compendium of all of their examples of ecological principles. It was very strong on examples and very weak on principles. No organization to speak of. And so I think it is not surprising that I didn't bother to keep the notes I took from Emerson's course, and so I couldn't pass them on to Joe and don't remember very much about it at all. He was a mild-mannered gentleman, I took the courses over in the Zoology building. That is really all I remember out of the course. I remember a few examples. Mainly from having read about them in the Great Apes, most particularly, his analysis of the evolution of termites. And the ecological separation of different species of *Peromyscus*, different habitats immediately adjacent to each other in New Mexico. The White Sands National Monument, where the sands were white gypsum, and immediately adjacent sands built from decomposing lava fields which were black. And so you had two very closely related species of *Peromyscus*, living immediately adjacent to each other. And it is like different habitats. That's it!

Sewall Wright, on the other hand, was everything that had been predicted. Sewall Wright was a very mild mannered gentleman who would stand there looking hurt and blinking when his students would not come to the obvious conclusions--obvious to him from the analysis that he put on the blackboard. He would cover the entire blackboard of a 30 foot wide classroom, 3 times a lecture.

JC: With simple math? Or complicated math?

RES: With oh, nothing very complicated. It was nothing beyond algebra. He never got into either differential or integral calculus. A little bit of statistics. It is just that it was perpetual equations. And I was able to follow them really quite well. Joe has my notes on the course, and basically, they consist of transcriptions of the equations that he wrote on the blackboard, together with some cryptic notes as to what these equations actually meant. And it was not that difficult for me to follow the equations. It was simply that I never had occasion to really apply these to paleontology. Nonetheless, I could see how these things were working and how all of these were essentially logical consequences of, among other things, the equations.

JC: By that time, you had read *Tempo and Mode*.

RES: By that time, I had read *Tempo and Mode*. I had read *Quantitative Zoology*, by Simpson and Row, I had read most of the works of the *New Systematics*, I had read *Genetics, Paleontology and Evolution*, and I don't recall when. Yes, I do recall when I was introduced to Sewall Wright's classical 1930 paper. That was actually in Natural Sciences III when I was an undergraduate. It was one of the corpus of readings that we were assigned in an offset paperback book, carefully designed for the course, so I do have not only Bovine's reprint of the thing, but also a reprint that I bought as part of a textbook. This paper, of course, was critical. Because more than anything else that Wright did, it was conceptually useful to a paleontologist. It was useful to Simpson. It was useful to me. And I had had enough geometry at this point so that I will argue with Provine assertively about what Provine thinks Wright meant and what Simpson and Wright thought the illustrations in that paper really meant.

JC: You are talking about the adaptive landscape paper?

RES: The adaptive landscape paper.

JC: What would you argue with Provine about?

RES: This was a topographic map of landscapes that were essentially fitness based on contiguous points. In a two-dimensional representation of a multi-dimensional grid of gene frequencies of all sorts and Provine has argued that it makes no sense at all. I had enough geometry and had stretched my mind enough with the notion of topology and of projective geometry to see the that you could, in fact, generate such a landscape of adaptive fitness and it would make reasonable sense.

JC: So you are also saying that ..

RES: That Provine's arguments are our big criticism. I am sure it will not change his mind.

JC: And you are also saying that there is a historical point here that you actually did understand.

RES: I actually did understand what Wright was proposing in the adaptive landscape paper and the adaptive landscapes of course, were taken over bodily with slight alteration by Simpson in Tempo and Mode. That turned out to be very useful. And all you had to do to make it more reasonable, is essentially to have, in addition to the adaptive landscape, which condenses things into a three dimensional representation, with height being fitness, and the XY axis being some ordering, not necessarily linear, of genetic components. But if you add the fourth dimension, to have a series of these transforming as the habitat changes, then you are dealing with a moving topography in which a gene frequency that was adaptive at a particular stage would find itself on the slope and would have to migrate up into another frequency. And this was a very apt analogy for me. This, I think, is what Simpson was seeing. This is what I was seeing as a result of my abilities to condense multidimensional things into things that I could represent in three dimensions, sometimes with four if you add color in my brain, and that now of course, could be done easily for anybody to see on a computer screen. In modern terms, you could carry this analogy of the adaptive landscape model into quick time movies with changing topographies, and changing shifts of populations as the landscape changed out from underneath them. This was a very apt analogy of what I was seeing in the fossil record.

JC: Now, and you are also saying it was useful to help you conceptualize some of the issues that Wright brought to mind.

RES: Oh, it sure did. It is not accidental that this paper is so widely reprinted. Just recently, I am getting ready to present a paper next month, and I came right back to the 1930 paper of adaptive landscapes, and the set of situations in the rates of change of gene frequency or rates of evolution would in fact be most rapid than those where you have semi-isolated populations with a limited amount of inbreeding and outbreeding possible. This is precisely what produces the most rapid kinds of evolution we see in fossil records, Sewall Wright's course, I didn't bother to remember the mathematics, I don't remember the equations. I have got the volumes of Sewall Wright's works, and I have got Provine's biography, and I have got Provine's collected papers of Wright's work, of course, the collected selections of papers, that is enough. But it provided a visual framework for the pattern of evolution that went far beyond the simple phyletic gradualism that was always talked about in paleontological textbooks, and it was the principal reason that Steve Gould got so upset and created this straw man alternative of punctuated equilibrium.

JC: Right. And so it is the combination for you of Simpson's Tempo and Mode, where he talks about Wright and having Wright himself that reinforces this thinking.

RES: Wright was not as bad as some students have said he was. Some students said he wrote with a right hand and erased the first part of the equation with his left hand. It wasn't quite that bad.

JC: There are two things I want to ask you about the class itself. One is you described some of your other classes at the Field Museum where they were extremely informal and you were encouraged to raise any and every point. Was Wright's class the same way? Or was it?

RES: No, it was not. Wright was visibly presenting material and you had to spend all your time writing the equations down together with short sentences describing what equations meant so that you could assemble the logical frequency, the logical structure of his arguments. And some people could do it. Some people couldn't. I did well.

JC: And so would it be fair to say that this work was much more like a lecture format than a discussion format?

RES: This was a lecture format, very definitely. A very exciting lecture format. He would bring in examples. I didn't see this, but my friend and partner, Ernie Lundelius, who was one of the groomsmen in my wedding, describes a case where Wright brought in a guinea pig. He was displaying the guinea pig and showing some of the variations in coat color. This particular guinea pig was somewhat more fractious than usual and was scurrying around on the desk and was not about to be quiet. When Wright worked at the blackboard, traditionally he tucked an eraser in his left armpit. And to keep the guinea pig quiet, he tucked the guinea pig in his left armpit. And when he was through and ready to erase some space so that he could put the next equation down, he reached for the eraser and grabbed the guinea pig and started to erase the blackboard with a squeaking guinea pig. Of course, this was one of the tales that went around among all the students of evolution. Whether they had been there or not, I doubt it ever got into print. It deserves to be there, as part of at least the bibliography of Sewall Wright.

JC: (laughter) I didn't say this at the start of the tape, but it is the 14th of March, 1996 and it is about 3:00 in the afternoon at present.

I want to change gears now and talk about some other parts of your research career. When you came to Minnesota and your research program changed, you spent a lot of time working on Minnesota problems and Minnesota materials. You also spent some time working with the Geological Survey in Minnesota. Could you describe some of that work from its early stages on?

RES: The first thing that happened was that I was introduced to the Geology of Southeastern Minnesota by Charlie Bell. The second week of the last time he taught Geology 115 in the summer of 1953, essentially the 19th of July, the first date I was employed by the University, to the end of the week. We essentially went to Hudson and Afton and then to Lake City and on down to a series of exposures along Highway 61 to Winona. And then jumped inland to look at the middle late Ordovician of Fillmore County, which had just been described by Malcolm P. Weiss, who was Charlie Bell's last Ph.D. student at Minnesota. Mac is a long term friend of mine. We have worked together for many, many years. In any event, when I got here, I was expected to do research on

Minnesota problems.

The first problem I chose to tackle, having spent the fall and winter reading up on Minnesota's stratigraphy and paleontology, was the Cretaceous rocks of Minnesota. And I spent about two summers working seriously on this problem all over the state. I quickly found why no one had ever managed to come up with an acceptable synthesis of these rocks. I found a grand total of 150 exposures or useful well logs in the entire state. The longest distance correlation I could make was from clay pit in Springfield to a well three miles out of town. That was it. Every place else the Cretaceous rocks were the result of a long period of extended tropical weathering on bedrock of a wide variety of ages, from Precambrian to Devonian. So local bedrock geology greatly influenced the character of the Cretaceous sediments and in effect, there were several major facies of Cretaceous sediments in different parts of the state. A great deal of relief. We were talking about 2,000 feet of relief topographic relief at the time the Cretaceous seas overran the state, continuous changes in the position of the coastline, the fossils were sparse. I got Rick Pierce to work on the pollen. The leaves had already been worked on. Rick Pierce was employed by the Geological Survey to work under my supervision on field work and then did the laboratory work under John Hall's supervision. This was when John and I first became cooperating professors, sharing students regularly.

JC: Did you get the sense that this was a large scale research program ready to take off?

RES: This was a large scale research project with inadequate information. I wrote it up. I wrote it up badly. The manuscript kept getting thrown back at me and it was not finally published until at least a decade after I had finished the work. And in the meantime, I was suffering a blow to my abilities because I couldn't come up with an acceptable description of what I had seen.

JC: Acceptable to whom? The survey?

