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University of California
Berkeley, California

Caution and Care: The Evolution of Paleontology at the University of California Museum of Paleontology:

Volume II

Interviews with

David Archibald
Mike Novacek
Annalisa Berta
Lowell Dingus
Marisol Montellano
Zhe-Xi Luo
Charles Marshall
Nancy Simmons
Don Lofgren
David Polly
Jessica Theodor
Greg Wilson
Mark Goodwin

Interviews conducted by
Paul Burnett
in 2015 and 2016

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Interview History

The Bill Clemens/UCMP oral history project has been several years in the making. Historian Sam Redman first proposed to do a history of members of the University of California Museum of Paleontology in 2011, specifically to interview Dr. William Clemens and a number of his graduate students. The concept behind the project was novel and important: to document with long-form oral history of successive cohorts of students who were advised by a single scholar, while at the same time interviewing the scholar in depth about the evolution of his field, as well as the key transformations in the institutions in which he played significant roles.

UCMP Associate Director Mark Goodwin was the fulcrum in organizing the project, from fundraising to arranging for interviews with Bill's students from all over the world. My first session with Bill was December 18, 2014, and my last was March 10, 2016. One of the factors contributing to the length of time spanning these sessions was the fact that Bill was caring for his wife Dorothy "Dot" Clemens while she battled cancer. There was some hope that she would live to see the project completed, but she ultimately passed before its completion. After a time, Bill resumed the project, in tribute not only to UCMP, his colleagues, and students, but also to her memory, as Dorothy Clemens was deeply committed to ensuring that Bill's oral history was documented for the ages.

Several themes are explored in the interview. There is a longstanding concern in the history of science with the ways in which scientists establish and maintain their credibility within and beyond their communities. By the 1950s, the queen of the sciences was physics, and the public was consumed by the promise and peril of high technology, from the splitting of the atom to the electronic consumer items in the shops. In the public mind, paleontology perhaps had more in common with the 19th-century field sciences than with the growing domains of digital computing or molecular biology.

When Bill Clemens started his undergraduate work UC Berkeley Department of Paleontology at the beginning of the 1950s, the modern evolutionary synthesis in biology, which linked laboratory research in genetics to field studies, statistical analysis, paleontology, and Charles Darwin's theory of evolution, had only just been worked out before the war. The helical structure of DNA was announced in Bill's junior year. In other words, Bill began his career at the beginning of a new common cause in science — the evolution of species and their adaptations to changing environments — with cascades of new questions to follow in the decades to come.

The drama of paleontology is often heightened by the interest in the gigantic specimens. Owing in part to the Evolutionary Synthesis, the paleontologists of Bill's cohort were interested, not just in the structures of fossils specimens themselves, but in where and how they lived in relation to one another. To get at some of these ecological questions, these students turned to the very small microvertebrates which could be found with a new technique of screenwashing, which is basically sifting for tiny fossils. What they found in the Lance Formation in Wyoming in one season equaled the number of fossils of their kind ever discovered up to that point. The field was moving away from the romance of the big dinosaurs and toward a more detailed understanding of evolutionary relationships among specimens and of the developmental characteristics that might tell the scientists something about how the creatures lived.

It's important not to understate the importance of this scale and extent of fossil collection. The organized work of Clemens' generation and the one that followed made possible newer types of data-intensive computerized research on paleontology, evolutionary biology, and climate change, areas far beyond the classification of fossils based on their structure. In fact, it is not uncommon for doctoral students today to conduct their research entirely with collected specimens in a laboratory, although Bill might not recommend this exclusive a course of study.

It is here that we come to an important focus of this history, which incorporates the second volume of this project: the thirteen interviews with Bill's graduate students and the current leader of the UC Museum of Paleontology, Charles Marshall. Bill and his students are witnesses to the changes in the field of paleontology, the increasing use of computing to process large quantities of data, and the field's involvement in the most pressing questions of the last four decades: the resilience of species, the interdependence of organisms, and the consequences of a changing climate on the abilities of organisms to adapt to both sudden and gradual changes.

These questions are also a reflection of my initial interest in credibility in science. Through these interviews, we see how paleontology has adapted itself to a changing scientific climate, contending with the introduction of new species of ideas such as the asteroid-impact hypothesis for the extinction of most dinosaur species at the end of the Cretaceous, or through the adoption of sophisticated mathematical analyses of the surface structure of mammalian teeth to answer questions about the evolution of a particular species' diet millions of years ago. Scientists struggle for credibility, and one way of doing so is to hybridize their research techniques and programs with the dominant sciences of the day, such as molecular and structural biology. The Department of Paleontology's integration with the Department of Integrative Biology at UC Berkeley was part of a larger effort to cross-fertilize ideas and techniques from different but related disciplines that focus on evolutionary processes. "Interdisciplinarity" had an early home here at Berkeley and especially at UCMP, long before Integrative Biology was founded in the 1990s. One result of this integration is that the UC Museum of Paleontology has once again assumed a worldwide leadership role in the conduct of cutting-edge research, though it has long led the field of mammalian paleontology.

On a more human level, you will find in these pages that the engines of research and innovation are fueled by human virtues as much as intellect. Bill and Dot's patience and empathy for Bill's students, as they navigated the challenges of graduate school and the dust and heat of the field, are well documented, as is Bill's curiosity, meticulousness, patience and care with which he draws his scientific conclusions. It is surely a mark of his influence that his students have taken up his approach by using new techniques and evidence, carefully tested, to gradually move their respective fields forward increment by increment.

Paul Burnett

Berkeley, CA, 2017

Introduction, by Jason A. Lillegraven, the first graduate student of Bill Clemens

Jason A. Lillegraven

Arts & Sciences Distinguished Emeritus Professor of

Geology/Geophysics and Zoology/Physiology

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Parallels — Getting a secure start on a career in science is always a challenge. In the early 1960s, both Assistant Professor William Alvin Clemens ('Bill') and the then recent master's-degree holder Lillegraven ('Jay') were in remarkably parallel settings—even though immensely separated in terms of attained knowledge about their common interests in paleontology of the Mammalia. The lives of those two academic beginners converged via U.S. mail in September 1963, followed by their first face-to-face contact in the late summer of 1964 in Lawrence, Kansas. I arrived on the KU Jayhawk scene following study in South Dakota to become Bill's first student to seek the PhD degree.

Correspondence — Ever since those days, I have saved almost all of our correspondence—now totaling hundreds of richly informative documents. The diversity of our exchange ranges across broad paleontological concepts and taxonomic details, news of our personal lives, commentaries on local settings, field work, friendly and absurd insults, surprises as recognized through our microscopes, and views about the marvels of organic evolution. Bill's wife Dorothy ('Dot') regularly joined in the fun with her own perspectives. And yes, of course all those items of correspondence will be properly archived and made accessible to the professional community.

Extractions — In preparation for this brief contribution to Bill's extensive oral history, I extracted the bulk of our correspondence from my file folders and read it all again. I did so because of the realization that I am in a unique position in having coexisted in person with Bill for three wonderful years (academic years 1964–'65 through 1966–'67), when both of us were rank beginners in our university roles. Although refreshing those memories from 1963 to today was rewarding in itself, my focus today is to answer the question, 'How did Bill do as a neophyte professor in guiding his first doctoral student?'

In terms of reliability of my analysis, recognize that this was my first experience with an academic advisor who was so young. Mentors approaching senior citizenship guided my previous studies (undergraduate at Long Beach State College with the brilliant teacher John A. White; and master's degree at Rapid City's School of Mines with the taciturn 'professor's professor' Robert Warren Wilson). Dr. Clemens, in contrast, was a mere six years older than I. Nevertheless, at the time it hardly occurred to me that we were so close in age. His levels of knowledge in nearly all matters were so profoundly greater than mine that I responded almost the same way as in my prior experiences with advisors far more senior. Bill immediately put me at ease personally, and I just naturally responded in awe professionally. Dot, as well, instantly made me feel comfortable and welcome in their home, and I was treated almost as a junior colleague.

The truth is, and as I more fully appreciated in the following decades, even from the outset I was experiencing absolutely genuine expressions of personal warmth from the Clemens family.

O**pportunity** — To my special delight, we linked through love for the conduct of field-based studies. Starting almost a year prior to my arrival in Kansas, Bill and I were discussing possible dissertation projects. In the most pleasantly diplomatic way, he encouraged broader thought than my own suggestion to basically expand my master's project within the White River Group. Little did I know that less than a year later he would bestow upon me the opportunity of a lifetime to take scientific charge of a project in uppermost Cretaceous strata of the Red Deer River valley in the beautifully rolling plains of southern Alberta. Bill had already done the background work that would involve international cooperation including assurance of the existence of wondrous new fossils and the opportunity of suitable field vehicles along with field assistants and supplies—continued for three full summers. Even at the time I recognized the generosity that was based almost completely on his faith in my abilities. The realities—as well as the attendant responsibilities—of that generosity become progressively clearer through each and every subsequent year of my life.

Terror — Bill and I had adjacent offices during our three years of overlap at KU. So the academic guidance I experienced toward the zoology diploma was thorough and a regular topic of discussion. In addition to the diversity of courses that one would consider as 'normal' for the discipline (e.g., invertebrate zoology, comparative physiology, evolutionary systematics, mammalogy, a summer course in marine ecology, etc.), I'll never forget the question he asked of me dealing with the coming fall semester of my second year. "How would you like to take an advanced course in geology from a world-famous University Distinguished Professor?" The teacher was Professor Curt Teichert, and the graduate-level course was 'Geological Development of the World.' I was one of three graduate students, we met weekly surrounded by Dr. Teichert's immense personal library, and the four of us alternated doing the week's assigned two-hour presentations. That was a stunningly important and positively terrifying challenge for all of us; it included many early hints toward plate tectonics. And that's the kind of academic guidance Bill would make into the stuff of an irreplaceable university experience.

Embarrassment — One fine day early in my second year at KU Bill showed another quite unexpected and wonderfully valuable aspect of his academic treasure chest. Almost always he had his facts correct prior to making sweeping statements. Note that I said 'almost.' That morning we were chatting about the early diversification of marsupial and 'placental' mammals. And he said something like: "You know, Jay, there have been no early embryological studies done on marsupials since the time of Sir Richard Owen." Oddly, something about that statement didn't quite ring true for me, so I spent that same afternoon plowing through historical and current literature in KU's massive biosciences library. That initiated my learning about the very existence of high quality and truly exciting literature on early development across the Marsupialia. I don't know when I've enjoyed a conversation so much as the one I initiated with Bill the following day! His erroneous comment of the day before eventually led to composition of 'Polarities in mammalian evolution seen through homologs of the inner cell mass' (2004, *Journal of Mammalian Evolution*, v. 11). But rather than having been embarrassed by my exhortations after the initial experience in the biosciences library, Bill actually was enlivened and genuinely pleased by our interaction. Although I then recognized that nobody's perfect, much

more importantly I better appreciated that Bill's generous reaction was yet another sign of a great academic advisor!

Comprehensiveness — How about Bill's knowledge of his own field of mammalian paleontology? Anyone who has taken his courses has experienced astonishment over the thoroughness of his carefully updated notebook, the breadth of his personal awareness of the key localities and their biostratigraphic relationships, and his seemingly encyclopedic recall of essential identifying characteristics of the faunas. The late Professor Donald E. Savage, my close friend and regular handball opponent, told me much about Bill as a grad student in the late 1950s at the University of California, Berkeley. At that time Bill had very strong competition from several other top-notch doctoral candidates. According to Don, when he would quiz the group of students about some aspect of the diversity of extinct mammalian groups, Bill was rarely the quickest to respond. But almost always, Bill's answers were the most accurate, thorough, best organized, and most articulately presented along with specific mentions of the relevant literature. And that was Bill's style in the classroom as well. Glitz was never a primary goal for this man, but thoroughness and scientific credibility simply could not have been higher. And his updated lecture notes were fully accessible to anyone bold enough to ask.

Stress-barometry — How about his diplomacy, or simply dealing with difficult people? Bill and I probably could not be more different in that personal capacity. Outwardly, he is an extremely mild-mannered, universally cordial individual. Nevertheless I did learn important lessons, especially by unobtrusively observing seemingly trivial details. Particularly telling was to intently watch his 'stress barometer' as surreptitiously revealed by his knotted jaw muscles and clenched teeth as they threatened to pierce clear through the battered stem of his tobacco pipe.

Churchillianism — More seriously, I must cautiously mention one occasion that demonstrates his genius in calming troubled waters. Upon completion of a publication, I had failed to acknowledge—by name—a particular person who had contributed aid to initial establishment of the project. Clearly, I was in error through that 'unfortunate omission.' And the fit hit the Shan through that individual's broad dissemination of a fearsome letter containing unwarranted personal vilifications, inappropriate assertions, and it nearly led to an international research disaster. But Bill, quite on his own while in England, composed a graciously apologetic response, which must have been very difficult considering here-unspoken details of the complete story. Then, in a separate, wonderfully phrased letter to me, Bill let me know that the situation probably was neutralized and that "No more can or need be done." Translated correctly knowing Bill, that very phrase actually said "Now keep your bloody mouth shut!" Handled less intelligently, that incident could have been the end of a wonderful relationship and perhaps more than one career. I know that, and experience has informed me that sometimes one really must bend over backwards simply to appease, no-matter how great the pain. That's easier said than done, of course, but Bill handled the situation masterfully in a manner that protected us both. What he did remains beyond my diplomatic abilities even today. It also caused me to appreciate that I might be better as a regimented Eagle Scout than Bill, but he operates much more like a Winston Churchill than I could ever emulate.