RES: To the survey. And in fact, Paul Simms, who ultimately published the thing as survey director some time around 1963 or 4 never did really understand the paper. It turned out to just be a paper done before anybody needed it and it was not until 20 years later that finally a wide variety of economic geologists and U.S. geological survey paleontologists found this thing and realized what an important piece of work it is and by that time the remainders had been pulped. So it is hard to come up with copies.

It did have its influence, but I clearly was going to do something else. One of the things Charlie introduced to me on the field trip was the Carimona and Decorah Bentonites, as they were called at the time. And these represented a pair of isochronic strata with an interval between them that was exactly the same interval of time no matter where you saw it. I noticed on detailed measurement of Paleozoic stratigraphy, because at this point I had shifted to working on the Paleozoic rocks, and mapping.

JC: Before you continue, Bob, you never really said why you chose the Cretaceous.

RES: Because it was the biggest, splashiest, undone problem in Minnesota.

JC: So, as the new guy coming in to the University--

RES: As the new guy coming in, I wanted to have something that was truly spectacular that would create a lot of impact and it did, and no one in the Department or the Survey understood it.

JC: And really the end result of the process that you have been describing is, I am looking at your bibliography right now, and it is Sloan 64, the Cretaceous System of Minnesota finally came out as a report of investigations for this Minnesota Survey.

RES: A full 10 years after the work was actually done.

JC: And that is the manuscript you are describing having a lot of trouble getting through the survey?

RES: Yes.

JC: Can you say a few things about why they were sending it back, what the problems were. Clearly, this is a complicated situation.

RES: The problem was, it was a problem in sedimentary facies variations. And neither Dr. Schwartz nor Paul Sims had ever dealt with a situation in which the stratigraphy was so outrageously variable. The basal Cretaceous sediments, no matter where you are in the state, are usually the only ones preserved. And each is the direct result of the weathering of the immediately underlying bedrock. And the variety of kinds of Bedrock in the state went through every known possible kind of igneous metamorphic and sedimentary rock. And the sediments immediately above them were sometimes called Precambrian, even though they were Cretaceous. Simply because they were regarded, "Oh, it is just a soil developed on the Precambrian", but it wasn't. It was a sediment.

And the facies changes were truly outrageous. And the only way I was ultimately able to solve the problem was to create from scratch a total graphic map of the present altitude of the pre-Cretaceous surface all over the state. Consider this as a surface which was then essentially submerged gradually by rising Cretaceous seas. There were major Cretaceous changes in sea level. I have discussed my analysis of those in a section in the autobiography called "Cretaceous Sea Level Changes."

And having done that, I was then in a position to draw some conclusions. I had two sets of marine rocks, of very different type and very different faunas, but both the fossils were exactly the same age. And from that and the topographic map, I was then able to restore the attitude and shape of the coast line in that particular Cretaceous stage, the Turonian, throughout the entire state of Minnesota. And it was not the simple kind of coast line that had usually been shown in all the old paleogeographic maps such as those Schuchert and Dunbar had put together, or those that Thiel and Stalfer had used in their various papers on southern Minnesota Paleozoic rocks. Instead there were three major promontories, extending clear to the Dakota border and three major bays coming almost to the eastern edge of Minnesota. It was very irregular coast line. And in part, this was because the plutonic rocks of Minnesota were of greatly varying ages. The youngest plutonic rocks had been eroded away, least the oldest plutonic rocks had been eroded away far more and producing this extremely irregular coast line. So that was the Cretaceous. I had stubbed my toe. It took me 10 years to get this thing into a shape where the survey would finally publish it even though the head of the survey couldn't understand the paper.

JC: What do you think this work in the Cretaceous did for you, for your reputation here in Minnesota?

RES: I don't think it helped me at all.

JC: Because it was a complicated problem?

RES: It was a very, very complicated problem and I was writing short at that time, expecting people to be able to see what to me, were obvious conclusions. And not necessarily writing them all out.

JC: You said to me in another context that you were having difficulty as a writer at that point in your career, that you hadn't really learned yet to write well. Did that play a role? Were you articulating the ideas for people understanding what you were trying to say?

RES: I couldn't get the ideas across to Simms or to Schwartz simply because my sentences were long, strung out sentences.

I needed to write a lot. In the long run, the thing that improved my writing, was my writing other than professional writing, starting in...well, I wrote articles on model railroads, starting about 1972. These were short, declarative things describing one small thing, not very complicated at all, illustrated with a couple of historic photographs and a plan I would draw. And as I drew more of those and more and more people became enamored of reading these articles, my confidence in my writing and my skills of writing improved. I also shifted from depending on pen or pencil, paper drafts to doing my own typing. This came about when we had a particularly incompetent student secretary. This was the secretary I was to use for typing when I would give her a manuscript, she would produce a manuscript with two or three serious errors per page and when I would correct the errors and give it back to her for a second draft, she would correct those errors, but introduce at least as many in the rest of the paper. This got very, very frustrating at which point I acquired first an inexpensive electric typewriter. The Department would not give me the use of an IBM Selectric, which was the desirable typewriter at the time, and I used that Brothers electric typewriter for several years until finally we got our first home computer which was a Hewlett Packard 2647A smart graphics terminal with a very simple text editor and a dot matrix printer. And this is where I really learned to type. And so I became computerized by about 1978 or so.

JC: Now in terms of your work on Cretaceous in Minnesota, and your next major project, the K/T boundary problem..

RES: That was not the next major project. The next project was working on these bentonites in the Ordovician of Minnesota and I measured a series of measured sections from McGregor, Iowa to St. Paul and North of Stillwater on the St. Croix, totaling 75 sections of the Platteville Formation with an average spacing between sections of about 4 or 5 miles. This is sufficiently close spacing so that it was possible to see a number of things, most particularly that it wasn't just the two volcanic ash beds, which are now known as the Deicke and the Millbrig but between the Deicke bentonite and the Millbrig bentonite, these were not just the only things that were continuous, but the limestone beds and frail beds in between were continuous. And I would see a six inch shale bed in the Twin Cities came down into a parting plane in southern Minnesota. And in between, you could see this parting plane become first an inch of shale and then a couple of inches of shale, and so on up to the Twin Cities. I was literally able to trace all of the limestone and shale beds near the ash beds throughout this entire 200 mile distance. I then continued with the rest of the Platteville Formation and was able to do the same thing in the whole Formation.

So I started with this isochronic interval and then spread to the whole formation. Which led to interesting conclusions that the Glenwood shale and the Platteville were in part contemporaneous rather than strictly Glenwood always older than the Platteville. In part, the Glenwood is the same age as part of the Platteville. And furthermore, part of the St. Peter is the same age as part of the Glenwood. And in 1972, we had a Geological Society of America Convention and I was working on the Cretaceous by that time. But I finally described what I had done in a paper in the Geology of Minnesota, 1972 paper, and essentially left the Platteville there.

In the meantime, I had had graduate students working on this bed tracing, and particularly on conodonts. Conodonts were a group that my predecessor once removed, Clinton Stauffer, had worked on as a pioneer. Some of his only good work was on the conodonts. But he and everyone else who had been working on conodonts, had been seriously violating the code of zoological nomenclature.

JC: In what ways?

RES: Standard treatment for conodonts, even though people knew that there were several different kinds of conodonts, in the conodont animal, whatever the conodont animal was, because some conodont assemblages of things that were part and parcel of a single animal had been found with all these separate kinds of conodonts in bodily association, always in the same orientation. The standard practice in conodonts was to get a grab sample of rock, extract the conodonts from it, illustrate them and give every single isolated conodont a separate, specific name. And so you would have five or six genera of conodonts and the same number of species. Sometimes more than that, in terms of species, present somewhere in a single species of animal. So you have got multiple names for the same animal, which is a very serious breech of the code. Some of the conodont specialists seriously proposed that there be a series of parataxon in which the code would not apply. And conodonts were a prime example.

JC: They were making a clear methodological mistake?

RES: They were making a clear methodological mistake. Because normally people did not get these conodont assemblage. They got the isolated conodonts. Well, it was quite apparent from some of my first students who were working in the Platteville, that conodonts were a lot more common than one might think. Pete Palmer had taken Fred Swain's place as a visiting professor one year while Fred was off on sabbatical. And Pete was a Cambrian trilobite specialist but who was also a great believer in dissolving rocks to see if there was any insoluble residue that could be looked at and he had a number of classic cases of trilobites that he extracted, and brachiopods that he extracted by etching limestones in formic or acetic acid. Since conodonts are acetic apatite, I don't remember whether I got the idea of etching limestones for conodonts directly or somebody else did, it was in the wind, to steal a phrase from that colleague of yours. Anyway, we did. And as students were measuring a segment and describing it, I would also have them extract a chunk of rock, limestone, dissolve it in formic acid, and count the number of conodonts per 100 grams, at first just a simple statistic about the rock comparable to grain size and grade shape and all the other usual sedimentological properties. This is one additional feature of the rocks. However, thinking about the massive numbers of specimens that came out of these things, we had some horizons near the ash bed where there were as many as 1,000 conodonts per 100 grams of rock.

These are small, tooth like objects about 1/2 a mm in diameter, so there could be a lot of them. And at this point, I began to see that it should be possible just using these very large samples, to figure out on the basis of the assemblages of the loose conodonts that you have got in different beds to see the waxing and waning of several species, one species would be abundant in one bed, and therefore, its component species would be very common and in another nearby bed, that species would be rare. But another species would be the common one. And so by an analysis of large numbers of specimens, you ought to be able to statistically determine what the assemblages really were. And then you could get away from this violation of the code. It would call for a massive reorganization of conodont taxonomy and it ultimately did. And when the first masters thesis of mine strictly on conodonts was done by Willis Thompson, he came up with a set of conodonts that collectively made up the conodont animal for which the prior synonym was *Phragmodus undatus*. And so he had in his thesis a picture of an artificial assemblage of the component parts of the biological species, *Phragmodus undatus* as opposed to the form species, yadada, yadada, yadada, about six synonyms. I knew this was going to not go well in terms of the department.

The department didn't care. It did not go well in terms of professional paleontology and so what was necessary, was a more complete analysis. Henry "Bud" Anderson did the immediately follow-on thesis in the base of the Decorah Shale over a considerable area and came up with the same kinds of species that Bill had. And in the meantime, I was getting ready to do something more elaborate. So far, I had confined myself to about 20 feet, maybe 30 feet, at most, of the 600 feet of middle and late Ordovician rocks in Minnesota. Jerry Weber came along, wanted a thesis, so I put him on the most similar rocks, the Dubuque Formation, in the area that Mack Weiss had very thoroughly done in Fillmore county, and of course, I was very familiar with these rocks, and Jerry did a good job on the Dubuque conodonts from Wubbles ravine. Then in the summer of 1959, I started collecting, from the best exposures I had, a complete set of samples from the top of the St. Peter sandstone, to the top of the Stewartville dolomite. We already knew there weren't many conodonts in the overlying Maquoketa Formation, there wasn't any point in doing those. This would essentially fill in below Jerry's, but provide a single continuous sample with conodonts throughout the entire middle and late Ordovician sequence, which had no breaks in deposition at all. While collecting at Mack Weiss' type section for the Cummingsville, then Member, now Formation, of the Galena Group, I had gotten samples collected through the Platteville and Decorah and was some 20 feet or so into the Galena group, about 200 feet thick, when the ledge I was standing on, a limestone broke out and I fell and smashed my left ankle very badly, dislocated my right shoulder and was out of field work for that time. Rod Bliefuss, who was doing a thesis on the Cretaceous iron ores, which he thought were Tertiary, of Southeastern Minnesota, kindly finished making the sample collections.

JC: This sample here?

RES: 1959. So I had all of these carefully collected samples. Jerry had just finished his masters' thesis, and now it was time to do a Ph.D. thesis. And I suggested to him the most appropriate Ph.D. thesis would be to take this set of samples that I had collected, extract all conodonts, and identify them first by form species, and then using statistical techniques, group the form species into biological species, and so we did. He finished his thesis about 1963 and it was finally published in 1966 as a truly major reference. After he had finished it in 1963, he went off to a meeting of the Geological Society of America, where conodont workers were holding a symposium. I think it was in the northeastern section of the GSA.

And he heard to his great horror, Tom Shopf of Columbia and Walt Sweet of Ohio State, who had been a cephalopod worker, but had switched to conodonts, present exactly the same idea. This was Tom's Ph.D. and Jerry's Ph.D. and they were both dreadfully and woefully down in the dumps. "I have done all this work, and I am not going to get my degree at all! This is terrible!" And then they realized that they were getting a lot of static from people who said this could not be done. Here it had been done three times. Quite independently, none of the three knew the others were doing it. And they had all come up with exactly the same biological species of conodonts at which point, they got together, wrote a joint paper, were thoroughly booed by most of the conodont specialists for the next 5 years, but eventually all of conodont taxonomy was in fact turned on its head and grossly simplified as a result of this tri-part statement that yes, you could do it. You just need bigger samples. Most people would have been satisfactorily pleased to have 100 conodonts to write a paper. Jerry had 20,000. Walt and Tom had equivalent amounts. All you needed were big samples and then you would take care of the problem because the statistical error of prediction becomes very small, the bigger your sample becomes.

JC: Now about this time...

RES: By this time, I had switched to working on the Cretaceous. I still had students working on the Ordovician. The reason I still had students working on the Ordovician was that the Ordovician rocks were close at hand. A masters student could afford to go out and do them with minimal amounts of support.

JC: Within a few hours of the twin cities?

RES: Within a few hours of the twin cities and maybe a couple of weeks of field work, you had enough materials to do a respectable masters thesis. So I had theses continuing to come out on these Ordovician rocks. I was always involved with them, even though I was concentrating most of my efforts on the Cretaceous Tertiary boundary problems, dinosaur extinction, and the Paleocene mammal radiation, which are three different aspects of the same problem.

JC: Now, before we talk about your exploration of the Ordovician yourself, and the Paleozoic materials, let me ask you this. In the late 50s, you said earlier, that you were getting a lot of static from the Department about promotions. This conodont work strikes me as quite innovative. Yet the Department is telling you that you are not doing innovative work.

RES: Yes.

JC: What is the relationship?

RES: Because what I was doing for the survey to get summer employment was basically mapping where all the individual Paleozoic rocks of southeastern Minnesota occurred. These are gently dipping rocks, but the area is not without structure, and pre-existing maps were truly awful.

JC: You described that in your autobiography.

RES: Yes. Stauffer routinely mis-identified rocks as much as 300 feet in position and got some stratigraphic horizons wrong on the 1935 state geological map by as much as 40 miles. The horizons were generally correct along Highway 61, where it is all Cambrian, and along Highway 52 and in between, the Lord knows, he just sort of eyeballed things in.

JC: So the Department, at the time when they were talking about your future...

RES: I was literally mapping these rocks where they really occurred, finding the structures that were present in southeastern Minnesota, this was not regarded as major precedent-setting research. It is hack work and I ought to do something more spectacular.

JC: And because your students were doing the conodont work, you weren't getting the credit for it. Is that correct?

RES: That is correct. Because one of the things I refused to do, throughout my academic career, was ever include my name as a senior or junior author on a student's thesis.

JC: Why is that?

RES: I didn't think it was right. I still don't think it is right. And I get very huffy where the professor's name gets stuck on one that is clearly the students' work. Sure, the professor provided the impetus and some direction. Didn't write the paper, didn't do the work, shouldn't be given credit for it. That is what you are expected to do as a professor. I know I am a recalcitrant reactionary in this matter. I guess part of it is I have read *Cope Master Naturalist*, by Henry Fairfield Osborn, the most noted of my academic ancestors, and *Othnial Charles Marsh, Pioneer in Paleontology*, by Schuchert and Levine at Yale. And Osborn was able to say some nice things about Cope. He wasn't always a schnook. Marsh was always an ill-tempered tyrant who took all the credit for the work that his juniors did. Osborn did the same thing. I wasn't about to do that. It undoubtedly had an impact on my salary because these things did not appear on my bibliography with my name attached to them.

JC: So when salary reviews, or tenure reviews come up, they look at your bibliography and don't see this other material.

RES: Yes. But somehow, I just couldn't do it.

JC: You also came up though, Bob, in a tradition field museum where credit wasn't doled out by authorship, everybody knew what people were doing and if you picked up that as part of the working culture of a scientist, you could easily see why you wouldn't push credit. And you also may have been in a situation where authorships weren't immediately relevant to your status as a scientist.

RES: I suspect so.

JC: That is putting words in your mouth.

RES: It is putting words in my mouth, but it is a reasonable set of words. So I went at that point, and I physically switched myself to Cretaceous studies and had my students continue to work on the Ordovician under my direction. But I put my own research on Cretaceous Paleocene problems.

JC: We have about 2 minutes left on the tape. Let me ask you a quick question about the accident you had with your ankle and shoulder. How did that affect your, it obviously affected your physical ability to move to the field. How did it affect your research?

RES: The first thing it did was produce very interesting problems. This happened late in September. Classes were to begin in 2 weeks. I was completely laid up. I was scheduled to teach my usual fall quarter courses, Introduction to Paleontology, and Historical Geology. If I did not teach them, I would not get paid. It was kind of the Minnesota Geological Survey to pay my medical bills, because there was no such thing as University Faculty Health Insurance at the time. So the Survey paid my medical bills, but in order to get paid for fall quarter, I had to teach those classes. Pillsbury Hall is not handicapped accessible. I could not even get in the building.

JC: So what did they do?

RES: When I got home, which was about the time fall quarter began, I made arrangements for one of Fred's Ph.D. students, Richard Benson, who was working on ostracods, to live with us and drive me in my Studebaker station wagon, to the University. I managed to get an office in the Bell Museum where there was an elevator, and I managed to get temporary parking rights for my car in the Nolte Parking Lot.

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Interview with
Robert E. Sloan
by Joe Cain
Session 15

A series of interviews with Professor Robert E. Sloan regarding his career, the recent history of paleontology, and the life of an American scientist in the second half of the twentieth century.

These interviews were made possible with a generous Mellon postdoctoral fellowship from the Department of History of Science, University of Oklahoma.

#15 Tape 8, Side 2
March 14, 1996

RES: Professor Robert E. Sloan, Department of Geology, University of Minnesota
JC: Dr Joe Cain, interviewer

JC: We are continuing our conversation about the fall that Bob suffered and how that affected his research.

RES: All fall quarter, I taught these two courses in the Bell Museum out of a wheelchair in the small auditorium down at the basement. I could use the elevator to get from the basement of Nolte up to the third floor of the museum where I had an office near Dwayne Warner and Bob Dickerman. Then I could wheel myself into the bathroom which was easier to get into than it might have been in other buildings. It was not yet what you would call handicapped accessible and then I could take the elevator down to the classroom. I presented the lectures for this course using an overhead projector with my right hand strapped to my left shoulder, and drawing and printing things on overhead films on an overhead projector in a small auditorium, using my left hand. I am right handed. Fortunately, I have had enough practice with my left hand, so I could cope. And I did. It was not really until Winter quarter that I was able to get into Pilsbury. And because I could teach in this fashion, it meant that I was in fact, paid for this period of time. Dick lived at home with us. We gave him a place to sleep. He did not get charged for any rooming and he got his meals free in return for providing chauffeur service.