Earthiness — Through the years, I have heard from several of Bill's acquaintances that one of his flaws is he's just 'too perfect'—indeed, they said he's an outright 'prude.' Let me here

and now put that nonsense to rest by relating a single incident that told me so much. Back in the 1970s a devilishly funny author named Barry Humphries was establishing himself as a Mark Twain-equivalent for Australia. One of his prime characters in comic books and film was a supremely irreverent, heavily beer-drinking young Aussie named Barry 'Bazza' McKenzie who loved beautiful sheilas, didn't get along with the Poms, and chundered richly with tomato skins. A favorite quotation from one of Bazza's drinking-pals was: "Mon Dieu. I tell ya, Baz, I'd'a crawled over 'alf a mile of broken glass just to hear that little sheila piss into an empty jam tin." So what is the relevance of Bazza to Bill's reputed humorlessness? Well, to my utter astonishment and delight, he carried a pair of Humphries' illustrated Bazza-sagas all the way back from Australia and quietly presented them to me with a smile. I guess he didn't want me to be 'too perfect' either. And those two comic books are treasures today within my professional library.

Independence — About midway through my third year in Kansas I learned that Bill was accepting a faculty post at UC Berkeley. Rather than fomenting a disaster for me as a KU graduate student, Bill turned what initially seemed like a lemon into a pitcher of splendid Margaritas. Indeed, I learned soon enough that I really could complete the dissertation research under conditions of real independence. That knowledge came largely thanks to Bill's thoughtful planning just when I needed confidence the most. With close cooperation from KU's administration, he eased the way by continuing as chairman of my graduate committee, providing extensive correspondence from Berkeley when needed. His letters typically included a significant section dedicated to some relevant and instructive aspect of mammalian paleontology. Those arrivals, therefore, were scientifically important little essays, and they remain of interest today. Remember too, that was in the time of carbon paper and envelopes, well before today's convenience and dispatch allowed by digital technology.

Benevolent Professionalism — Also very important at the time, Bill responded with good advice almost immediately when I needed help in structuring a final draft. He even traveled from Berkeley to Lawrence to chair the public defense of my research. Thus, in that last year I enjoyed the best of two usually contradictory worlds—holding a sense of genuine academic independence simultaneously with secure knowledge that I could get solid help if needed. I suspect that almost nothing in that overall arrangement is common, as my young advisor went far out of his way to see that the arrangement actually worked! His levels of responsibility in the guidance of a student and Bill's performance of the duties of a mentor were unlike anything typical of a beginner in higher education! He held from the beginning the wisdom of a benevolent professional.

Berkeley — My studies at KU were completed before the end of the 1967–'68 academic year. Later that year, Bill was one of three supporters for my application to join the faculty at what was then called San Diego State College. I successfully interviewed for the position in Zoology that was to begin in the fall of the 1968–'69 school year. But Bill's strong influence on my everyday life did not end there. Indeed, because he was going to be spending that year studying Mesozoic mammals in England, he saw to an invitation for me to conduct a post-doctoral year of independent research at UCB. With a National Science Foundation traineeship in hand, and with the great courtesy of San Diego State's blessing to hold off my residence for a year, my wife and I moved from Lawrence to Berkeley. Before our arrival, and without our prior knowledge, Bill had contacted UC's Faculty Housing Office to check on availability of a suitable

apartment, he had received all of our worldly belongings from the movers into his home garage, and he even paid the movers from his own pocket because I had sent a standard check (rather than a certified version) which the movers had refused! Now that's service way beyond the call of duty . . . .

Despite the chaos in Berkeley during that year of the Peoples' Park riots and unfortunate departmental distractions, the long-term benefits of that postdoctoral year of scholarship, limited teaching (Paleo 101, Phylogeny and Evolution), and establishment of new professional contacts were enormous. And those experiences simply would not have been possible in the absence of Bill's unexpected invitation.

Flexibility — Bill Clemens has always been keenly focused on his fossils, their description, and how they can be best interpreted. In my presence, however, he was stuck with a student who kept drifting off into related disciplines—such as structural geology with my master's thesis, comparative mammalian reproduction with my PhD dissertation, paleogeographic evolution of the Rocky Mountains later, and even today with the nature and timing of large-basin structural fragmentation into small remnants (2015, Late-Laramide tectonic fragmentation of the eastern greater Green River Basin, Wyoming: Rocky Mountain Geology, v. 50). This cheeky graduate student even wrote to Bill (letter of November 20, 1968) about this tendency to stray from disciplinary purity, saying "The problem with paleontologists is that they're too damn [sic] interested in fossils . . . ." Was Bill threatened or even uneasy about advising such a cross-disciplinary renegade? If he was, it was never, not even once, made obvious to me. Quite to the contrary, Bill seemed to thoroughly enjoy joining with me into new avenues of our research. I think we both recognized that such diversions continued to be directly linked to essential underpinnings that could be provided only by information gleaned from the fossil record. And that is exactly what all Earth-science professors should come to realize!

Shortstop — The present summary is very personal. It is short on science, but long on Bill Clemens' characteristics as a beginning professor, personal mentor, and research-oriented human being. With exception of the much-too-early death of our beloved Dorothy, Bill and his children have earned an almost ideal life together, with admiring fans living all across this beautiful globe. I feel enormously honored to have been a consequential part of the story of Bill's early days as a professional paleontologist and mentor. I'll be the first to admit that Bill has had several intellectually much more gifted students than I. But he has never had a more appreciative student than I continue to be.

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The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology:

David Archibald

Interviews conducted by
Paul Burnett
in 2015

Since 1954 the Oral History Center of the Bancroft Library, formerly the Regional Oral History Office, has been interviewing leading participants in or well-placed witnesses to major events in the development of Northern California, the West, and the nation. Oral History is a method of collecting historical information through tape-recorded interviews between a narrator with firsthand knowledge of historically significant events and a well-informed interviewer, with the goal of preserving substantive additions to the historical record. The tape recording is transcribed, lightly edited for continuity and clarity, and reviewed by the interviewee. The corrected manuscript is bound with photographs and illustrative materials and placed in The Bancroft Library at the University of California, Berkeley, and in other research collections for scholarly use. Because it is primary material, oral history is not intended to present the final, verified, or complete narrative of events. It is a spoken account, offered by the interviewee in response to questioning, and as such it is reflective, partisan, deeply involved, and irreplaceable.

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It is recommended that this oral history be cited as follows:

David Archibald "David Archibald: The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology" conducted by Paul Burnett in 2015, Oral History Center of the Bancroft Library, The Bancroft Library, University of California, Berkeley, 2017.

David Archibald, 2012

David Archibald is Professor Emeritus of Biology, Evolutionary Biology Program Area, and Curator of Mammals, San Diego State University Vertebrate Collection. He received his PhD in Biology from UC Berkeley in 1977.

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Interview Date: February 12, 2015

Audio File 1

Interview 1: February 12, 2015
Begin Audio File

01-00:00:08
Burnett: This is Paul Burnett interviewing Dr. David Archibald for the Bill Clemens UCMP Oral History Project in the University History series. Dr. Archibald, welcome.

01-00:00:21
Archibald: Thank you.

01-00:00:22
Burnett: You've come up here to talk a little bit about your experience at UCMP and working with Bill Clemens, and also your own career and subsequent research and published work. Can you tell us a little bit about where you're from and how you came to become interested in paleontology?

01-00:00:46
Archibald: First, let me say happy Charles Darwin Day. It's February 12, so it's his birthday today. James David Archibald is the full name. I've always gone by David. I was born in Lawrence, Kansas. My father was going to college on the GI Bill to get his engineering degree. I grew up along the Mississippi River, in Iowa, and moved to Ohio and finished my undergraduate—excuse me, my high school degree there, in North Olmsted, and went to Kent State as an undergraduate. Got a geology degree. Yes, I was there, I was a sophomore, when the shootings occurred. Then came out to Berkeley, my first wife and I. She was getting a PhD in French literature. I came out then to work on my paleontology PhD degree, and this was in 1972. I was here from 1972 to the summer of 1977.

01-00:01:40
Burnett: What attracted you about paleontology?

01-00:01:44
Archibald: I have to say that I was interested in geology from the time I was fairly young. There were other things I entertained doing, like going into drama and maybe being a director, or even professional scouting, but for several reasons, I liked geology from the time I was twelve years old. *Journey to the Center of the Earth*, both the book and the movie. So things do impress people when you're younger. I had a friend who was interested in geology in Iowa, and he sent me some books when he went off to Harvard. So that interest was there. I knew I was going to do geology when I got to Kent State. Paleontology, I had always kind of a background interest, but there was an undergraduate advisor, Owen Lovejoy, who was an anthropologist, and he got me interested in paleontology and actually advised me, he said, "Don't work on human evolution. There's too many people searching after too few fossils." The fossil record for humans was not so good at the time. This would have been the late sixties, early seventies. So I became interested in paleontology. Began as primate evolution, but then expanded that when I got to Berkeley. We applied to four places, but

my first wife and I wanted to come to California. I'd remembered as a kid visiting from Iowa, and loved San Francisco, the Bay Area. That's where we ended up from '72 to '77.

01-00:03:16
Burnett: When you started, had you decided on a kind of specialization within that, or is that something that evolves during the graduate program?

01-00:03:24
Archibald: When I got here, I wasn't sure who I was going to work with. At first, I thought about working on primate evolution, as I mentioned, with Donald Savage, and for various reasons I don't need to go into, it just didn't work out, but I was interested in other early mammal evolution. Bill Clemens had a project out in Hell Creek in northeastern Montana. I was still kind of smitten, still, by primates, and in fact, I remember finding a fossil once and him saying, "David, there's more than primates." But by then, I was becoming interested in generally placental mammals, the group to which we belong. There's a little over 4,000 species of those today. That's how I started working with Bill then.

01-00:04:11
Burnett: Did fieldwork follow shortly after the first year of coursework? How did it work there?

01-00:04:17
Archibald: I point out, as a geology major, I did six weeks of field camp during undergraduate geology, so I had a sense of that and I was kind of an outdoors person. I arrived in Berkeley the summer of '72, Bill's—one of his first summers out in northeastern Montana, and '73 was the first year I went out there. I know I was there '73, '74, '75, and '76. One of those years, and I am not really sure if it was '75 or six, Bill was actually on sabbatical leave in England, so he trusted me enough to run his field camp while he was away playing in England.

01-00:04:56
Burnett: It seems like this is a self-selecting group, that you have to be an outdoors person to do this kind of work, it seems.

01-00:05:06
Archibald: I've got other colleagues who are very excellent paleontologists, but their fieldwork is in museums. I've always been the view that I want to find the fossils that I'm going to work on. That probably started with Bill, but since then, other places I've worked through my career, most of the writing I've done is on material that I or colleagues have found. But again, I have colleagues who do essentially no fieldwork and work through museums or modern specimens.

01-00:05:35
Burnett: Right, they just work with the collections that are available?

01-00:05:37
Archibald: Correct, and there are many of them. Right now we're talking about the four floors of, I think, La Brea fossils you have in there. They were there when I was a student.

01-00:05:50
Burnett: Right, and I don't think they're processed completely yet.

01-00:05:53
Archibald: I don't know. I was told they were prepared, but they just get dust on them over many, many years.

01-00:06:03
Burnett: Can you talk a little bit about the dissertation work that you did, and Bill Clemens became your advisor as a result of those—

01-00:06:13
Archibald: By the time I went to the field in '73, it was pretty clear I was going to be working with Bill. Here's a case where, fortunate for me, that his interest and mine were very close together. I became more and more interested in general origins of placental mammals, not just primates. When we were doing fieldwork together or when I was on a graduate assistantship helping to sort through the fossils, it was things I wanted to work on as well. I was very privileged in that way. I didn't have to convince him what I was doing had merit, if you will. He was interested in it as well. What I ended up doing was that this is across—though it's now called the Cretaceous-Paleogene boundary. He was working on the mammals from the Paleogene, and for my dissertation, I worked on the mammals in the Cretaceous, the Hell Creek Formation. Also, I was doing the mapping of geology. The other thing I did, and actually most of it never was published, was I did all the turtle material, because I still like turtles. I still have three live tortoises at my home in San Diego. I still have love for those. That was never published, but the mammal and the geology of the area was published in '82 or '83. The work was finished in '77.

01-00:07:34
Burnett: These are, of course, heady times in the 1970s for paleontology From what I've read, paleontology is getting an influx of interest from population biology and these other disciplines. Or there's more interdisciplinary activity going on.

01-00:07:58
Archibald: Correct.

01-00:07:58
Burnett: Could you talk a little bit about that?

01-00:07:59
Archibald: The thing with the population biologists actually go back a little earlier. What was called the Modern Synthesis came about in the thirties up until about the fifties, and maybe into the sixties. Paleontologists were involved with this, population geneticists and naturalists. What happened is you recombined

population genetics with naturalist sort of studies to come back to what one can call the waxing of Charles Darwin's ideas of natural selection. By the time I was a grad student, that was still of interest, completely, but several things were happening. I, as I said, came in '72 and left in '77. So, I would discuss them in a book that I published this last year, there is the change in philosophy of systematics. There is the rise of molecular studies and genomic studies, and their increasing computer power. Now, these didn't completely overlap while I was here, especially computer power. I still had pretty crude computers to use, and my thesis was not done on a computer. It was done on a Corrasable typewriter. But the other parts were taking off, the molecular work especially. What was happening is that molecular biologists, who had not been particularly interested in systematics and evolutionary studies until this point, became very interested in it, and it's taken off and blossomed greatly since that time. All kinds of genomic, molecular studies. Those three things were happening, the very beginnings, at this time.

Another point is that I was with a cohort of students that was very good and have gone on to do nice things. I think I can argue, and I have this philosophy, that I probably learned more from my fellow graduate students than from the faculty. That takes nothing away from the faculty. I think it's just how the process works. The faculty were there as mentors to guide me where to go, but I learned from my colleagues. As I've had students, I think this is still the same thing. The cohort you're with can be very important to your own education and training.