JC: Now you made a transition in your work back into Paleozoic after you had gone through the K/T boundary problem. Can you describe that transition?

RES: Yes. The transition came about mainly as a result of the work of my colleague, Dennis Kolata, of the Illinois Geological Survey. Dennis and Warren Huff of the University of Cincinnati, had worked very specifically on tracing these Bentonites throughout the eastern half of North America. People had started tracing Bentonites back in the 1920s. In fact, Sardeson, who was Stauffer's predecessor, had done some rather spectacular work on these in about 1926. But between the work that Dennis had done, the prior work by Templeton, and Wilman, on these middle and late Ordovician rocks, in Illinois, Wisconsin, and the work we had done here in Minnesota, between Mac Weiss and I and all of my students, I kept talking to Dennis. You know, we really ought to pool all this stuff and come up with a major symposium on the middle and late Ordovician of the upper Mississippi valley.

JC: About what year was this?

RES: Oh, we talked about it probably in 1984 and 1985 and really decided to get busy on it in 1986.

JC: Did you do a symposium?

RES: Yeah. This was in the spring meeting of the North Central Section of GSA in 1986. At this time, actually in 1985, I was called into Peter Huddleston's office. He was the chair of the department and told that we had accepted as hosts of the North Central Section of GSA, for the spring 1987 meeting. And I was going to be the vice chair and the program chair. I said, "Fine. I will do it if in the process, we can put together this symposium on the Middle and Late Ordovician of the Upper Mississippi Valley".

JC: Why do you think you got picked?

RES: I don't know. I really don't know. I suspect because I went to more GSA meetings than anybody else in the Department except Peter. Most of the people in the Department did not go to GSA meetings. Instead, they went to a competing society, the American Geophysical Union, and here we were going to be hosting a GSA meeting and at the same time, there had better be some people who would go to these meetings from the Department. And so Peter gave me the chore and I took it on. We got the bid in April of 1985 so I went immediately to the 1985 North Central Section Meeting and later to the 1986. By the 1986 meeting, things were well in hand. Dennis and I had started accumulating all the bits and pieces that had been finished but not published. There was the classic Templeton and Wilman stuff from Illinois and Southwestern Wisconsin. There was the Mac Weiss stuff in Fillmore county, and there was now my geologic map of the St. Paul sheet, which was basically all of southeastern Minnesota.

And I had a couple of master's theses that had never gotten published. Most particularly, David Des Antel's thesis and I knew of a considerable corpus of work that a pair of amateurs in Northeastern Iowa had done. These were Calvin Leverson of Riceville, who was the postmaster of the town, and Art Gerk, who was a sheet metal siding salesman from Mason City. They were amateurs who decided they wanted to collect fossils. They found that in order to collect fossils, they had to know the stratigraphy, because there were some horizons that were extremely rich in the fossils they wanted and others that were not. So in effect, they did the geology of the transitional rocks between those of Templeton and Wilman and of Mack Weiss. And they had published a couple of papers in the transactions of the Iowa Academy of Sciences, a guidebook paper or two, but their work was far more extensive than anybody realized.

JC: So you and Dennis decided that you were going to use this opportunity to do something truly big.

RES: Yes. Truly big. To essentially organize all the published and unpublished data on the middle and late Ordovician rocks and fossils of the upper Mississippi Valley, namely, everything from Rockford, Illinois to the Twin Cities. In Illinois, Iowa, Wisconsin and Minnesota. For a variety of reasons I won't go into right now, there had been an agreement to disagree between Iowa and Wisconsin on the one hand and Minnesota on the other. Ask me about it later, because it is a story that needs to be told. It hasn't been. But in any event, I had an unpublished thesis. We had these detailed measured sections by Leverson and Gerk which were drawn at a scale of 1/4 inch to the foot as graphic sections, and had data down to a fraction of an inch on position of all the beds. Any information on what fossils occurred in which beds, same sort of thing from Mac Weiss.

I had heard about this Ph.D. thesis from Harvard in 1963 by Larry DeMott, who was a professor who had just retired from Knox College at Gailsville. Dennis, of course, had a copy. The work had been funded by the Illinois Geological Survey. Dennis sent me a Xerox copy of the text and bad fourth generation Xeroxes of the plates. I read this thesis and realized that this desperately needed to be published, because it was the first adequate treatment of trilobites by distribution in these rocks since a very poor one in 1927. And nothing was comparable to it and it was truly spectacular. So having gone through this and found a thesis at Duluth that had been finished, but never granted a degree for, the kid just up and left without taking the degree. The thesis was a good job, it was vouched for by Dave Darby at Duluth, but it was the only treatment of trace fossils that had ever been done in this region. And so I called DeMott, asked him if he would like some assistance in getting this thesis published. He explained that he couldn't do it, he hadn't done it in 22 years and now that he was retired, he had retired for health reasons, he had had one stroke and a heart attack and was not in great shape. He was an invalid. But he still had the thesis and he would be happy to turn it over to me.

JC: So he wasn't going to do it. And if anybody was going to do it...

RES: If anyone was going to do it, it was going to be me.

JC: And you thought it needed to be done.

RES: It needed to be done. At the same time one of the most useful but least studied parts of Stauffer and Thiel's Paleozoic and Related Rocks of Southeastern Minnesota were the stratigraphic unit by stratigraphic unit description of what species of fossils had been found in Minnesota. And this was in fair detail. I decided that an appropriate thing to do would be to produce a composite stratigraphic section for the entire upper Mississippi valley. And enter Stauffer's lists of fossils class by class, order by order, and then circulate them to colleagues who knew these fossils from these rocks for any additions and changes.

JC: This is well prior to the meeting.?

RES: This is well prior to the meeting. I sent out a stratigraphic list of the units and showed how the units corresponded and included the lists from Stauffer and Thiel, and they added more.

JC: Were you hoping to get a consensus?

RES: I was hoping to essentially summarize all the data in very accessible form. There was no such summary of these rocks, for this area or any other. I wanted something that would be a reference work that would be lasting, that would provide a basis for work and that would suggest additional work to people who would come later. And of course, it did. Larry Demott's thesis, he had the plates, all 13 of them. They didn't need changing. I spoke to Fred Shaw, who was one of Larry's fellow students at Harvard under Whittington. Fred agreed, with the help of Ron Tripp, a retired executive who was working out of the Royal Ontario Museum, and had done a tremendous amount of work on trilobites in Scotland. They were North American trilobites in Scotland, because in the Ordovician, Scotland was part of North America. So Ron and Fred proceeded to revise the taxonomy of Larry's trilobites, and abbreviate it and bring it up to date, so that the plates would be of use. And I took the critical sections of his stratigraphy and biostratigraphy and boiled it down to an introduction. Larry had a stroke. He knew the thing was coming on and was going to come out. He never recovered consciousness, so he never knew the thing was published. But his family was of course, very grateful getting this thing out. All bits and pieces came together. I rescued a paper that the survey had not published by Fred Swain and his students on ostracods, they published some but wouldn't publish this one. It was just too much taxonomy. Walt Sweet did an analysis of conodonts in a particular interval. We published Bill Rice's master's thesis on Decorah brachiopods which is of Ph.D. quality except he didn't have the sufficient credits of course work to get a Ph.D. degree. It is a big master's, definitely worth publishing. So we wound up getting the whole thing pulled together, we created at the two-day meeting, a one-day field trip in the Twin Cities before the meeting, attended by about 90 Ordovician specialists from all over the world. Two days of the meeting in which we had four sessions on Ordovician paleontology.

JC: Which is unusual?

RES: Which is very unusual. And we followed it with a two-day field trip to Southeastern Minnesota and adjacent Northeastern Iowa. For another 85 people, complete with a Guidebook, we used Leverson and Kirk's detailed measured sections to provide illustrations for all the outcrops we were going to, so there was very precise graphic stratigraphy, a description of what fossils we were expected to find, where. Where we didn't have Leverson and Girk sections we had our own and drew them up at the same detail. And published the Guidebook for the field trip and Report of Investigations 35. Middle and Late Ordovician Lithostratigraphy and Biostratigraphy in the Upper Mississippi Valley. This was not an official international Ordovician Symposium, but it was an unofficial one. And this volume has about the same currency in Ordovician literature as the symposium for the now 8th International Symposia on the Ordovician system.

JC: For the tape, we commonly refer to this volume as RI 35 so that is what we are referring to. What do you think has been the impact of RI 35?

RES: This provided a complete description of half of the Ordovician for platform sediments in North America. There was no such description prior to RI 35. It was there in bits and pieces but nobody put it together. RI 35 synthesized the whole thing and put it in a form which could be used for a variety of purposes. As a last effort in the assembly of RI 35, the assembly of RI 35 is a story that needs to be told in itself. I requested all the people whose papers were going to go into it, to submit their manuscripts on disk as well as on paper. We got all sorts of disks. 5-1/4 inch disks, DOS, Macintosh, every possible word processor imaginable. A few papers had to be re-typed onto disk. I typed some of them myself. I typed the final version of the Trace Fossil paper from the unsubmitted thesis. Because we had all of the bibliographic citations for each individual paper, we simply pooled them all and had a common bibliography for the whole volume. The University press converted all of these to the same word processing program, and sent us galleys which we then corrected and pasted up into pages. And I literally was doing the paste up of the thing and drawing art work for this fool thing. I got the last paper in in the middle of February.