01-00:10:16
Burnett: It sounds like you've collaborated over the years with a number of those students.

01-00:10:20
Archibald: Many people. It's been less in the recent time that Bill and I have written together, but Bill Clemens and I wrote a number of papers together about extinctions, about mammals, and so forth. It's been a while since we worked on a paper together. But yes, I've had many collaborators. In the nineties onwards, I worked over in Asia, in what the Russians call *средняя Азия*, or Middle Asia. A somewhat colonialist term, but it was Middle Asia. Uzbekistan, I was there nine times, Kazakhstan twice, and Pakistan, even before that, twice. Most of my work, starting approximately mid-nineties, was mostly in Asia. A little bit in western North America, where I had worked, up until that time, almost exclusively.

01-00:11:10
Burnett: And so the collapse of the Cold War permits this kind of—it opens up these territories to—

01-00:11:18
Archibald: The answer to that is a yes and a no. A colleague of mine, who was a few years older than I named Lev Alexandrovich Nesov, at St. Petersburg, then

Leningrad, we communicated with each other, and he did a lot of his work in isolation. First time I went there, it was still Soviet Union. It was only a year or so before the collapse, and then we had a field conference in Kazakhstan. It's the first time I did some work with him there. He was interested in very similar things to what I was interested in, and we talked about—in fact, he came in 1991 and took a tour of the U.S. and he said, "David, my life is separated into parts, before and after I came to the United States." And Canada as well. Then I went over there at various times, and I was actually a Fulbright in St. Petersburg, after, unfortunately, he died in 1995. I worked with his student, and I've continued to do so. He's no longer a student. He's in his mid-forties. Alexander Averianov. We've worked together a lot.

01-00:12:22
Burnett: There has been this kind of opening and this collaboration that's taken place.

01-00:12:26
Archibald: Yes. Why I said yes and no is that we probably could have worked together, in mostly Uzbekistan, as I mentioned, while it was still the Soviet Union. In some ways, it would have been easier to get into it as it became its own country. What happened was the sort of collapse of science, *Academiya Nauk*, the scientific academy [Russian Academy of Sciences], and the rise of customs houses. Whoever had the power shifted dramatically, so it made it harder to do fieldwork, in a way, than when it was the Soviet Union. It would have been harder to take specimens out. Averianov loaned me—this is, again, after Nesov's death—loaned me all the collections that Lev had made since the seventies up until the early nineties. In the time of the Soviet Union, I probably couldn't have borrowed them, taken them out of the country, and now, even with all the problems we're having with Russian scientists still, deal back and forth. I just emailed this guy yesterday with talking about a project together. He's taken specimens back that we collected. I know that he has some at St. Petersburg. The idea of taking specimens in and out is easier. It's still a little hard to get things out of Russia. It's still not really a free country the way we think of it.

01-00:13:51
Burnett: As far as Kazakhstan and Uzbekistan, is it more expensive as well?

01-00:14:00
Archibald: More expensive than if it had been Soviet Union?

01-00:14:02
Burnett: Yes, if there are customs—

01-00:14:04
Archibald: No, no. We could go down, as they say, a rabbit hole, about how we're dealing with things, but the issue is that what you bring back to Uzbekistan, who is in control, who signs off on us taking specimens out, and we did make some letters of agreement. I wrote it in English and we translated it into Russian. One of the things I did is I availed myself, when we had the very

small printers, I would take them. When you wanted to do things, it was, okay, you come back two or three days later and we'll have it. Well, I had the printer there, so we would do it in English and Russian, and we would have it done in two hours. You get things done more quickly, so you could not sit in Tashkent for five days. You could leave after three days, after you bought your groceries and beer and headed out to the field. I learned after a while, and silly things, like they love stamps. I don't mean to make fun of this at all. This is just the culture. A stamp was official. A stamp was made, "Department of Biology, San Diego State University," and I just would stamp things. It didn't mean anything, but they liked the stamp, so I had the stamp.

01-00:15:06
Burnett: "You must have the papers."

01-00:15:07
Archibald: Must have the papers. We had the papers. Back to the question about the difficulty of doing work there. I was getting permission. We had some good colleagues there, that were not paleontologists, that helped us get in and do the work and so forth. We would try to be open and honest, but it's sometimes like feeling, grabbing at things. Who was the person that was going to tell me can I take them out? Yes, you can take them out, and so forth. It became somewhat of a difficulty at times. But as one of my graduate students said, "David, you're happiest when you're in the field." What he was pointing out was that I was there with professionals and we were ready to do work. I'd had no other concerns other than doing the work. I was away from the city, Tashkent. We had paid everything. We had grants, so we had the money, and we could do the work.

01-00:15:51
Burnett: And that's at its most satisfying.

01-00:15:52
Archibald: Yes, absolutely.

01-00:15:54
Burnett: Can we go back to your training at Berkeley and working with Bill Clemens? Can you talk a little bit about how Bill Clemens's approach shaped your approach to paleontology?

01-00:16:09
Archibald: There are some very exact ways that it did affect it, and those are easier than all the philosophical things. One way is, we did what was called screen washing, that archeologists had first done. Malcolm McKenna was one of the first people to bring it to paleontology. Another fellow at Michigan did it as well. We'd have these huge boxes of various meshes of screen, down to half a millimeter or so, because these are tiny teeth. You would take the dirt, or the matrix, and put it in the boxes. You'd let it sit in the water, and then you'd screen it and so forth. Then you would take it out and dry it and pick it. Sometimes you'd ship it back to Berkeley and work on it here. The first times

I did this work in the American West, I did the same thing with these large boxes. It were a bit back-breaking to do this work. I'm not saying this was the wrong way; things just change. I followed what my advisor had done and I did it the same way. Also, in terms of taking field notes and taking photographs, I followed the same thing. My writing was a little bit messy, but at that time, this was the age of polaroids and so forth, so you didn't have digital cameras. You would take slide pictures and so forth, but you didn't know if they were going to come out, and so you wanted to have records of localities. In a number of the notebooks, we'll have polaroids there. I don't know how long they're going to last, but you have pictures of these areas. We didn't have GPSes at the time, so you used township and ranges. I kind of knew how to do this, but Bill helped me more how to get into—recording localities I guess is the way to say it. These were things about how you approached the subject.

Philosophically—you didn't use that word. I'm using that word. Bill is very methodical in the way he works. Sometimes people would kid him about it. One student put on his door, "infernal turtle-ization." One can say it's picking on Bill. It's not. I would have taken it as a compliment, because he was very methodical about going through things, working on things, taking his time to think about them. I think I'm a little more brash, but learning to be a little bit more careful about these things, I think I got from Bill. It did help that we had mutual interests that were the same.

01-00:18:45
Burnett: That may be a good segue. I don't want to cut you off there. One of the things that happened shortly after you finished your PhD at UCB in 1977, and in 1980, there was the Alvarez hypothesis, the extinction hypothesis, which generated a great deal of excitement and controversy, and still excites some controversy today. I was reading one of your books here about your initial reaction to—

01-00:19:27
Archibald: Which book is that?

01-00:19:28
Burnett: This is 2011, *Extinction*— [*Extinction and Radiation: How the Fall of Dinosaurs Led to the Rise of Mammals*]

01-00:19:32
Archibald: Okay, so moderately new.

01-00:19:33
Burnett: Yes, not the most recent. You wrote about your reaction to this hypothesis. I think what you said was, why was there this need to have a simple cause to explain this? This is a complex historical phenomenon, biological phenomenon, and needs to be addressed as such. Could you talk a little bit about your reaction to this hypothesis and how Bill Clemens and others reacted to it?

01-00:20:18
Archibald: As you mentioned, the paper by Alvarez, et al, was published in *Science* in 1980. The, I think, first reaction by a lot of people was, oh, no, it just can't be true, but the evidence kept coming forward that, in fact, there had been the impact of an extraterrestrial body, whether it be a comet, asteroid, or some other feature. I'm not sure what people are now arguing was likely. I think asteroid is probably the most common cause. I think the acceptance and denial about this goes through several stages. It first was the concern more about, oh, the extinctions are clearly gradual. I think I would have probably been in that camp—I was in that camp—versus having a very rapid, or, if you will, catastrophic, extinction. That was the first way that this was presented. Over time, this kind of morphed into—it didn't change, but it became important to have a terrestrial versus extraterrestrial cause, so it evolved this way. Finally, today, the way it's really discussed is whether it's a multiple cause versus a single cause. I think what I can say is that my view about the effect of the impact has been far more accepting over time. The part that I don't accept is, I do think it's a multiple cause, and I've written about that, in fact, the book that you were talking about in 2011. I see it as a multiple cause. It was all of these factors contributing to the extinction.

01-00:22:02
Burnett: That was clear to you and others. It just seemed immediately clear based on the research that you'd been doing in the 1970s, it seemed.

01-00:22:10
Archibald: You mean about multiple causes? No, I don't think it was immediately clear. I think what started to emerge was more evidence about the impact, where it occurred, the size of it, and in terms of volcanic activity, the Deccan Traps in the Indian subcontinent. Gerta Keller has been one that's pushing this a lot. Getting closer and closer in terms of our timing of that, and I am sorry, I don't remember the timings. One of the major eruptions is near the K-T boundary. One that I pushed, and it's not popular with a lot of other people, is the loss of seas, I think, changed the climate a lot. We have the greatest loss of seas at that time than in the last hundred-and-some million years. You added, basically, a continent the size of Africa. North America was split in two, and you lose the seaway rather rapidly. I think it's the—Americans like to use sports metaphors—the one, two, three strikes you're out. One of the, I think, curious things, a thought problem, is that if one of these things had happened, or two of these things had happened, but not with the other, what would have happened? I don't know. It's a mind game to try to decide what would happen. But obviously, it became extinct, and I accept that. People make a big issue about whether a few dinosaurs straggled on or not. I don't think that's material to what causes the extinction. I definitely accept that there was some sort of, I assume, asteroid strike, volcanic activity that changes the climate, and the loss of seaways that also changes the amount of habitat in which the dinosaurs we know were living in. They may have been living elsewhere, but we know we lost part of the habitat they lived in, the Hell Creek Formation, for example. Again, for me, it's a multiple cause thing. You have to question, did we know

that in 1970? No, we didn't. This is a historical science. It's not an exact science. The information has kept coming forward and coming forward and coming forward.

01-00:24:15
Burnett: It seems that, I guess, perhaps the research was in retrospect, but what you were working with was evidence of extinctions or a reduction in species diversity in the millions of years prior to the extinction. Is that something that becomes clear later?

01-00:24:39
Archibald: The work I did really could not say that. The only thing is—and there's still some controversy about it—in the last ten million years, and we knew this in the seventies, using material from Canada, where it's better known, ten million years before the impact, the numbers of species in general, dinosaurs, decreases by at least 40 percent into the Hell Creek, and all the people try to argue around that. There is a decrease. There's nothing that really can dispute that, even though people have tried. I'm rather emphatic about that. That's the evidence. The part, however, is what happens in the last million or so years in the Hell Creek Formation and other comparable beds. At the time that I was working on it, we really did not have the kind of detail that's now available. Some of Bill's other students, while Bill was working there, were Greg Wilson, Mark Goodwin, and Jack Horner. There has been new material done, Pearson as well as others out in the Dakotas. And I don't agree with some of the results—it depends on how you approach the problem—by David Fastovsky and some of his colleagues. I don't think the data was so good. But be that as it may, the answer is no, we did not have that information. We could not say that they were going out at a gradual rate in the seventies. We did not know that.

01-00:26:07
Burnett: That question wasn't necessarily being posed, because there wasn't someone coming along and saying everything died at this moment, at this exact moment.

01-00:26:14
Archibald: They were saying this. Yes, the single-cause idea was that, yes, an asteroid hit, and, bang, everything is gone within—now, be fair, some people arguing acid rain, darkness, thermal—I can't think of the phrase, but it was like being in an oven. Also people said it became too cold. So there's combinations of things. People who are pushing for a single cause couldn't completely, and still don't completely, agree with each other. But the point here is that a lot of these things have fallen by the way. There's not good evidence for those, and so people were arguing at the time that everything that died, died at that instant. The evidence just is not there for that.

01-00:27:11
Burnett: If I could generalize, it seems like the work of UCMP in vertebrate paleontology, Bill Clemens's group and others, has been very much invested in recovering as much of an understanding of small vertebrate diversity in the period that they're studying. So, there is this movement, I guess probably worldwide, away from the big dinosaurs, because that was the sexy thing that was being done for ages. Because of an interest in really parsing out the evolutionary changes, you needed to get a sense of the larger biota, all these different species that had been overlooked. And literally overlooked, because it takes screen washing to find some of these fossils. They're so tiny. I read something from your own work, in the 1996 book, that talks about the paucity of dinosaur specimens, talking about, I think in this area that you were working in, diversity is being estimated for dinosaurs from fewer than 100 incomplete skeletons, compared to estimates of small vertebrate diversity from a database of 150,000 teeth and bones.

01-00:28:35
Archibald: Have to make it clear, we did not sit and count to know that we had 150,000. We just knew the drawers. You go to the drawers and find hundreds there. This was a seat-of-the-pants guess. I'm sure it's within a magnitude. That still holds that the number of dinosaurs based on specimens—the mammals are pretty fragmentary, too, the little mammals, but the numbers of specimens you could identify as being individuals is far higher. But I have to make it clear, that's the same as if you go to the Serengeti in Africa and the number of gazelles is generally—it's not always true—is lower than the number of rodents that are living in some of those environments, or monkeys or whatever. Not always true, but often the smaller you get, the more individuals you are. So there's nothing odd about that pattern that you describe. You see it both in the modern biota and you see it in the fossil record. It gets biased, of course, by fossilization.

01-00:29:37
Burnett: What gets preserved.

01-00:29:38
Archibald: Right. There really hasn't been a shift towards looking at the smaller vertebrates. The public is always going to be more interested in dinosaurs than they are the small mammals. People ask did I ever collect dinosaurs. Yes, and I said I kind of kicked them out of the way so I could get to the small things, jokingly. Their interest hasn't shifted this way. The thing that I think you can say, what Bill has been very instrumental in doing, is bringing many generations—and I'm kind of at the older generation, but not the oldest, and to the youngest—of working on these things and continuing to work on them after they've left Berkeley and developing new ideas. Bill has been, maybe besides being a mentor, a good catalyst for other people doing this kind of work. Yes, you're right. His interests are in the small mammals, but other people have done the turtles, the amphibians, the birds, whatever we could get our hands on that would tell us what the biota is like, not just the dinosaurs.

01-00:30:45
Burnett: It also seems, perhaps, that, getting back to the Alvarez extinction debates, that there's general interest in getting a fuller picture of the gradual evolution. That's kind of the orientation. Those are the questions that you're asking. So it stands to reason you'd be interested in the other kinds of multiple causes for extinctions or the emergence of new species. You might have the recession in the Pierre Seaway and the Deccan Traps volcanic explosions, and also the asteroid impact, but what matters is the continuity and the discontinuity. The K-T impact hypothesis was just focused on discontinuity, but your whole research world is about the continuous and the discontinuous, and that was—

01-00:31:52
Archibald: Yeah, because things obviously became extinct. The rates of extinction of vertebrates, depending on how you count them, range from 50 to 70 percent. It's not the highest. The ones at the end of the Permian are supposedly the granddaddy of extinctions, where it's over 90 percent. But there's the continuity, because things obviously survive. You mentioned something about gradual evolution. In fact, at this time, if anything, I would say the process of evolution sped up. Again, we're hypothesizing, because we don't really have a comparison, and the extinctions we're causing today are probably faster than what was going on then, but things either adapted or died, became extinct. So you did see, when you go from one place to another—or, I mean, one time to another, across the boundary—you see some profound changes, a combination of extinction, a combination of immigration, which Bill has worked on a lot, and evolution. Trying to tease those apart—what's immigration, what's evolution, what's extinction—is sometimes difficult to do.

01-00:32:57
Burnett: That's partly the work on the emergence of mammals as well, is that there's a kind of ecological space that's vacated by the dinosaurs.

01-00:33:10
Archibald: I think that model still works. They're gone, and so things fill that void. Life abhors a void. It will fill it, if at all possible in any kind of form. Evolution is very opportunistic. Sometimes people talk about it as a terrible term, the "survival of the fittest," and one colleague says, "No, no, no. It's survival of the adequate." Whoever can reproduce and leave more in the next generation, those are going to survive and pass on their genes. Again, at this time, I think it was very rapid, and I think it screwed around with some of the so-called molecular clocks, too. I think it increased the rates of change in that as well.

01-00:33:52
Burnett: Oh, could you talk a little bit about that?

01-00:33:54
Archibald: Nobody is working a lot on it, but there is apparently rates of change and—I should have read about this before we talked, because I can't remember the exact parts of it. I do mention it in the 2011 book, but the rates of chromosomal change. There was, it seems to be, a jump, as I recall, near this

boundary. Nobody has made any big deal about it, and I think there may be something to that. What happens is, when you use a molecular clock, in some instances it appears that things are farther back in time, and I think this very dramatic change, lots of extinction, lots of speciation, that it may compress that clock somewhat across the boundary, making it appear like it should be much farther back in time when it is not. I could be wrong, but I think there is something there that really needs to be looked at more.

01-00:34:47
Burnett: So there's an increase in selection pressure that these things have to adapt, and it increases the speed and perhaps mutation rates as well.

01-00:34:54
Archibald: Right. Well, there was a combination. I argue in this book, again, that actually, for a while, the selection pressure dropped off, because you're the only ones that are around. There's nobody you're competing with. Then there starts to be more of you, and slightly different ones of you, and then you start to compete, so the selection pressures go from being kind of normal to diving down, for those things who survive. You could say the selection is pressure very high, because if it's too high, you're dead. But for those who did survive, I think they didn't have competitors to go after whatever was left for them to go after. But then as more things showed up, by immigration, by evolution, the screws were put to these organisms and the selection pressure increased dramatically. That's my kind of harebrained idea of how, probably, it happened.

01-00:35:46
Burnett: I read somewhere, I think, in your work that—there's kind of an ode to the mammals, that you start off one of your books with. That they have these great adaptations that allow them to range more widely, that allow them to survive more readily than other kinds of species.

01-00:36:11
Archibald: Use an example. Did I interrupt you?

01-00:36:12
Burnett: No, no, no. Go ahead.

01-00:36:14
Archibald: I think I know what you're referring to. We can talk about success, evolutionary success, in various ways. More species, bigger animals, and so forth, bigger brains, or whatever. But in terms of ecological diversity, and I mean it only in that way, mammals seem to—I would argue are the most successful of the terrestrial vertebrates, the tetrapods. There are more species of birds, and birds belong with the reptiles, so there are many more, like three times as many probably, something like that. But if you look at what is the largest vertebrate that ever lived, whale. What is one of the smallest—and that's not the smallest—is one of the shrews. I've seen some of the lizards in Madagascar that are tiny. You can hardly see them with the naked eye. We do

have some smaller lizards, but mammals get that small as well. In terms of being aquatic, flying, burrowing. What I think you see is ecological plasticity in mammals that you don't quite see in other at least tetrapod groups. I will beg off on comparing to fishes. There are several reasons for that. One, unlike birds, you got locked into 180 million years ago to being bipedal with hands, the dinosaurs and then became birds, mammals, their ancestors, the mammals, were very generalized in terms of their feet, and so we get huge diversity from hooves to wings to claws to our hands. That kind of diversity was possible because you weren't so specialized. The kind of reproduction of—especially in placental mammals. Marsupials are successful, but in a slightly different way than placental mammals, because the female takes care of the young internally, and then she feeds it with milk. Then, also, you can talk about brain size, is pretty important. There are some other factors, but all of those combine together to allow, I think, mammals to be very ecologically diverse, which other tetrapod groups are not as diverse that way. It's luck of the draw. It's evolutionary luck of the draw.

01-00:38:43
Burnett: You've published a number of books on the dinosaur extinction and the rise of placental mammals. You've retired, finished off your career as a paleontologist, and you started a new career. I was wondering if you'd talk a little bit about that.

01-00:39:05
Archibald: I'm playing in somebody else's sandbox, yours. History. I'm doing work on history of science, specifically in biology. I've always been interested in books. I'm a book collector. A lot of paleontologists—I don't know about the newer generation, but my generation—loved to have reprints and books, old books, of the people that had been two or three generations before. Also, just the general interest in history, I think, is kind of in the genes of paleontologists. Without going in great detail about how it came about, I did a couple of articles for history of science journals, both in England and the U.S., and then I got interested in expanding this into a book-level thing, which just came out last August and is called *Aristotle's Ladder, Darwin's Tree*. It's about the visual representations of biological order. Not of evolution, because you go back to the ancient Romans and it's nothing to do with evolution. It's not even anything to do with organization of life. It's about annual cyclicity of things coming and going, coming and going. The Christians picked this up and so forth. I've been very interested in that, and I started a new book called *The Paradox of Darwin's Fossils*, which we don't need to go into. I am interested in history of science. I'm still working with some of my Russian colleagues on some mammal papers, but not very much, and I'm done talking about extinction, other than in a historical context. It's very interesting now to kind of go back and look at my work and other people's work and use these as examples of visual representations. I did this in the book. I work on mammals, so they probably got a little bit more play than other ones. Seeing these trees

in different ways now, rather than the scientist doing the work, but rather historians saying, huh, what were they trying to mean?

01-00:41:00
Burnett: Absolutely. These representations are clues—they're representations, but they signal ways of seeing.

01-00:41:10
Archibald: Metaphors.

01-00:41:11
Burnett: They're metaphors, and they're ways of understanding. They really are a window into the conceptual foundations of any kind of science. Historically, the history of geology can tell you something about the early resistance to the impact hypothesis because of uniformitarianism.

01-00:41:35
Archibald: I think here we'll talk on this. There's a little misunderstanding about uniformitarianism. I don't remember the exact terms. Actualism, and then there's another term. The idea of the present is the key to the past is one aspect of uniformitarianism. The other one is that the rates in the past are the same as today. We're rejected that long ago. The idea of an asteroid impact not fitting uniformitarianism is totally wrong. It does fit—we just didn't understand the importance of it. Both Alvarez's [Luis and Walter Alvarez]—great work, fantastic work they did, and we found other ones, and sometimes we've not found them when we thought we'd find them. Or the possible importance of comets hitting the early earth. This kind of impact from extraterrestrial sources fits the uniformitarian model. If it doesn't, the scientists, we'd be in trouble if we can't use processes we see today and go back in time. Of course, those processes do change. People who came into this, especially some of the people—astrophysicists and so forth—says, oh, we're going back to Cuvier, and they also misrepresent Cuvier. But it does fit the uniformitarian model in the sense that the processes operating today operate in the past. We've had examples, like over Russia and stuff, where we've gotten comets and—I can't remember if that was an asteroid or a comet. It flew across and broke windows. I can't remember if people died in that or not, but they're happening now—

01-00:43:06
Burnett: In 1908?

01-00:43:07
Archibald: No, no, no, very recently. Not Tunguska. I don't think anybody was hurt in Tunguska, which just blew things down. What's happened is that the idea of extraterrestrial impact has been brought into the fold of uniformitarianism. We have to realize back in the nineteenth century, people didn't accept some of the other processes, the importance of volcanic activity, and these have all been brought under that umbrella.

01-00:43:31
Burnett: And the notion of geologic time as we understand it today was very much, much shorter.

01-00:43:38
Archibald: Yes, absolutely.

01-00:43:39
Burnett: Everyday occurrence has a different meaning when you're talking about several billion years.

01-00:43:43
Archibald: This is the appreciation of deep time. If anything else, in paleontology we have an appreciation of deep time. I can't think about what a million dollars might look like sitting on a table, but I can think about what a million years is like. Or talking about this book. One of my favorite places is to sit in Rome and—to tell something about my bad habits: I'll have a cigar and a grappa and watch all the people walking around. The Pantheon is one of my favorite structures that exists. It still exists, because it was made into a Catholic church. It says "*M. Agrippa Fecit*" on the top. It was actually built by Hadrian, or rebuilt by Hadrian. To sit there and think about what's gone on in those 2,000 years that people come and go and come and go and come and go. How many people were killed there? How many people were made love to there and so forth. Take that 2,000 years and expand it by a magnitude of a hundred and you get millions of years. I think one of the things I do feel fortunate is I can think in millions of years. I have this concept of that. I think those who have a geological background, whether you're a paleontologist or a hard-rock geologist, you have that concept of time. I'm sure Bill was one of those instrumental in helping me get that kind of concept.

01-00:45:05
Burnett: It has, I imagine, some kind of impact on how you see life, right, to some extent?

01-00:45:13
Archibald: Yeah, I guess it does. I have to be careful, because I live in the physical world rather than metaphysical world.

01-00:45:21
Burnett: [laughter] Right. I just wanted to close by asking you about the influence of Bill Clemens on your work, UC Berkeley's shaping of you as well, just the institution. You talked about the cohort of graduate students, and I would agree with you that graduate student cohorts are enormously important. The institution itself, did it leave its mark on you as you went out and learned about other institutions and other kinds of formation that people had at other universities?

01-00:45:55
Archibald: Let me back up to when my first wife and I drove in to the campus. I still remember coming up the campus, coming up—it's University, that's the main

road up to the campus—and going, “Wow, we’re here.” It really did have an impact on you. You can’t diminish that. And then becoming a grad student, [let’s] make it clear it’s a lot different being undergraduate. I would work until eleven o’clock at night in the little lab we had, and I’d go home and sleep and go back and so forth. You weren’t as involved in campus activities as a graduate student as you were as an undergraduate. At least I was not, and most of my friends were not. Not that we weren’t sometimes wild and crazy, but not the same way as when you’re an undergraduate. You’re driven by what your interests are. The campus life definitely affected me, but I think within the confines of the paleontology department of that time were more instrumental. What Bill offered me was doing this fieldwork, and I can’t thank him enough. I had some field experience before, as I mentioned, but I think doing this kind of fieldwork and entrusting me one of the years to, as I said, run the field camp, that was great. I’ve run my own field stuff kind of based on Bill’s model for many years. I’ve changed it over the time, over forty years, but that’s very important. And serving as a catalyst. I did take classes with him, paleo-mammalogy class, which, by the way, I modeled my course on I taught at Yale. I taught modern mammalogy in San Diego, which I like to do because it’s more diversity. You can talk about reproduction ecology and so forth. When you’re talking about mammal teeth, it’s a little hard in a paleo course, but Bill’s was just a superb course. I learned a lot from him there, but it was more in the fieldwork. You get to know people very well, what they’re like, doing fieldwork with them. It’s a very interesting sort of way to learn about people.

01-00:48:07
Burnett: Absolutely. It’s a whole work culture unto itself.

01-00:48:11
Archibald: Absolutely.

01-00:48:12
Burnett: Definitely. I want to thank you for your time. We really appreciate you taking the time to talk with us.

01-00:48:17
Archibald: No problem. It was enjoyable to talk about Bill.