The volume, which is about 130 pages, appeared in print the second day of the meeting at about May day. As a last effort in putting these things together, I had converted Walt Sweet's composite standard section of conodonts for the middle or late Ordovician, which he described in 1984 into a chronological, numerical time scale based on these rocks. And the reason we could do it is that we knew from his composite standard section, exactly what the level of the Deicke or Carimona bentonite was and the Deicke bentonite had in fact been radioactively dated in about 6 localities in Tennessee, Kentucky and Virginia. So we had a precise numerical age with a precision of plus or minus million years for that point, which we knew in these rocks very well. We had another date from Silurian graptolitic shales in Alaska for the end of the Ordovician and the beginning of the Silurian, and with two numerical dates and this composite standard section, which essentially averages out all the differences in rates of accumulation of these rocks, all over the continent, we could provide excellent numerical estimates of the precise age of all the stage and age and series boundaries in the Ordovician. And so as the very last thing in the volume at the very end, there is an Ordovician time scale, showing the calculated ages of the standard international ages and stages based in England and those in the United States. And this has not changed in a decade. It is within a million years of being right all the way up and down the column. A very, very useful thing. Well, the meeting came and went and I got kudos from all sorts of places from my colleagues at least, if not from my Department.

JC: Your colleagues outside the University?

RES: My colleagues outside the University. It has always been a situation where my Department has never appreciated the worth of what I have done. And my colleagues at other institutions have.

JC: Why do you think that is is the case?

RES: In large part because of the extremely rapid growth in the Department that took place during the time Tibor Zoltai was the chair, immediately after the Cloudian era. Cloud completely upset the departmental structure by giving it a shake in every possible way. And then he was essentially driven out of the Department. We had hired Thane McCollough be the department chair, in April or May, and he didn't show up in September. He sent a letter or resignation in and never appeared. And we were suddenly without a chair.

JC: What were the circumstances of that?

RES: I have no idea. But if he didn't want to come here, it was just as well he didn't. Except that we certainly needed a chair and Tibor Zoltai who was then an assistant professor, about to become tenured, was a mineralogy professor, did become the Department Chair about 1963 or 1964. You have got the precise dates in the autobiography.

JC: Right. Yes.

RES: Tibor then used every trick in the Department Chair's Handbook, legal and viable or unethical and illegal, to increase the size of the Department. He wanted to get a world-class geology department in the quantitative aspects of geology, meaning mineralogy and petrology and perhaps geochemistry and geophysics. Let structure and stratigraphy and paleontology and hydrogeology go hang. And from this point on the Department has been grossly unbalanced in the distribution of its fields. When the Department was a small department, we had a tradition of every graduate student working on a Ph.D. having to take at least one course from everybody in the department. You can do this when there were 7-10. You can't do it when there are 20. Tibor raised the department from 10 or so to 20 in a matter of about 3 years. Completely stood the Department on its head, pulled a variety of dirty tricks, not all of which I am going to talk about. And grossly distorted the character of geology in Minnesota.

JC: That would require a considerable amount of University support. How did he arrange that?

RES: By trick and sly endeavor. He would hire somebody on soft money from research grants and then somehow sneak it past the dean and regularize it as a regular tenured position. And he simply kept doing this over and over again. And as the Department became more unbiased, the groups toward which it was unbiased or towards which it was biased, egged him on. And greatly changed the distribution of fields and distribution of faculty.

JC: So the politics within the Department changed rapidly.

RES: The Departmental politics were outrageous. This was the most unpleasant time to be in the Department. If anything, it was even more unpleasant than the shenanigans that went on at Chicago under Newhouse.

JC: And to be...

RES: And the result is the unfavored less quantitative aspects of geology were essentially restricted to one member per subject. One faculty member per subject. Olaf taught all the hydrogeology courses and in fact, produced twenties of graduate students, and Olaf's teaching load and salary did not reflect his contributions to the Department.

It was very difficult for those of us to get tenure. In my case, it took...well, I had tenure, but I got promoted to Associate Professor just as Cloud left, and it was not until Tibor's era was almost over that I was able to provide enough demonstration of my international prowess as a researcher to be granted my full professorship.

JC: What got you that international prowess?

RES: Bug Creek. So I have had these two threats, the Cretaceous, and the Ordovician. Always been active in both of them. Sometimes more active in one than the other, but always in both.

JC: And in the basically the mid 1980s you got seriously involved in the work on trilobites?

RES: Yes. It was after RI 35 and the precise reasons were that Bill Rice and Dick Benson and another student that I can't think of at the moment, prevailed on me to give Advanced Invertebrate Paleontology. It was a course that was on the books. They wanted more paleontology. And so they prevailed on me to offer it as an additional course, a third course, in winter quarter. And so I did and I chose the Stratigraphic Distribution of Trilobites in North America.

JC: Why did you choose that?

RES: The reason I did, was that I had this time scale. I had acquired all of Larry Demott's trilobite library. I had the background of working through Demott's thesis. I knew what the references to trilobite were, it was not that difficult to read enough papers to find all the critical trilobite papers of the last 60 years, and so we proceeded to extend the kind of analysis of stratigraphic distribution of fossils that we had for RI 35 to trilobite. The trilobite chart in RI 35 was a very preliminary one based on a very limited sample. And I had this Ordovician time scale, based on Walt Sweet's composite standard section, the same sections from which the conodonts to produce that composite standard section. I had also produced trilobites and these trilobites had been described all over the country and so we essentially spent the quarter reviewing Ordovician, Litho and Biostratigraphy and searching out the trilobite papers that formed the basis for these things. And we would find the papers that described the middle Ordovician trilobites of the Shenandoah Valley Region, which was this whole series of papers by Whittington.

The trilobites that described the early Ordovician trilobites of Utah and Nevada and this was Rube Ross and Leigh Hintze. And the Newfoundland, the Scottish papers, papers from all over the continent as it was in the Ordovician. And we had this composite time scale. We simply started finding the first and last appearances of each genus of trilobites of all the genera of trilobites in the Ordovician of North America. We got quite far into it during the course of winter quarter and produced a preliminary draft. Just at that time, HyperCard had been announced by Apple as a major graphic database system that could easily be self-programmed by anybody who was not a computer programmer. And we got a copy of it. I saw some examples in the Mac magazines of various HyperCard stacks that organized a certain batch of data in a neat way and looked at a few of them.

And at this point, the university set up an arrangement with Apple in which Apple would provide a little bit of funding for faculty members who would write computer programs for teaching purposes. Didn't say anything about HyperCard. But I had this data on Minnesota Ordovician trilobites. So knowing what I knew about HyperCard, and on our old Macintosh with two slot floppies and no hard drives, I wrote a quick and dirty HyperCard stack on the Ordovician trilobites of North America, using the data that I accumulated in winter quarter, submitted this as a proposal to the Minimac project, Minnesota Macintosh, and was awarded a Macintosh SE and 1/2 a scanner and some software. The Department paid for the other half of the scanner and also paid some Xeroxing costs for getting Xerox copies of the trilobite literature that I didn't have. And gave me a student to go Xerox the stuff. So I got the grant, got the computer, and had some programming assistance from a Minimac programmer, which led to the first stack on Ordovician trilobites. By the time I had done this, it was apparent that something more was possible.

JC: Something more in what sense?

RES: More elaborate, and the Fifth International Symposium on the Ordovician system was scheduled for 1988 in St. Johns, Newfoundland. And so I put together all the data I had on stratigraphic ranges of all the trilobite genera in the Ordovician in North America. I first presented it in the spring meeting at the North Central Section of GSA, I got very minor criticisms on it. Then I wrote the paper, presented it again in June in Newfoundland, and it was immediately accepted for publication. Eventually it came out in 1991. One of the perpetual problems is that paleontological papers somehow take longer to produce, between finish writing and printing than do the quick and dirty lots of data and very little or less thought kinds of papers that the more quantitative aspects of geology require.

JC: Right.

RES: So it eventually came out. And I have always had about a 3 year backlog between submission and publication. It is the bane of paleontologists.

JC: What else of your trilobite work do you think stands out?

RES: Well, at this point, I was suddenly recognized as a trilobite specialist. I had carefully organized the graphics for this paper presented to North Central GSA and then in a more elaborate version, at Newfoundland, in such a way as to literally document the adaptive radiation of Ordovician trilobites. And I had worked up in great detail, because of the composite standard section, the precise chronology of all of these trilobite genera. So I could literally now finally provide for invertebrates the same kinds of measures of rates of evolution, rates of generic duration, extinctions, and radiations that Simpson had suggested and I had documented, for the Paleocene and for the Late Cretaceous. So now I could do it for invertebrates in the Ordovician. While I was at it, I continued it into the Cambrian of the original book in which the composite standard section was presented as an idea used, Pete Palmer's, Allison R. Palmer's pioneering work on the Cambrian of the Llano region of Texas as an example. This was thoroughly published. I had the data for the Upper Mississippi Valley and for a number of other places in the late Ordovician, so I could push the data a number of genera of trilobites back into the Cambrian, as well as the Ordovician. So I had some quantitative measures. I had some extinctions. I had some neat conclusions about relative durations about trilobite genera as a function of the time since the last major extinction. In other words, what was going on in radiations. So this was still another example of the kinds of principles I had been striving to thoroughly document so that there would be teaching examples for students to look at.

JC: About when did you see that? About when did you determine that this was going to be a case to explore for its larger theoretical implications?

RES: Winter quarter, 1988. Which is of course, not very long after RI 35. Because I started to do this thing right after RI 35 came out, or as RI 35 was coming out. So one led progressively right into the other. At this point, I had to start looking at research projects in which my leg problems would not be limiting.

JC: Your ankle and...

RES: My ankle and knee are very debilitating. And I could no longer do the field work and collect the new materials that I needed to come up with new ideas for good demonstrations of old ideas to counter some of Steve Gould's shenanigans because I simply was not physically able to do it. On the other hand, the trilobites, I could work with.