[End of Interview]

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Berkeley, California

The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology:

Mike Novacek

Interviews conducted by
Paul Burnett
in 2015

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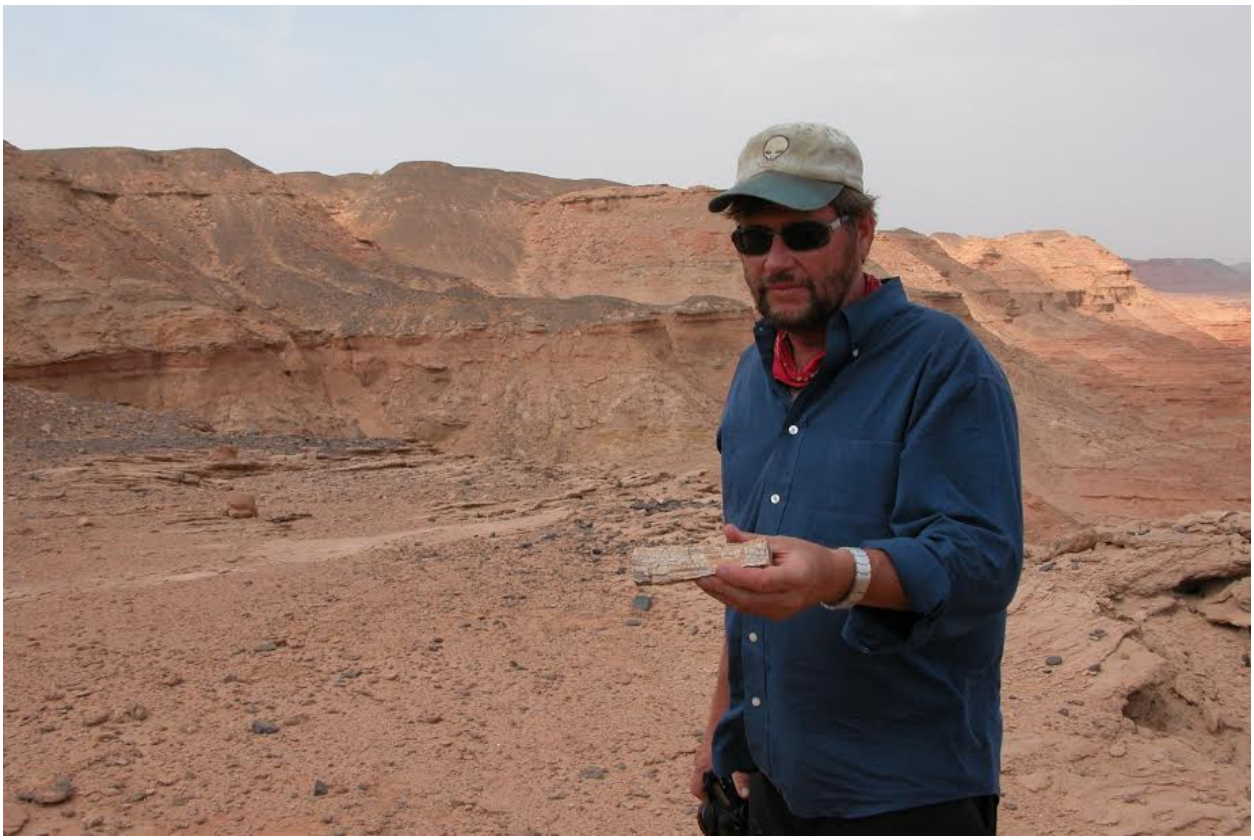
It is recommended that this oral history be cited as follows:

Mike Novacek "Mike Novacek: The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology" conducted by Paul Burnett interviewer in 2015, Oral History Center of the Bancroft Library, The Bancroft Library, University of California, Berkeley, 2017.



Mike Novacek

Mike Novacek is Senior Vice President and Provost of Science, and Curator of Paleontology at the American Museum of Natural History. He received his PhD from UC Berkeley in 1978.

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Burnett: This is Paul Burnett interviewing Dr. Michael Novacek for the UCMP/ Bill Clemens Oral History Project and it's April 5, 2016 and we're communicating via Skype, and Dr. Novacek is in New York City, I presume, at the American Museum of Natural History. So Dr. Novacek, I took a look at your *Time Traveler* book from 2002 and one of the questions I've been asking folks in the project is about the romance of paleontology and I was thinking of that when I was reading about your recollections of early childhood. Can you tell me a little bit about where you grew up and when you got bitten with the bug of paleontology?

01-00:00:55
Novacek: Okay, sure, Paul. Yeah, I think I was infected at an early age. As far as my memory goes, maybe at the time of birth, I don't know. So I was very young when I became interested in dinosaurs and fossils, but also really interested in science and a lot of other things, too. I just loved to read. I remember in third grade especially reading books about science, fairly sophisticated books, a Time Life book about the history of the earth. I think I mentioned that. But I also, in a somewhat perhaps overambitious mode, even tried to read some chemistry textbooks that were sitting on my mother's shelf from her college days at UCLA. I'd got a series on science and I pretended that I could understand them. But fossils were really probably my biggest love. I read all about dinosaurs. Probably about my favorite book in the third grade or so, when I was seven years old my favorite book was *All About Dinosaurs* by Roy Chapman Andrews, who was a curator here, of course. It was all about the expeditions to the Gobi Desert and all about some of his other adventures, and then a lot about fossils and prehistoric animals and dinosaurs. His second book was called *All About Strange Beasts of the Past*, or the second book that I read from Andrews, and that featured and opened with this riveting chapter about La Brea Tar Pits. And, of course, I was in LA, so what a great coincidence. So as you might expect, I really almost demanded that we go to the tar pits as much as possible. My parents were fortunately very encouraging for that kind of thing, so we visited La Brea very often and I used to imagine what it was like to be there 15,000 years ago. And the tar pits still have a real primitive kind of sense to them, a kind of primeval atmosphere. You can still see tar bubbling up and even, once in a while, evidence of a rabbit or something that may have fallen in the tar and been entrapped. So you get a sense of the power of that. So that was transforming.

And having the Los Angeles County Museum nearby or downtown and all these incredible skeletons that had been removed from the tar pits that were put together and mounted. The mammoths and the giant ground sloths. Some of those are here at the museum, as well, from that time period, not always

necessarily from La Brea. So it was a great experience and it reinforced my interest in paleontology, to say the least.

01-00:04:33
Burnett: And you also remarked in your book that there was an enthusiasm in the popular culture for dinosaurs but usually of the monstrous variety and there are all kinds of science fiction representations of dinosaurs that continue to this day.

01-00:04:51
Novacek: Sure.

01-00:04:53
Burnett: But I think a lot of people at one point in their lives think about the romance of the dinosaur hunters. What do you think allowed you to persist and pursue this as a career when other people just do other things with their time?

01-00:05:13
Novacek: Well, it's kind of interesting. I won't say I lost interest but it was kind of a backburner by the time I got to high school. My father was a jazz musician. That may have had something to do with it. He played guitar. But I really got entranced by music and all kinds of other normal high school distractions. I was still intellectually engaged in a variety of things, including literature and writing. I really started, I guess, on the intellectual side of my activities as a teenager. I did write. I wrote a lot. But not necessarily about science and not necessarily about dinosaurs. I wrote poetry. I wrote sort of short stories. And then when I really got involved in music I wrote a lot of songs. So high school and a good chunk of college, school or careers in science or professionalism wasn't really on my front burner.

On the other hand, even in high school, on occasion—I had a friend whose name was Terry Schofield. And Terry was the best basketball player at our high school and maybe the best basketball player in the state of California at that time. He later played on the UCLA Bruins and he was the sixth player on the team when Lew Alcindor was on that team, Johnny Wooden's team. So that was one hell of a team. Well, Kareem [Abdul-Jabbar] was on that team. So Terry played in a lot of games with Kareem and everybody else. So he was a star sport guy, good friend of mine. And he loved fossils, too. He liked to go out and collect. And what we generally did in high school is we'd go out to places like Hughes Aircraft. There was a lot of vacant lots out by Playa Del Rey. There was a high school up in the hills there that had a bunch of hills on the campus and they were old Indian sites and we dug up arrowheads and all kinds of stuff. And it was fun. We just spent the day doing that. And I probably went up with him to the Santa Monica Mountains and collected fifteen-million-year-old clams and other things, fossils from the Miocene that were deposited in the rocks along the roads, the road cuts there. So it was a kind of pastime but not necessarily something I thought I would do the rest of my life.

01-00:08:02
Burnett: Right, right. And, of course, in the teenage years there is an explosion of interest in all kinds of things. And you were a musician and a songwriter. You were in a band, is that right?

01-00:08:11
Novacek: Was in a band, a number of different bands. We were terrible when we started. I remember our first job at Saint Monica's High School, we were so bad that a bunch of guys in the audience replaced us. [laughter] after the show.

01-00:08:37
Burnett: That must have been humiliating.

01-00:08:38
Novacek: It was humiliating but it was probably the best learning experience. They say this has happened to so many musicians. Best learning experience we ever had. We said, "That will never happen to us again." And we got good. And I remember Saint Monica's would never hire us after that debacle. But then as we were getting better, a year later or two, we had built a kind of great reputation. We were being hired all over the place, at Venice High and Santa Monica High, the high schools, and big dances and community dances. We were too young to play in clubs. And so Saint Monica's, the student president, came back to us and asked us to play. And we said, "Well, I don't know." We demanded more money. And we finally did. So I was really into it. In early college we were intensively involved. We were starting to do some recording work. MGM Records was interested in signing us on, so we did some recordings with them. The band lived together in a house. It was the height of all the interest in rock and roll with Jimi Hendrix and Led Zeppelin and later Cream and all these bands. So it was a great life to be in LA. And that was pretty much engulfing. School was a way to stay out of the draft, frankly.

01-00:10:16
Burnett: Right, right. But that sounds like more than just a hobby or a teenage pastime. This sounds like a serious endeavor. And so there's a transition point between that in-depth involvement in music and completing your undergraduate degree in zoology.

01-00:10:41
Novacek: Yeah. In some ways it's the fundamental transition in my life, which I probably never quite completed. Well, the last five or six years I've been heavily involved in music again, and playing intensely. I haven't played much with other musicians, but a little bit. But I'm totally enraptured with it again. It's almost like I never left. But I don't regret the choices. The choice was interesting. It turned out that it was a rich life. But you can't always assume that you're only going to do one thing all your life or you'll just do one thing. You might do two things or three things your entire life. Playing for me, writing those books, is really not what I'm paid for, those trade books. If you're a scientist and you're looking for advancement, or you're an administrator like I am now and spending a lot of time raising money or

managing and doing all these other things, you're not being paid to write trade books. But I find that tremendously fulfilling. So I've always been a little bit schizophrenic in my approach to life. But the thing about college was I was hanging in there. As I said, I was still interested in biology, was a zoology major at UCLA, and then I met a paleontologist.

01-00:12:24
Burnett: Yeah. Who was that?

01-00:12:26
Novacek: His name was Peter Vaughn. He was a young professor. I think he was already tenured, though. But he was a young professor who had come from Harvard and he was trained by a very, very famous paleontologist, Al Romer. Al Romer wrote the central textbooks on vertebrate paleontology that we all read and he was a remarkable man, a larger-than-life individual. And Peter Vaughn was nearly his last graduate student. Maybe there was one younger than he. I was a junior at UCLA. Well, I wrote about it in *Time Traveler*. I was taking a course from him. It was a comparative anatomy course. And he had a sense that I was a pretty good student. He took an interest. I had more than a connection. I learned a lot of things about skeletal anatomy and so forth as a kid. And so it was natural for me, like learning a language in your early years. So he took an interest in me. I came to talk to him about something, a question I had about an issue in evolution and he told me later, he said, "Well, this guy could be a scientist. He thinks like a scientist." And I was a junior. So he went out every summer to Utah and Colorado and took his graduate students. He said, "Would you like to come in the field with me?" I said, "Well, I don't know because our band has a lot of work over the summer and I don't know if it would work out." And he said, "Well, see what you think." And then it turned out that we had a couple of dates in July that were big dates in, I think, San Francisco or somewhere. There were certain things on the line. And those got canceled. And we were kind of on a hiatus and I could spare the time. And then I just said, "Well, I need a break anyway." I'm not sure the other guys in the band, including my brother, were too happy about it. But I said, "I'm going to do this." And they thought this was crazy, said "What?" "I'm going with this professor and we're going to go collect fossils in Utah and New Mexico." They said, "Are you nuts? You don't want to do that." But I did. And I think that was a fundamental change, obviously, as I wrote about it.

What happened is after several weeks of working there, it just dawned on me—just being out in the country. We didn't find many fossils because he worked in the Permian and it's hard to find fossil reptiles in the Permian, especially in places like New Mexico and Utah. But I think just being out in the desert and being in those canyon lands and living out there and camping there and I said, "Holy moly, this is incredible. It's beautiful and it's inspiring. It's fun. And this guy's getting paid for this. He's getting paid. This is a job. He has a comfortable life. He has a nice house. He has a grand piano in his

living room. He's interested in music. He has all kinds of interests. He's a very smart guy and he's sort of taken me under his wing." There was a gravitational force pulling me, attracting me. And, yeah, I have to say people were so encouraging; he was, the older graduate students, professors I met subsequently.

Meanwhile, the business side of the music was getting worse and worse. The two things were in collision. It was really getting depressing. There's a new [television] series out, it's pretty bad, but I've seen some of it, called *Vinyl*, about the music business in the seventies, which was exactly the time I was involved in all this and it shows the seedier side and the corrupt side and the bribery to get songs on the air. And we had a fight about the songs I wrote and who would publish it. Well, a long story. So the business was getting more and more of a downer. Everybody wanted to be the Beatles and that's impossible. We didn't know when we'd break. A couple of guys in the band quit. My best friend in the band, who was the pianist and he's the other singer, and he and I wrote most of the songs, he went into filmmaking. His dad was a famous choreographer. He was at USC and he finally said, "I got to commit to this now. I'm really involved." And he later became a well-known Hollywood director. He made a number of hit movies and a number of failures, as well. He chose that career. I think he had a lot to do with me thinking, "Well, this other thing seems to at least have a future and people are very cultivating."