JC: Let me ask you a question about mobility. Since I have known you, you have had an extensive network of amateurs in the positive sense, people who weren't employed as professional paleontologists, but who were excited and did a lot of collecting. Since I have known you, you have had an extensive number of amateurs. Is that a function of mobility in the sense of did you increase your contacts because you couldn't be doing field work? Or had you had them all along?

RES: I had them all along. When I was doing the major Bug Creek explorations in 1963, 1964 and 1965, I made a point of cultivating amateurs and asking them if they knew of anything like Bug Creek that would provide localities.

JC: These are people out in Montana?

RES: These are people out in Montana or in the Dakotas. The North Dakota Paleocene thesis came about strictly because Steve Lund had done as a science fair project a major collection of Paleocene mammals near Judson, North Dakota. He had had bad experiences in college. I managed to get him in at the University of Minnesota. He brought his collections with him. He didn't work on them himself. Instead, he was shifted to magnetic studies, but he gave me the collections and the locality information which I developed into Dick Holtzman's thesis. Virgil Carmichael of the Northern Pacific Railroad gave me a collection of fossils he found while coal mapping for resources on Northern Pacific lands in southeastern Montana. And this developed into the Olive locality. I had specifically gone to Circle slides in tow while in the field, to give a free presentation to anybody in town or in the neighborhood who wanted to come and find out what we were doing. I showed them pictures of what we were doing in the field, why we were doing it, what the kind of animals really looked like, and in the process, several people gave me localities, one of which turned out to be the locality on the Glen Waller farm known as the Circle locality. The amateurs in Ft. Peck kept giving me localities. So in the long run, we spent more of our time processing localities that other people had given us than we did actively looking for localities of our own.

JC: So there was a prospecting function?

RES: There was a prospecting function. And they were usually very pleased to be part of this sort of thing.

JC: I also know you well enough to know that you are interested in these people for the stories they tell, not just as resources.

RES: They are interested in my stories too. I will never forget Don Beckman's description of what happens whenever anybody showed me a specimen. "He would tip his hat back, he would put his pipe in his mouth, fire it up again, take a couple of puffs, then he would ask, "Where did you get it?"" At which point I would tell them what it was and what its age was. But there was always the "Where did you get it?" (Laughter)

JC: The role of collectors, is an important, and I think not a very well explored one. When you are involved in a project, say the trilobites, did you find yourself basically sending out feelers through this amateur network, "hey, I am working on this."

RES: Yes. Clearly I did that. There was the initial set of some 400 specimens that Larry DuMott had used. We had another 400 or so in our collections, for which we had reasonable data. Leverson and Gerk had of course, very extensive collections of the trilobites from Northeastern Iowa, adjacent Wisconsin and Minnesota, including horizons that Larry DuMott hadn't even thought of looking for trilobites in. There had been a major change in the rocks, mostly as a result of subsequent later metamorphosis. Most of the high rocks in Illinois were dolomites and not very productive. When you got into Iowa, the higher rocks were limestones and you could produce fossils abundantly. Between Leverson and Gerk, and their friends, Glen Crossman and Brian Gossman, I added another 800 or so specimens with very precise stratigraphic data. So at this point, I have got something in the neighborhood of 1600 specimens throughout 16 million years of time with very, very precise information. And I can talk about the kinds of evolutionary changes in the trilobites. There are cases of both punctuated equilibrium, where one trilobite goes extinct, only to have an isolate from the ancestral population from some locality spread over the whole region to take its place, so there is a punctuation. And there are also cases of phyletic gradualism. They both exist in the fossil record.

JC: One of the things that you have said a number of times today is that the punctuated equilibrium people are up to a variety of shenanigans. Now it is not the case that you have rejected punctuated equilibrium completely, because you just, for example, said that there are cases where that takes place. That said, what do you mean when you say shenanigans?

RES: The particular cases that served as the examples for Steve Gould and for Niles Eldredge, ..

JC: In their original work?

RES: In their original work, describing punctuated equilibrium, they each came from different groups of organisms and came to the same conclusion, they were precisely the set of circumstances in which you would expect punctuation rather than gradualism. Niles was in the case of trilobites, particularly in phacopids and particularly concerned with the number of facets in the eye This is a relatively small number and it is an integral number. So it is not surprising that one might expect punctuation. Steve Gould's studies were originally based on his work on Bohemian land snails. And Bahamas is an environment where the land snails can only exist on the Bahamas in interglacial stages, because the Bahamas are completely swamped and covered with shoal water in which the snails can't exist during interglacial stages. It is in glacial stages they can exist, and in full interglacials, they can't, so there is episodic extinction and re-colonization. This is precisely what punctuated equilibrium is all about. And so it is not surprising that he should come up with the idea on that basis.

JC: What do you mean by shenanigans?

RES: They, then Steve Gould took many of the ideas from *Tempo and Mode,* recast them in a new terminology of his own invention, and then proceeded to declare it as a new idea.

JC: And so the material in *Tempo and Mode*, you mean quantum evolution.

RES: Yes.

JC: Right. And so is it the case that what is bothersome about what Gould is doing is the...

RES: The intellectual dishonesty.

JC: .in claiming that it is novel.

RES: Yes. I am fairly stubborn about that. I am as stubborn about that as I am about not putting my name on my students' work unless they have totally abandoned it. I did rescue one thesis for RI 35 and I will probably rescue one of my Ph.D.'s thesis and publish it jointly with him. But it will be a rescue job.

JC: And what do you think the affect of the punctuated equilibrium gradualism debate argument, something, whatever you want to call it, what affect has that had on your thinking about evolution?

RES: Not much. Because I had seen both modes right from the beginning. Steve Gould set up a set of straw men which you could knock down, and said, "See, there is nothing to phyletic gradualism." which was the only mode that anybody talked about. One of the straw men was that evolutionary rates are constant. Now people who are really students of evolutionary theory, except maybe molecular biologists, have never seriously argued that evolutionary rates are constant.

And I certainly had examples of 20 fold differences in evolutionary rates in a short interval of time in the same group of critters.

JC: And so the punctuated equilibrium bishops, in a sense, would set up a straw man and said, "People used to believe that evolutionary rate was constant and it was slow." And then they come along and say it is not constant, and it is not slow.

RES: In fact, it is zero, except when it is very fast. And neither is the real situation.

JC: What effect do you think the...for a while, punctuated equilibrium had almost a religious fervor to it.

RES: It still does. The vast variety of invertebrate paleontologists and some vertebrate paleontologists religiously believe in punctuated equilibrium, because they have never really looked at a long term sequence of evolving organisms. I am a weird paleontologist. This is what I have done. I have looked at long term sequences of the same critters. And sometimes I have seen things that are clearly punctuated, and sometimes I have seen things that are clearly gradual. And to some degree, the differences between these are based on the mode of reproduction and to steal a term from Sewall Wright, the neighborhood size. If the neighborhood is small, phyletic gradualism is routine. That is if the working population is relatively small. If, on the other hand, the neighborhood size is very large, and the possibility of inbreeding can cover a sizable area, you have essentially very slow rates of evolution until you have an extinction. And the new species that replaced the extinct species, are essentially derived from an isolated peripheral isolate. The sort of thing that has been shown by Sepkowski and Sheehan as the onshore/offshore phenomenon in marine rocks. New types tend to be generated in on-shore areas, and eventually survive only in off-shore areas

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Interview with

Robert E. Sloan

by Joe Cain

Session 16

A series of interviews with Professor Robert E. Sloan regarding his career, the recent history of paleontology, and the life of an American scientist in the second half of the twentieth century.

These interviews were made possible with a generous Mellon postdoctoral fellowship from the Department of History of Science, University of Oklahoma.

#16 Tape 9 - Side 1
March 14, 1996

RES: Professor Robert E. Sloan, Department of Geology, University of Minnesota
JC: Dr Joe Cain, interviewer

JC: It's about a quarter until five in the evening. Because this is the last tape that we'll be doing in our interview this session, I wanted to ask Bob some larger picture questions and get him to reflect some on the legacy on his scientific career.

Bob, looking back over your career, what do you think are your scientific legacies?

RES: My scientific legacies have been in many different fields, but they've all been concentrated on basically the same thing. Stressing that evolutionary rates are anything but constant, the close relationships between extinctions and radiations of something else, the development of ecological victors in time, and changes in evolutionary rate. I have provided the kind of documentation for what Simpson spoke about in *Tempo and Mode* in many different subjects, and have had an impact. Sometimes its been acknowledged, sometimes it hasn't. The work on conodonts, for example, the acknowledgement has basically been in my students acknowledgements in their published papers that they owed Robert Sloan. So did a lot of other people. So I have documented varying rates of evolution, have established a standard set of events that happened during extinctions and radiations.

One of the things I did talk about is the distinction between adaptive radiations and the phyletic separations that our cladistic friends talk about. Artiodactyls are differentiated from other ungulates, mainly on the basis of the development of the double pulleyed astragalus and it's a relatively rapid transition. But, in fact, if you look at what we do now about the history of the ungulates from the work that I did in the Paleocene and Cretaceous, it is quite apparent that the first phyletic separation between say Perissodactyls, whales on the one hand, elephants and sea cows, and Artiodactyls on the other, was a twenty-five percent difference in size which is simply what you need to be able to have two species of the same genus co-exist in the same region. You have to establish an ecological separation and the easiest way to do that is to separate foods and the easiest way to do that is to have about a twenty-five percent lineal change in dimension, somewhere in between ten and twenty-five percent, which means they're roughly one-hundred percent change in volume or mass.

That I haven't really talked about much.

JC: Right.

RES: But I shall. There are problems with what my future reputation will be.

JC: How do you think people will assess you?

RES: It won't affect me. I'm not going to be around to care. But it riles me now, there have been students who have become major academics and who have made a career of going around redoing things that I did coming to substantially the same conclusions and presenting them as their own, not just once, but many, many times. So the original reference will not be cited. The later reviews will be.

JC: Just to be clear, your not suggesting that people are plagiarising your ideas.

RES: No, they're not plagiarising.

JC: They're going to the sites?

RES: They are faced with the standard problems of academics and you need to enhance your publications list and the way to do it is the same way that most people do. And I try not to. To publish multiple papers on the same topic with minor changes from one version to the next.

JC:

RES: And its very understandable, but people always cite the most recent papers, not the ones that actually provided the start.

JC: The most recent thinking that they're are the most--they're the most revisionist in some way?

RES: Yeah.

JC: I see. When you say that about your own work, can you give a particular example?

RES: I can give a couple. J. David Archibald was a graduate student of Bill Clemens that I introduced to the Bug Creek area. While he was a very early graduate student or an advanced undergraduate, he has gone on to do exactly the same kinds of studies that I have on similar localities, come ultimately to the same conclusions that I did and yearly publishes the latest revision of the community changes across the Cretaceous Paleocene boundary.

And David Kraus did exactly the same thing with the paper that Leigh and I wrote on competitive exclusion, and came to precisely the same conclusions we did, but his is a more recent paper so his is the paper people now cite. Fortunately, in that case, our 1966 paper was included in the list of star publications in vertebrate paleontology. In that Dryden and Hutchinson volume on *Source Book in Vertebrate Paleontology*.

JC: Right, which we talked about in an earlier tape.

RES: Yeah.

JC: Right.

RES: So those are two examples. They aren't the only ones, but they'll do.

JC: Thinking again about--

RES: Not saying that either Dave Kraus or Dave Archibald is being unethical about it, I am just saying that it will take someone who has read the entire corpus of the literature to realize that I was the one who came up with the initial idea.

JC: Thinking again about the ideas of what your legacies and impacts have been, what do you think--within the different professional communities that you've been involved in, what do you think have been your--where have you caused the wakes? Where have been your impacts in those communities?

RES: I made a major impact in the Ordovician community with RI-35. I made another major impact with the VISOS (5th International Symposium on the Ordovician System) paper on trilobite chronological distribution in North America.

I have made a significant impact with something we haven't talked about and those are my HyperCard teaching stacks in paleontology. They are widely distributed internationally. I have gotten almost no criticism of them and everyone seems to be very grateful to have them.

I've answered the criticisms in later versions of it--that cleaned up. The work on Bug Creek, the work on multi-phylogeny, the work on Paleocene stratigraphy, had major impact. And I have done some odd things along the way. Like there's something that I hope to get published after I retire and that's the complete analysis of the paleoecology of the rapid evolution of mammal-like reptiles during the late Permian.

JC: This is related to the Pele-hypothesis?

RES: This is related to the Pele-hypothesis and others because its not a simple problem.

JC: Right.

RES: Extinctions rarely are simple.

JC: Right. Now what you've described there are professional contributions, contributions to your community in terms of intellectual and through papers.

RES: I've had other impacts as well.

I suppose I have had somewhere between 16,000 and 18,000 undergraduate students over the years. And am I remembered fondly by a sufficient number of these people so that they stop me when they see me and say, oh, Dr. Sloan, I took your course when I was in college, and it was one of the best courses that I took.

And this happens often enough so that I think its real.

RES: I have met students at the state fair. I have met former students at a copy shop in Cedar Rapids, Iowa. I have met them on railroad trains.

RES: I have had students who register for the course and say my father or my grandfather told me, I ought to take this course.

JC: That must make you feel--

RES: It makes me feel ancient. But it also pleases me to no end. I am a teacher who normally works in the lecture demonstration mode for the big classes and in modified lecture mode in the small, advanced classes and in hands-on demonstration in the field stressing the points that I have made in lecture.

JC: So teaching is a place where you feel like you've had a strong impact?

RES: Oh, I've had a strong impact. Another aspect of this has been something that you have seen. When I present a paper at a meeting, the room may be empty for the preceding paper, it fills for mine and people are standing in the aisles and then they go away.

JC: Right. I remember that with your trilobite material at the North Central GSA. Right. And that certainly was the case, of course, and you also had giveaways as I recall--

RES: Yeah.

JC: --of a cast of or samples of the specimens. It is also fair to say that you've not only taught at the college at the graduate level, but you've also done quite a number of guest lectures in elementary schools and middle schools and high schools.

RES: I do about three or four a year. Usually on the extinction of dinosaurs, but actually on a fair number of other topics and there has been all the teaching I have done in connection with the Minnesota Geological Society. You'll note up there on the wall the certificate of appreciation. I'm an honorary life member of the Minnesota Geological Society. They gave it to me after I had been talking to them for forty-one years.

JC: Forty-one years?

RES: Yeah.

JC: And what about your impact in the Department at Minnesota?

RES: I have not had much impact in the directions of the department has gone. I have had my strokes and kudos, not so much from the department as from my colleagues at other institutions. I have gotten strokes and kudos from my work on establishing a peer group for comparing salaries of the University of Minnesota and the fact that I have done this for about twenty some years. And I have gotten thanks, even grudging thanks from some vice-presidents of the Administration of the University of Minnesota, who originally wanted to use the Big Ten as the peer group and not the research universities.

JC: Something that we're going to explore in another interview. Just for the tape is Bob's role in AAUP, the American Association of University Professors and precisely the discussion of salaries and that sort of thing. Bob has been involved for a very long time there and has been an important campus representative for AAUP. We're going to explore that in another interview context. Bob, one of the things that will surprise people is how little money you've spent--how little grant money you've received over the course of your career. There is--as I recall, most of the money came in the Bug Creek work through NSF.

RES: Uh huh.

JC: And everything else has been paid for with the relatively small grants through the Geological Survey and other sources. What do you think about that and what do you think about the role of funding in geology today?

RES: I lay part of this on the peer groups. It is relatively easy to down-grade an imaginative program for not being practical. And this has happened to me over and over again. And then I went and did it. And it wasn't impractical. It was, in fact, eminently doable.

But that doesn't help in the times of peers' evaluation. All it takes is someone who thinks I am not enough of a specialist to do this particular program to down-grade my rating to good or fair, to totally blow the grant away.

And so the grants go to unimaginative but safe proposals. And my proposals have always been imaginative and may not have looked safe, but in fact were.

JC: When you say have always, have you applied consistently for money through NSF?

RES: Yes, and consistently been turned down.

JC: Really?

RES: The only one that I got was the two-year Bug Creek Grant with what was for that time a very large grant of $20,000 which I stretched to the limit.

And then because of family problems, I couldn't renew it.

JC: Right. Now, you've requested money from granting agencies a lot, what role have you played in doling out money?

RES: I have, on the strength of RI-35 and the Abstracts I have published bent my correlation, had an awful lot of NSF projects to review. I have thought they were very excellent projects and they have always been turned down from other people--turned down by other reviewers.

JC: Why do you suspect?

RES: Well, I have no idea.

JC: I see.

RES: It, I think, is inherent in the peer proposal. If someone is proposing a big project, it is likely to be down-graded as being undoable, and you can't do big projects on little money. Lord knows I've done enough big projects on little money.

JC: So how is granting--the choices in granting--not just through NSF but through other sources, how is that affecting what is happening in geology today?

RES: The number of proposals granted in paleontology has gone down because the number of subjects that are asking for grants out of the same pocket of money has increased. And so we're actually running about a ten-percent chance of getting a paleontological proposal funded.

So you have to scrounge and do everything you can any way you can.

JC: What do you think are going to be the long-term consequences of that?

RES: The immediate long-term consequences are the continued decline of paleontology. Paleontology is not done. It is the only way that most of the geologic time scale can be managed. For a considerable part of the geologic time scale, it is not possible to use radioactive dates in routine correlation and as a result people have to fall back on the fossil record, but as a result of the expansions in other areas and the current depletion of funds for universities are all sorts and for the U.S. Geological Survey, the funding for paleontology is suffering outrageously. I don't know how this is going to be solved. One of the things I am doing right now is producing a HyperCard teaching stack on a whole introductory paleo sequence so that there will be something of mine out there to help train students when I'm retired. And we'll see if it works. Let's hope because I don't see much hope otherwise. The number of practicing paleontologists in academics is slipping every year.

JC: Let's return to thinking about your career and reflecting on it. We talked about what you think your impacts have been and where your legacies are. What would you like to be best known for out of all your work?

RES: For a student who had imaginative ideas of how to demonstrate variable rates in evolution in the fossil record. And I sort of would like to have my historical geology textbook published because that too would be a legacy.

JC: The text that you're working on now, would it be fair to characterize that as a integrated synthesis of your thinking of magnum opus?

RES: Yes, it really is. Its an integrated analysis of all the aspects of historical geology. Not just the paleo-biological or the stratigraphic, which is what many historical texts turned out to be, but the relationship of phenomena in these subjects to plate tectonics and even mantle tectonics and global climatic change and major changes in the atmosphere and global climate and sea level, all the rest of these things.

In effect, whereas most paleontologists have been looking at best at secondary consequences of their raw data, I have pushed it further to Tertiary consequences, in a few cases either Quarternary consequences with the Pele hypothesis as an example of that. Here you have a chain of circumstances that cascade like a series of dominoes and ultimately significant changes in organisms are based on a major event that took place in the mantle just above the core for reasons yet unknown.

JC: I see. You've had a lot of highs and lows in your career, what would you say have been some of the high points?

RES: Oh, one of the high points was presenting Bug Creek in the 1963 SVP Convention.

RES: Another one was the `87 North Central meeting that culminated in RI-35 and the associated meeting.