And I don't remember really consciously making the decision but I just kept going. I graduated from UCLA. Then I went to San Diego State for a couple of years and got a master's, and then up to Berkeley where I met Bill. Even when I was in graduate school at San Diego State, the first two years, I was still coming up to LA and playing music. I still had a lot of connections in the music business. But by the time I moved to Berkeley that had kind of really tapered off.

01-00:18:46
Burnett: Right, right. Well, it's something that a lot of other interviewees have talked about, is the people and how important the people are in paleontology and they are characters. It sounds like, from your memoirs, that there are some *real* characters in paleontology. And you have to be that way.

01-00:19:07
Burnett: It almost sounds like you have to be that way to go out into the middle of nowhere and do what can be, by turns, incredibly tedious work and incredibly exalting work in the rare moments when you have a discovery. But the people are a really important piece of it. And you met a number of folks. Did you work with Jay Lillegraven in the master's level?

01-00:19:34
Novacek: Yes, I did.

01-00:19:36
Burnett: And so you're already sort of getting into that orbit around Bill, it seems.

01-00:19:42
Novacek: Well, no kidding, yeah, because Jay was Bill's student, maybe one of Bill's first students.

01-00:19:47
Burnett: Yeah, his first student.

01-00:19:48
Novacek: And his star student. Bill was very proud of Jay and his accomplishments. So, yes, I was already in the orbit.

01-00:20:00
Burnett: So can you talk a little bit about UCMP when you started up there? This is now in the mid-seventies, I suppose, early mid-seventies.

01-00:20:12
Novacek: Yeah. There are a couple dimensions to this. It's kind of interesting. On the academic and professional side, in a sense I had a lot of experience. When I graduated from UCLA I had a lot of on-the-job experience. So when I graduated from UCLA I worked at the [La Brea] Tar Pits. I actually got a job there supervising the reopening of a tar pit and the dig there and also curating a lot of the stuff that came out. So I learned a lot about identifying fossils, bones, parts of skulls, a lot of things, and then doing the work. And then that was for about six months or eight months. I forget. And this was before I was heading for San Diego State. I took a break between my bachelor's and when I started the master's program.

And then in San Diego State I had a lot of practical experience because the project was actually excavation and I had to describe the fossils that I had found. And it turned out to be a very challenging group of fossil mammals, small fossil mammals. There were a lot of problems in understanding the evolution of these mammals, so I took up the challenge. So when I came to Berkeley I was probably both more experienced in terms of the work and more inured to the challenges and the hard things to do in paleo. And so I think it's fair to say that I kind of was greeted with that expectation. And some of the other students perhaps didn't feel that same sense of confidence. I had met people from Berkeley, not Bill, but Don Savage had come down to San Diego on extended periods while I was a master's student. And Malcolm McKenna from New York: I had seen him and he was like one of the golden boys of paleontology at that time. And Malcolm tried to get me to come to Columbia to work with him. So I chose Berkeley after a lot of soul searching and ironically here I am. One thing, I thought, "I can't live in New York. The ocean is east of me, it's not west of me," things like that. So that's one dimension.

I have to say, though, on a personal level, something happened on the eve of my arrival at Berkeley and it was just a curious event. I wrote about it in the book. I was moving furniture from San Diego to store it in LA before we went to Berkeley, my former wife and I. And we had a lot of stuff. And we stored it in my brother's garage in LA. But I had been driving back and forth and there was a huge heatwave. And I passed out on the freeway.

01-00:22:37
Novacek: And it was a very odd event because there was a guy who was living next door to my brother or visiting and asked me for a ride down to San Diego. And I said okay. And if he hadn't been in the car I would have been killed. Because he took over the wheel and drove me to a hospital. The nearest hospital was somewhere around Irvine. And it turned out I had massive heatstroke or heat—

01-00:24:07
Burnett: Exhaustion.

01-00:24:09
Novacek: Heat exhaustion. My blood pressure went up to some incredible rate. I remember overhearing the doctor say, "Does this young man know he's in a lot of trouble? He's in critical condition here." But I was released a day later and so forth. But I had lingering symptoms and it was very difficult. It must have damaged my inner ear. The very summer I wanted to go out in the field with Clemens and Savage, that was all planned, I couldn't go. I was totally immobilized and very depressed. And my first year at Berkeley was very hard on a personal level. Academically I was doing fine, but it was a huge struggle for me. It was hard for me to walk easily. I felt sometimes very disoriented and dizzy. The dizziness or the vertigo was the main problem. It was scary. So that's a lot of my memory of that first year. And then it gradually got better and at the end of the first year I did go out in the field. So there were two dimensions. One, the academic experience and strength and attention from people like Bill and Don, and then on the other hand this personal physical problem I had.

01-00:25:45
Burnett: Right, right. And then when you got out in the field, I understand you got ill from some water you drank. So there are a whole bunch of challenges that you seem to have to—

01-00:25:53
Novacek: Yeah, that was just a result of stupidity.

01-00:25:57
Burnett: But it shows you some of the challenges out there and a lot of the stories you tell are going into these remote places. You've got cultural differences to deal with. You've got the remoteness on a lot of those challenges. So, again, it speaks to having a good core group of people that you can rely on to get through some of those challenges.

01-00:26:18
Novacek: Clearly that's the case. The people I'm working with, the American Museum co-leader of the expedition to Mongolia, for example, we had our twenty-fifth season this year, but we've worked together for thirty years.

01-00:26:34
Burnett: Right, right. Yeah, absolutely. These long relationships. I'm jumping ahead here. Well, maybe we should proceed on through. What was your dissertation about?

01-00:26:51
Novacek: Well, it was about a very primitive group of mammals called leptictids. They're very insectivore-like. And I think it was just kind of lucky, and hard. But nobody really understood what these groups were related to. But the other thing is that they were very central to the question of how the modern group of mammals, placental mammals, radiated, how they evolved. We're a placental mammal, so are bats, certainly other primates, whales, this incredible group of all this diversity. How did that come about? And so this group was very primitive. And so sorting it out forced me to compare it with everything. Its relationships with any one of these groups wasn't very straightforward. So I learned a lot. I got a sense of the big picture of mammalian evolution. And I think the importance of the dissertation is that it, in its final parts, dealt with this larger problem. And in a sense much of my career, my scientific career, has been built around that in many, many papers. And then when all the genetics, the gene studies and everything else, the molecular studies came in, both the agreements and the conflicts between what genes were saying and what fossils were saying was very much part of my game. I was central to a lot of that interesting dialogue. And it continues. Yeah.

01-00:28:35
Burnett: Was that collections-based research that you were doing mostly?

01-00:28:42
Novacek: Sure. The interesting this is Don and Bill were trying to get me to do a faunal study. In other words, develop a program somewhat like Jay and somewhat like I did for my master's. That is, go out and collect at a locality and describe those fossils from that locality and do the stratigraphy. Berkeley had a very traditional kind of group in that way. But I had already done that. My master's thesis was already 400 pages long, 350 pages long.

01-00:29:19
Burnett: Oh, my God.

01-00:29:20
Novacek: In fact, a couple of outside professors, this guy from England who was a very famous paleontologist, Percy Butler, read it and then wrote to Jay, "Wow, that's a really amazing PhD dissertation." So I had done a lot of work already based on faunal work. So I said, "I think I want to cut to the chase. There's got

to be some great fossils in all these collections all over the country in different museums." So that's what I did. Berkeley had some of those fossils, New York, Carnegie, and nobody was working on this group. So I managed to collect all those fossils and bring them back to Berkeley and work on them. So that was the basis. So it was very collections-oriented, but collections, not just at Berkeley but many institutions.

01-00:30:09
Burnett: Right. In the 1970s there's an interpretative explosion in paleontology, too, right, this question of punctuated equilibrium versus the gradualist. Is that part of what was exciting for you, the paradigmatic questions?

01-00:30:27
Novacek: Yes, that was exciting because of the dialogue, although my work wasn't directly involved in that; what excited me more actually was the breakthrough in how you do phylogenetics or how you look at relationships in a much more scientific way, in a way that really was frankly a departure from the traditions of George Gaylord Simpson and the approaches that, frankly, Jay and Bill, the kind of work they were doing, which was much more classical ways of looking at organisms and species and their relationships. But I became really taken with cladistics because of the influence of some of the graduate students that were working in this museum, as well as Malcolm McKenna. And I was kind of a lone wolf at Berkeley in this. When I first started giving seminars and lectures about cladistics, as they call it, and its advantages, those were not well-received, I think even by Bill initially. Bill is a very careful thinker, and one should have rightful skepticism. So he wasn't immediately convinced that this was the approach to take, but I used that approach in my dissertation. It was probably the first dissertation out of Berkeley that actually took to doing systematics taxonomy, classical systematic evolutionary kind of work like that in this new approach. So that was a big deal. So it was an exciting time for that reason, not just for punctuated equilibrium and Steve Gould and Niles Eldredge and the stirring up that they caused, but it was also exciting in terms of looking at these relationships. And Niles Eldredge, one of the coauthors of all that work, of course, the senior author — punctuated equilibria was more his idea than Steve Gould's — Niles also very closely had embraced this more modern approach to taxonomy and systematics. He was a kindred soul in that way.

01-00:33:03
Burnett: Right, right. And so you transition, even before you finished your PhD you end up at San Diego State again in the department of zoology and you're a lecturer at first, then you became an assistant professor in the late seventies, and then associate. But then there's this transition. You flip coasts and you end up at the American Museum of Natural History. Can you talk a little bit about that transition? Is that related to your passion for the larger scope of understanding the relationships among all of these organisms?

01-00:33:43
Novacek: I think emphatically yes. Of course, a job here is the dream job. Not only the museum has probably the greatest public statement about fossils in the world, vertebrate fossils, and one of the most vigorous research programs, time honored and influential in the world for 150 years nearly, but the department happens to be well-endowed with its own funds. It was a lot of opportunity to travel and do some of the things that a lot of colleagues might not have. So it was a tremendously attractive opportunity professionally. There was no question. The problem was it was the most attractive job you could get, so there were a lot of other really good people who were interested in that. Actually, I think Malcolm McKenna wrote me a note and said one of the curators was retiring and maybe I should think about applying for the job, his job.

And I had spent a year here as a post-doc. I took a leave of absence from San Diego State, and got a National Science Foundation post-doc and spent a year here. Because San Diego, I had a tremendous teaching load, so I did a lot of research but it was under a lot of strain. And I said, well, if I spend a year at the museum and get a lot of research done and come back to San Diego, a very lovely town and nice place, great students, I'll just have that research under my belt now. I'll feel great. And I came back and I was totally depressed. I realized how amazing it was to be in a place where you could really get a lot of research done, where you had a collection five feet away from you, where you had one of the world's greatest natural history libraries, where you had colleagues who were basically the top in their field. It's kind of like saying, "Well, I got to play with Miles Davis," or something. So I just felt really depressed professionally about having to come back. Then I got this letter and they said there's a job opening up, and so I had no hesitation applying for it.

And I was incredibly lucky and they offered me the job. When I was offered the job then I started to think about, well, what am I giving up here? And so that was a difficult decision personally because all my family were on the West Coast. I was a Californian. Would I really survive New York and so forth.

01-00:36:38
Burnett: Well, you seem to have done so and thrived there. And right away, it sounds from your memoirs, you get involved in expeditions to Baja, California in the early eighties and the Andes Mountains in Chile in the late eighties, Yemen, and then in the Gobi Desert. That's a project that you are involved in and setup, and that's to return to the place that Roy Chapman Andrews had been working in, and that was the book that you'd read when you were a kid.

01-00:37:12
Novacek: Yeah, isn't that weird?

01-00:37:12
Burnett: Yeah.

01-00:37:14
Novacek: It is actually, if I step away from it, a very extraordinary kind of story. It's almost contrived. People would say if you were going to write a novel, you'd say, "Well, that's a little contrived. A kid reads a book about this guy and then he becomes this guy." That's like reincarnation. But it happened. It happened. And what's really interesting about when I had to decide to come to New York and I was really—there was some resistance from my former wife about it. She knew it was financially challenging and so forth, so there were a lot of things I had to carefully think about. I was talking to Mark Norell one day. Mark, of course, is the famous dinosaur paleontologist who works with me and is the co-leader of the Gobi expeditions. And we were eating lunch and stuff and I said, "I don't know. I've got to decide if I'm going to go there." He said, "Well, if you don't go there you're going to think the rest of your life what would have happened if you went there." And looking back on all the experiences I've had, it's been an extraordinary thing. So like you said, those different expeditions, culminating in Mongolia, that would have not happened anywhere else.

01-00:38:53
Burnett: Right, right. And I'm going to mispronounce this but the Ukhaa Tolgod discovery.

01-00:38:59
Novacek: Ukhaa Tolgod, yeah.

01-00:39:00
Burnett: Ukhaa Tolgod.

01-00:39:03
Novacek: The Mongolians say Ukhaa Tolgod. You can't [pronounce it the way they do].

01-00:39:07
Burnett: Okay. The richest site for cretaceous fossil vertebrates in the world. Some of these were complete skeletons in their nests. This is an incredible find. Was there a eureka moment when you discovered this stuff or was it a gradual discovery that this was something really important?

01-00:39:31
Novacek: No, it was the former. In the book, as written in the books, it was pure serendipity in a way. The site was not at all dramatic-looking compared to these beautiful canyonlands you might see in Hell Creek and the canyonlands indeed in Mongolia fifty miles to the west of this site. We had passed it three years in a row. But our gas tanker got stuck in the sand and the drivers had to dig it out and I said, "While they're digging it out why don't we just go up and look at those rocks up there." And clearly all the other expeditions, the Russians, the Poles, and the Mongolians over the years had passed this by, too,

because it just didn't look very good. And we were driving up and we started seeing stuff from the car.

01-00:40:27
Burnett: Wow.

01-00:40:29
Novacek: So we started seeing these little white blotches and didn't quite know what they were and then we stopped the Jeep on this little hill, which is now quaintly known as sub-locality First Strike, and right by the front wheels of the Jeep, we had almost ran it over, was a complete beautiful skeleton of a lizard just coiled up like that. Skull, everything, intact. Just beautiful. And we said, "What the heck?" We'd never seen anything like this, even in the famous sites at Flaming Cliffs and elsewhere that Andrews and the Russians collected. Before lunchtime we knew we had found the richest site in the Gobi Desert, within a few hours, because we had already recovered not just the dinosaur skeletons but we had already recovered about fifty skulls of mammals and lizards. Up to that time, over sixty years, there were probably only a dozen mammal skulls known. It was insane. And right before lunch Mark came over and he said, "I just found the best thing I've ever found." I said, "What?" And he took me back there and it was the embryo, the famous embryo that was on the cover of the *Time* magazine, or whatever.

01-00:41:48
Burnett: Yeah, my goodness.

01-00:41:49
Novacek: You've seen the cartoon of Newt Gingrich as the embryo in one of the eggs. And then later that day he found the first nesting dinosaur and then the next day I found the first troodontid and it went on and on like that. It just went on. And we had a great season there this year.

01-00:42:09
Burnett: Right, wow. And thirty years on. Yeah. You've been instrumental in establishing the museum's Center for Biodiversity and Conservation and you also wrote a book called *Terra* in 2007, I think. So can you talk a little bit about how paleontology has informed debates about climate change and how you got turned on to this kind of public-intellectual role? Is that something that was percolating all along or is it something that you really turned to more recently?

01-00:42:49
Novacek: It's interesting. No, it wasn't percolating so much. But when I took this job, not just with the oversight of the scientific programs, collections, and I also have oversight of the exhibition program, like a chief program officer sort of thing. It was not only for management but also to speak for why museums are important and why nature is important. So actually the job, just breaking into the job, moved me into that area. But I realized that it was pulling on a deep-seated passion about this. And then the next challenge was, "Well, there are

all these great people, biologists like Ed Wilson, who is a hero of mine, and he's a dear friend of mine, who use their work, their knowledge of science and their experience in science, to tell people about these issues and to talk about how important they are. And so I had to think about what can I contribute from my own knowledge, my own experience, that's a contribution to this issue that's so important for society. And it seemed evident, very evident, that, well, the fossils are the only thing that tells of what actually happened and certainly extinction events, if we're in the middle of an extinction event now, what people call the sixth extinction, then clearly what I know about previous extinction events has some relevance to this. And also just the reality check that life is ephemeral, that extinction is a way of life, some of the things I said, those are important and sobering lessons for people assuming that the earth is always going to stay what it's like, no matter what they do to plunder it and rape it and remove things from it and kill things in it. Those are powerful lessons. That's why I wrote that book and that's why I've done a lot in this institution in terms of exhibitry and so forth and rolling those lessons out. The book was a real pain to write because it was a more audacious undertaking than the first two books.

01-00:45:30
Burnett: I've asked this of a number of folks. Is there a kind of significance to paleontological work that goes far beyond? There's the element of fieldwork, going out into these rural locations. And you mentioned how beautiful some of these places were. And also your daily work is deep time; your daily work involves thinking about this radical change over very, very long periods of time. Is that something that inspires you? It's beyond just the scientific; it's into something else.

01-00:46:09
Novacek: Absolutely. Being so involved in music now, I still have what I think are strong aesthetics or artistic feelings about things that makes the passion, the love of nature, those things are important to me. And so being in a desert, being out there, thinking about deep time, those things are all beautiful. Those are all things of beauty that I respond to and I think others do, too. There's a communion of spirit when it comes to those things. And those things are very important. There's no question. Nature, the sense of nature, and something I said here has been used as a byline on all the educational programs in the museum. Years ago somebody asked me, "what is your objective in trying to bring all these schoolchildren into the museum or show our visitors these exhibits." I said, "You want to make people first fall in love with nature." That's a big part of it.

01-00:47:38
Burnett: Right. Dr. Novacek, thank you so much for taking the time to sit and talk with us and I wish you all the best with all of the work that you're doing.

01-00:47:46
Novacek: Thanks very much. Now, you didn't ask me many questions about Bill.

01-00:47:51
Burnett: Well, the project, it's about Bill and it's about UCMP, but it's about students who worked with Bill and the work that they did in their careers. Is there a word you'd like to say about—

01-00:48:05
Novacek: Yeah. I'd like to. Maybe it'd just be worth it to extend my gratitude to Bill for his mentorship. He had very high standards. He was very serious, he is a very serious scientist. He has great depths of knowledge. But he's also essentially a very kind person. That always helps.

01-00:48:41
Burnett: Yeah, absolutely. Absolutely.

01-00:48:46
Burnett: Well, thank you very much.

01-00:48:48
Novacek: Oh, nice talking to you.

[End of Interview]

Oral History Center
The Bancroft Library

University of California
Berkeley, California

The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology:

Annalisa Berta

Interviews conducted by
Paul Burnett
in 2015

Since 1954 the Oral History Center of the Bancroft Library, formerly the Regional Oral History Office, has been interviewing leading participants in or well-placed witnesses to major events in the development of Northern California, the West, and the nation. Oral History is a method of collecting historical information through tape-recorded interviews between a narrator with firsthand knowledge of historically significant events and a well-informed interviewer, with the goal of preserving substantive additions to the historical record. The tape recording is transcribed, lightly edited for continuity and clarity, and reviewed by the interviewee. The corrected manuscript is bound with photographs and illustrative materials and placed in The Bancroft Library at the University of California, Berkeley, and in other research collections for scholarly use. Because it is primary material, oral history is not intended to present the final, verified, or complete narrative of events. It is a spoken account, offered by the interviewee in response to questioning, and as such it is reflective, partisan, deeply involved, and irreplaceable.

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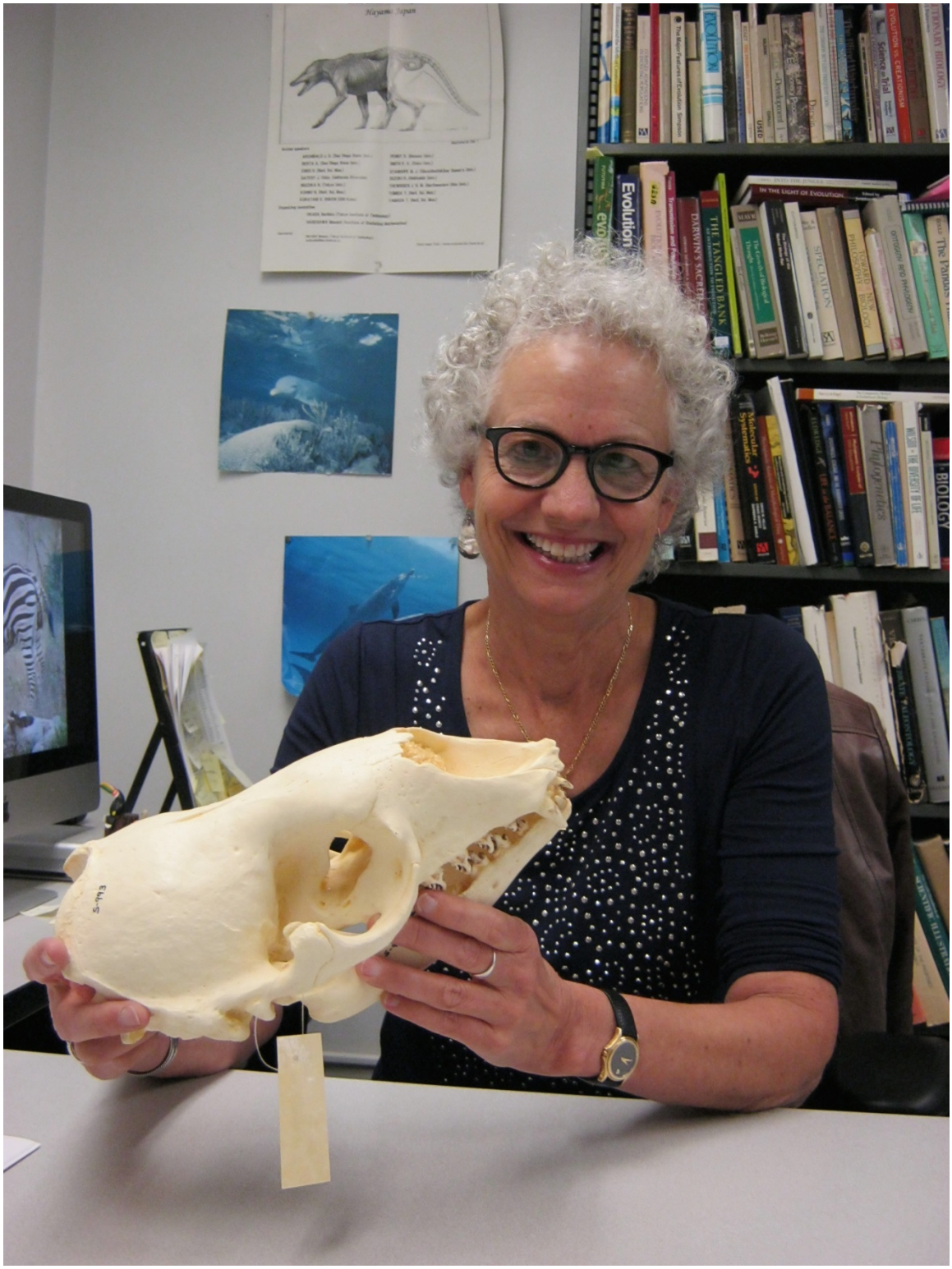
It is recommended that this oral history be cited as follows:

Annalisa Berta "Annalisa Berta: The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology" conducted by Paul Burnett interviewer in 2015, Oral History Center of the Bancroft Library, The Bancroft Library, University of California, Berkeley, 2017.



Annalisa Berta and a crabeater seal, 2015

Annalisa Berta is a Professor in the Department of Biology and Coordinator, Joint Doctoral Program in Evolutionary Biology at San Diego State University. She received her PhD from UC Berkeley in 1979.

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Interview Date: April 2, 2015

Audio File 1

Interview #1 April 3, 2015
Begin Audio File 1 berta_annalisa_01_04-03-15_stereo.mp3

01-00:00:00
Burnett: This is Paul Burnett interviewing Dr. Annalisa Berta for the Bill Clemens UCMP Oral History Project. It's April 3, 2015 and this is audio file one. So Dr. Berta, can you tell me a little bit about your background, where you're from and where you grew up.

01-00:00:25
Berta: Okay. I was born in Bethesda in Maryland. My father was in the Navy so we moved around a fair amount in my younger years. And then we moved to Portland because my mother's family was from there, Portland, Oregon. After a few years we moved to Seattle, Washington where I attended middle school and high school and as an undergraduate I was at the University of Washington. And then once I completed my B.A., I went to graduate school at Berkeley.

01-00:00:55
Burnett: And in your undergraduate years what did you major in?

01-00:00:58
Berta: I majored in anthropology and I also took a lot of geology courses. I didn't exactly have a double major but I had a number of courses in geology. And if I can just expand on that, my paleo interest a little bit. Actually it was in middle school and my teacher at the time—I took a geology class—and the teacher told the whole class about a place in eastern Oregon, a camp, a summer camp called Camp Hancock and that you could go there and be totally immersed in science for a couple of weeks. And it was a geology camp, an anthropology, astronomy, all kinds of neat things going on. So I went back home and I told my parents that I really, really wanted to do that, to go to that camp. And they said, "Sure, okay. You can go for a couple of weeks." And so I went. Then I was a camper for like four years in a row. I loved it, I had a great time. I participated in the anthropology dig and the paleontology dig. And the paleo—

01-00:02:03
Burnett: They had digs—

01-00:02:05
Berta: Yes.

01-00:02:05
Burnett: —as part of this camp?

01-00:02:06
Berta: Yeah, yeah. Where you actually worked. For the paleontology dig we worked on the Clarno (Eocene) quarry. This was interesting. The guys that were running it were PhD students at Berkeley. I just loved the idea of being able to dig and find these fossils that no one had seen for forty million years. I was

just totally taken with it. And I'd go back home and tell my parents how much fun this was. And I remember asking the guys that were running the dig, Bruce Hanson, who's still alive, he was a student at Berkeley, and Ron Wolff, who was also a student at the time. He's since died. But I said, "I am really interested in fossil vertebrates. Where can I go to learn about this?" And they said, "Well, you need to get a bachelor's degree first." And I was already partly into the program in geology and anthro. Then they said, "You really need to go to Berkeley." And so once I got my degree, then I applied to Berkeley.

01-00:03:16
Burnett: And what years were these four years in camp? What ages were you?

01-00:03:19
Berta: Oh, these were in the sixties. It was from when I was maybe thirteen to seventeen. I was a camper throughout high school and then I think maybe my senior year of high school they invited me to run the paleontology research team, which I did. I could influence the kids about what we were learning and why this was really important. That was where I decided this is what I want to do. This is my career. I want to be a professor and I want to do this kind of work, study the animals.

01-00:03:58
Burnett: And you got a little bit of the teaching bug, as well, at that time?