RES: The absolute pits are when Pres Cloud told me I wasn't good enough to be at the University and told me I ought to go even though I had tenure as an Assistant Professor. A tenured Assistant

Professor is a weird duck and he didn't think I was good enough for the University of Minnesota.

JC: About when was this?

RES: When he first came.

JC: Hum. And why do you think he said that?

RES: Because he was not impressed with the things that I had been able to get out in publication through the Minnesota Geological Survey.

JC: I see.

RES: Which had funded most of my work that was providing most of the publications.

JC: Did he ever change that?

RES: Oh yeah. He changed it. Bug Creek changed it for him very seriously. And there was--by the time he first quit as department chair and then ultimately left the department, he had apologized to me for it, but it really was the absolute pits. There have been other times. Every time a faculty decision goes in what I can now see with 20/20 hind sight was the wrong direction--I knew was the wrong direction at the time--and what I had to say was not considered of consequence.

It was the pits. So there have been highs and there have been lows. This is normal in an academic career. Its one of the prime reasons that tenure is such an important part. I was able to survive the pits because I had tenure.

If I had not had tenure, I would not be at the University of Minnesota now.

JC: Because you think Cloud would--

RES: Cloud would have thrown me out and I suspect Tibor might have also.

JC: Why is that?

RES: Because administrators who are managing programs, departments, colleges, universities, get to thinking that they know the direction these various entities ought to go and they get impatient when they don't. Academic behavior is a long term process. Just the fact that it takes three years between submission of a manuscript and publication is a very good reason for taking a longer than immediate look. We had a--well, one of the things I did in connection with AAUP was serve on several cases where failure to grant tenure was being contested and in one case, an academic vice-president of the University of Minnesota was dead-set against granting tenure to someone who had been given a majority vote on tenure by his department and endorsement by his dean, and it was blocked at the academic vice-president level, in part because the academic vice-president was close in academic field to the poor professor and made a personal decision that this research was not worth doing.

JC: Hmmm...

RES: Yeah. 'Hmmm' is right. Well it was. It was a spectacular piece of work and ultimately we did prevail. The academic vice-president left for other places and ultimately so did the now associate professor. But it's not an isolated case. I've seen it happen many, many times. It's administrative impatience with the pace at which academic changes are made.

And with the notion that if you don't solve it right now, it won't get solved.

JC: Would you--would it be--let's see, in reflecting back on your career, could you say that there are any opportunities that you missed?

RES: I had an opportunity to be the Director of the Science Museum of Minnesota which was then the St. Paul Science Museum.

JC: Right.

RES: I thought about it at length because there was a certain excitement about being a museum director.

JC: About when was this?

RES: Oh, late `50s, before Theil retired.

JC: And would this have meant to you leaving the University?

RES: This meant I would leave the University and would no longer teach. And I was extremely loathe to leave my students. My students have been a very important part of my life my whole career. And where I would have gotten a different kind of student directing in the St. Paul Science Museum, its not the same.

JC: That must have been a very appealing offer though.

RES: It was an appealing offer.

JC: It would come at a time when things might have seemed pretty tense at the University.

RES: Yes. I had been given tenure. I had not been promoted.

JC: And you seriously considered--

RES: I considered it for about three months before I finally decided "no".

JC: And mostly you decided "no" because you still wanted to teach at the University?

RES: Yes.

JC: Would you have been able to pursue research as a director?

RES: I think probably so although I am quite certain the administrative load would have overwhelmed me eventually. And I would have become like many other museum directors that shuffle paper.

JC: Are there other opportunities that you think you've missed?

RES: Not really. By the time I had a national reputation, I was anchored here and I really didn't want to go anywhere other than Minnesota. Minnesota has been a very nice place to be. The only metropolitan area that I would have enjoyed as much as the Twin Cities would have been Toronto. I certainly wouldn't want any part of New York or New Haven or Chicago or Baltimore or Washington or Berkeley?

JC: You're describing places where major museums are?

RES: Yes. And where major research institutions are.

JC: I want to change gears now and we've been talking about low points and high points and getting you to reflect on your career and I want to ask you about what--I know that you would describe as your very high point and that was when your marriage with Sally. And you've described in your autobiography quite a bit of that relationship and its origins and its history over the years. I want to focus on--I want you to focus on Sally's impact in your professional--in your scientific career. What do you say her role has been over the years?

RES: Basically picking me up and putting me together when I was really in the pits.

JC: She's been a good faculty spouse until the point when the Department got so large that we ceased to have any total department activities. In the early years, there were routine faculty Teas. There were seminars over at the Wright's house which I'm sure you've heard of.

RES: There were the annual spring banquets and she was very supportive in all of those. It went way beyond what one could expect a wife to do in such matters. And then we had our problems with our eldest child. The financial situation went to pot so it was no longer possible for us to exist on just my salary. We kept going deeper in the hole each year and it became necessary for her to finish her degree and get some sort of position and start making a financial contribution to the family instead of just the support function.

RES: And so I had to provide support for her. As she first finished and then started teaching. Its been very much a two-way street over our whole married career.

JC: Two ways in the sense of each helping the other?

RES: Each helping the other. Sal typed eighteen copies of my thesis in order to get the clean copies required for the graduate school. And for the library copy, I in fact submitted the printed version which was essentially exactly the same as the thesis, considerably smaller and no more typing.

JC: And you, of course, paid her back many years later by doing substantial work on her dissertation.

RES: Yes. I took care of making certain that I hired some people to transcribe the interviews for the case studies she was using for her thesis--retyped and formatted those transcriptions on to computer disks and did type some significant chunks, organized major sections of the thesis. She did most of it, but I was there helping and, in fact, I was holding her hand when she was in the stage of wanting to throw the thing out the window. I'm sure every Ph.D. student reaches that point.

RES: She held me together when I was finishing my thesis in the spring of `53 and I held her together to get the thesis organized and formatted when she finished her degree.

JC: Now you describe in your autobiography a number of trips that Sally accompanied you on that were both--they were combination vacations and collecting trips. Did the two of you at different times talk about the work you were doing? Were you talking about the implications of your work with her--that sort of thing?

RES: Oh yes. Routinely and regularly. When we took our first trip to Europe, it was funded by the Nicoria project and it was essentially to organize all the data on archaeological and zoo-archeological material from a seven-year excavation in the Peloponese, and we both went. We both worked. I identified and measured counted specimens and Sal took care of the statistical organization of this thing so that we can make some sense out of it.

JC: And so that in a lot of ways is a co-work?

RES: It is definitely a co-project.

JC: What are the--

RES: She went along chasing trains with me.

JC: And she also put up for a long time with you having materials all over the house?

RES: Oh yes. I was never properly trained to put things away and my mind works in weird fashions.

I only remember where things are if I put them there. And so if somebody else moves them, I'm in trouble and have to completely reorganize everything until I have cycled through everything and, again, know where it is.

JC: It's also--I think we should say for the tape--its important to note that Sally, the other night, said that when they became engaged, she chose to take a geology course at the University of Chicago and the reason why--I asked her why and she said while I was marrying a geologist, I wanted to know what he did. Its important to note.

RES: We have both always been involved in everything each other has done. When Sal was in the tryout for a place on the International Pistol Team of the United States, I was the one who made sure that she had targets to shoot on--that they were moving--that they were timed targets and that the rules, in fact, were followed so that she could have her tryout and go to the finals.

JC: Did she--now, in terms of field work, did she ever collect anything that had an important implications?

RES: Not really because most of the time, she was at home with the kids in the summer.

JC: Oh, I see.

RES: Or the kids had gone to their grandparents and Sal was essentially on vacation. It didn't work well for her to be with me in the field in Montana because she could not cope with the heat and you've been in Montana, you know what its like--120 degrees in the shade and the nearest shade is in a bar in Circle, 60 miles away.

JC: And so she did go out to Bug Creek at least once?

RES: Yes. She visited both Bug Creek and the Jordan area several times.

JC: How do you think your work would be different if you and Sally weren't married or if you didn't know her.

RES: I don't know. I do know that she has been more help to me than most spouses--

JC: We're starting up our conversation again right after Bob got off the telephone. Its a convenient place for us to stop and we have a prior appointment to go do other things at this point. So we're going to stop our interview for this set of sessions and we've talked Bob into the ground almost testing his stamina to the limits. I wanted to say on the tape that we've conducted this set of interviews in Bob and Sal's newly built house in Winona, Minnesota. There are a number of features during our taping process that have come to light and one is the low rumbling of the trains that have come by. Bob and Sally picked a spot where the trains come very close by and that's an important feature. And trains are an important feature in their lives and not trivial that they've chosen this spot.

RES: We also had occasional short sections of the interviews when one of us would point out the window at bald eagles flying next to the house.

JC: And this spot where the house is is well within Bob's research area. And, in fact, Bob has notes that he's stood at or very close to this spot early in his career. In fact, when Charley Bell was teaching Bob the geology of the area, that's important to know--not trivial that Bob's chosen Winona. In addition, Sal is a tenured associate professor--

RES: Yes.

JC: --at Winona State which, of course, we will talk about in another set of interviews. Any thing else you'd like to add, Bob, before we--

RES: Let's see--it was apparently the third stop of the second day of my employment at the University of Minnesota--that I first saw the present view from this house.

JC: Which Bob has dutifully recorded in his notebooks. Well, I think that'll do it for us. We do have to get going to go do other things. Thank you very much Bob.

RES: You're very welcome Joe.

JC: And its also important to know that Bob has been giving the recital of life without any notes and with no preparations--basically, off the top of his head--and that's quite an important feature--and staggering for someone with my experience in trying to remember things. Again, thank you very much Bob.

[end of tape side. End of interview series.]

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