01-00:04:02
Berta: Right.

01-00:04:02
Burnett: Yeah. And so I assume that when you did undergraduate anthro, it was physical anthropology?

01-00:04:09
Berta: Yes.

01-00:04:09
Burnett: It was human origins.

01-00:04:09
Berta: Well, it was physical anthro and archeology. But for the archeology part I was really only interested in the fauna part of it, the animals that lived with the humans. I was interested in how did these animals and humans come to live together, what was kind of the evolution behind this and they were more interested in, well, how many—let's say they were looking at a shell midden, a bunch of clam shells, and they wanted to figure out, well, how many humans could have subsisted. And I was like, "Ugh, not interested in that kind of question. I want to ask about the animals." So that's kind of where I sort of recognized that anthropology wasn't really the way I was going to go. When I came to Berkeley I was a little bit interested in human origins and at that time, I remember in Bill's vertebrate paleo class, China had just opened up. And we

had a couple of kids in the class that were learning Chinese so that they could go and work there. I'm interested in human origins but once I started looking at some of the possible projects it was just too competitive. I just could see that everybody was interested in this. There were so many other areas of research that interested me that it seemed like, well, why should I spend my time working in an area where there's already a whole lot of people. And I just didn't want to be part of that competition.

01-00:05:45
Burnett: What you said earlier, it seems that it was a little too literally anthropocentric, right?

01-00:05:51
Berta: Yes, exactly. Exactly.

01-00:05:53
Burnett: There's more to life than humans.

01-00:05:53
Berta: Yeah. The human thing wasn't that interesting. Although much later, when I came to San Diego State to teach, I was really hired because I had an anatomy background and I was hired specifically to teach human anatomy. So the anthropology interests and background kind of came up again in my career, which I thought kind of interesting.

01-00:06:16
Burnett: And so this is something that came up in another interview, that during this time paleontologists find work as professors teaching anatomy or post-docs teaching anatomy. I don't know if that was something that was there before, if paleontology was just its own thing. Perhaps because the nature of paleontology changes during this period, this becomes more important. I don't know.

01-00:06:48
Berta: I think it's a combination of things. But in my case, having that anthro background, and already had physical anthro, human paleontology. And at Berkeley I took primate evolution. So I had that background and so it was perhaps maybe a segue into someone with a background in human anatomy to be able to teach those kinds of courses.

01-00:07:11
Burnett: When you were at University of Washington was it clear to you that you wanted to be at Berkeley or were there other places that you were considering at the time?

01-00:07:24
Berta: No. Actually, I think I might have applied to some other schools. I don't really recall. But I really, really wanted to come to Berkeley because that's where these students that were in charge of the paleontology dig, that's where they had said, "If you're really set on vertebrate paleo." Of course there is no

longer a paleontology department, so I have this extinct degree. But I was there at the time.

But while I was at University of Washington I also was working with other paleontologists. Micropaleo is how I got interested in it. I started looking at forams and took a class in micropaleontology. But I wasn't so interested in the kinds of questions that they were interested in. They were more interested in using the fossils to date the rocks. And actually I had a job for a brief summer working for an oil company and I just thought, "Ugh, that's all they're interested. They don't care about the evolution of the animal. They just care about what's the age of the rocks." That wasn't something I was interested in so I didn't follow-up with that. But I could see more of the questions about the animals themselves, the evolution of the animal themselves in invertebrate paleontology. And, of course, you could ask those kinds of questions in no matter what area of paleontology you're in. But it just was the direction that I saw myself heading and perhaps the experience and the science in this Camp Hancock, working at a vertebrate mammal quarry. I think all of that played into where I finally ended up.

01-00:09:13
Burnett: In terms of the influence or the kind of demand for certain kinds of knowledge from the oil industry, for example, does that create distortions in the weighting of research programs or areas of research that people explore? Is there more support, for example for—

01-00:09:34
Berta: Of course I can say this now looking back on it. It's all a matter of timing. At the time that I was going through there was a big boom in oil companies and they were hiring. They were hiring a lot of paleontologists.

01-00:09:46
Burnett: Exploration, yeah.

01-00:09:48
Berta: Yeah, there was a lot of exploration geology going on, petroleum geology. So it depends on what years you're looking at. That's kind of ebbed and flowed. Sometimes it's done really well, other times they're not so interested in it. And BP back then had a very strong presence in geology departments, which today it's really more paleobiology. The example is here at Berkeley, integrative biology. Paleontology is really part of that program. So it's gone more that way. There are still paleontologists that are in geology departments.

The early days of going to meetings, when I was a graduate student here, geology was front and center and it was dating the rocks. So it was a lot of K/Ar dating. That's what you would go and hear talks on. And now the folks, the geologists that are interested in paleo are doing the isotope studies, the diet kinds of studies. So it's diet and paleontology but involving geology.

01-00:11:01
Burnett: I want to try and get as much in as we can in this time. But there are so many tremendous shifts happening during the time that you're in graduate school in science, in the world. So let's continue to talk about the shift to Berkeley. So you apply and you get in.

01-00:11:25
Berta: I get in.

01-00:11:27
Burnett: And you arrive there.

01-00:11:29
Berta: And Bill, before I even arrived, he sends me a letter, he offers me a job. I still actually have the letter. And the salary, which was like $3.72, I think, an hour. The job he has—he works on Mesozoic mammals. Material from Montana, where he did research for many, many years, he had screen-washed this matrix, rock matrix, and there are tiny teeth and bones in the matrix, and that's my job, is going to be to sit down under a microscope. I was very experienced having done all this micropaleontology work. And so I had a little paintbrush and a little thing of water and I would pick up the gar scales and teeth and all of that and essentially sort, do the micro-sorting for him.

01-00:12:27
Burnett: And the matrix is a grid on the—

01-00:12:28
Berta: It's on a grid. I think early on it wasn't. But just to make sure that we were looking at the material in a systematic way, yeah, is why we had the grid there. So he offered me the job. I came in the summer. I did not do a master's degree. So I had some research background but not a lot. But I was busy taking classes and hanging out with the other grad students. So it took a while before I developed a project.

01-00:13:01
Burnett: And what began to strike you? Did you shift from interests? Well, you said it took you a while. But—

01-00:13:08
Berta: Bill never directly said this to me but it became common knowledge from me talking to other graduate students that Bill did not take women in the field. And I knew that he had this very active field program in Montana. I never had a conversation with him about it. But from talking to my mates they just said, "Well, he's not going to take you in the field." And it's true. There were other female graduate students but nobody was going in the field with him. Now, many years later, Nancy Simmons, who I think you're going to interview a little bit later, she came at least eight years after me, maybe ten years, and I said, "Well, Nancy, how is it that you went in the field with Bill?" I think I was his first grad student that actually had a field project for a dissertation. She goes, "Oh, Annalisa, I agreed to cook. To be the cook." And I said, "Well,

there wasn't any way I was going to do that." I wasn't that crazy about cooking.

So what happened was there was a grad student in my office by the name of Larry Marshall, who is no longer in the field, but he was really very energetic and kind of a go-getter. And at that time he was doing some research in South America. He said, "There's so much to do there because there's a lot of material, fossil material in collections. I'm going to go down there because I'm doing this radiometric dating of these rocks to kind of figure out where these sequences fit." He goes, "Why don't you just write a grant and come down there and work on material there? There's so much that you could really do and you could really get in on the ground floor because there's so little known." And at that time I was exploring possible projects with Bill that involved carnivores. I was interested in carnivores.

So the thing about Bill was he was always really good about making sure that his students had guidance. And because I wasn't working on Mesozoic mammals he just made sure that one of his colleagues, Dick Tedford, who was at the American Museum, was kind of shadowing me. And so that's one of the things that I do remember very well about Bill. Is that even though my kind of research was not close to what he was doing, he was a great advisor in finding me someone that I could work with.

And then I got money along with Larry Marshall to go to South America and I spent three months and collected a lot of data. And I ended up doing my dissertation on—so I wasn't doing field work, of course. I was doing work on material in collections. And so that became kind of my lab, as it were. And I was successful at getting money. And that meant that my research was not going to be finding, discovering the fossils and doing the paleontology at that point but I was going to work with material in collections.

At that time at Berkeley another one of our fellow graduate students had gone to the American Museum and learned about this new way of doing phylogenetics. So looking at the evolutionary relationships of fossils and modern taxa, using a very different kind of approach and looking at specific morphological characters and then coming up with their evolutionary relationships based on—well, let's see. What do I want to say? Based on, oh, making comparisons but doing it in a more rigorous way. And at that time we were sort of looking at the characters and drawing the trees that made the most sense in terms of aligning the studied taxa with these characters. And, again, we're doing this all by hand. Now the field is all computerized. Because, of course, to make sure that you have the best possible solution to that set of data you can't do it by hand because there's hundreds and thousands of characters.

01-00:17:51
Burnett: Right, right. This is the rise of modern cladistics basically in the—

01-00:17:58
Berta: Yes, absolutely. And it was Mike Novacek. You probably have heard his name because he was a student of Bill's, as well, who's now at the American Museum. So he was the other grad student in the lab and he kind of brought it to all of us grad students and we were like, "Wow, this is like such a cool way." Because before then there wasn't really a rigorous method for analyzing characters and determining which characters were going to be significant ones. And it all has to do with making sure the characters within your in-group are what we call shared derived characters. You do what we call polarizing the characters. You compare them to an out group that's closely related but outside of your group in order to figure out what are the characters that unite the members that you're trying to look at. So that was kind of my thing and I really got into it. My early work was almost entirely systematics.

It changed. I started with carnivores, then I went and did a post-doc at Florida State Museum and I had a chance to go back to South America and do a little bit more work there, and then I got the job at San Diego State. And once I was I switched interests. So it was another serendipitous sort of situation. And I'll just explain that briefly if that's okay.

01-00:19:21
Burnett: Yes.

01-00:19:22
Berta: Or you want me to go back?

01-00:19:22
Burnett: Yes. I do want to go back to a couple of things. One is it occurred to me that you had already been in the field when you were doing these student summer digs before coming to Berkeley.

01-00:19:43
Berta: Yes.

01-00:19:43
Burnett: What was your reaction when you heard or learned, it was rumored, that women are not in the field or—

01-00:19:52
Berta: Well, at the time, I have to say, there wasn't much I could do. Bill was a very nice guy and I didn't see that there was any—frankly, I wasn't that interested in going to Montana. What he was interested in wasn't something that I was that interested in anyway. So I really didn't. I kind of just said, "Okay, well, I'm going to do something else." I'll never know because I never pushed it. And so that's what I just want to say. It was just my understanding. And there were no other women around that went in the field. So I really don't think he did take women in the field.

01-00:20:39
Burnett: Yeah. And it wasn't set up. So it's a question of when it's a completely gender specific project out in the field it's not set-up for a co-ed arrangement. And that it—

01-00:20:50
Berta: That was all at a time when it was a question about how the situation would actually play out.

01-00:21:01
Burnett: Right, right, right. Yeah. And I guess the other question is about when you mentioned doing the cladistics by hand. Did it lend itself in particular to quantification?

01-00:21:17
Berta: Yes. Yes.

01-00:21:19
Burnett: Because Stephen Gould is doing this kind of stuff at Harvard in the early seventies. So this is something that is—

01-00:21:24
Berta: Yeah. And then very shortly afterwards computer programs became available. Because it was very clear that you were not going to be able to do much with just a handful of characters and taxa, was there ever a chance that you might come at the right solution, to the right tree. It just wasn't going to work. And so the methods have just gone gangbusters since then in this area.

01-00:21:53
Burnett: Yeah. Do you remember when people first started using computers to do quantitative analyses?

01-00:21:59
Berta: Very shortly after that. But I had already—

01-00:22:00
Burnett: Yeah, so mid-seventies.

01-00:22:02
Berta: A little bit later than that. Because I was at Berkeley from '74 to '79. So just a little bit after I left when that really hit. Yeah.

01-00:22:13
Burnett: Okay. So you did your doctorate on the quaternary evolution and biogeography of—

01-00:22:25
Berta: Quaternary evolution and biogeography of—

01-00:22:26
Burnett: Quaternary evolution, yeah.

01-00:22:28
Berta: —of large South American canids.

01-00:22:32
Burnett: And those were the ancestors of kind of dogs, coyotes?

01-00:22:35
Berta: Right. Well, there were many more species of dog and wolf-like taxa than there are at present and many of them, during the last million years or so, came from South America up into North America. So it was kind of an interesting evolution. The Panama Canal, that whole thing. It was interesting. The faunal dynamics of the interchange was an important area of research at that time.

01-00:22:03
Burnett: Right. The biogeography of it, yeah.

01-00:23:04
Berta: The biogeography of the two.

01-00:23:06
Burnett: Moving into a new area and speciation.

01-00:23:08
Berta: Right. So the biogeography was part of it. But my work was mostly looking at these different fossil specimens and working out what were their evolutionary histories and then the biogeography was kind of a second interest. Yeah.

01-00:23:24
Burnett: Okay. Great. So by all means now we can go back to what you were talking about, the post-doc.

01-00:23:28
Berta: So I was at Florida working on South America canids but I realized that in order for me to continue to work down there I was going to have to get grant support to go there. I didn't really know how I was going to get those opportunities. And so I started sort of thinking, "Hmm, maybe this isn't a good area of research. If I'm going to get a job in the States they're going to probably want me to do research on material here. It might be more difficult to go out of the country." That's what I was thinking at the time. So I went to the Smithsonian to look at some fossil material and I ran into a curator there, Clayton Ray who, at the time, he had the whole skeleton of a pinniped, a seal, on the corner of his desk. It had been found a number of years before that. And he was kind of an expert in marine mammals, and seals in particular. He said, "Are you interested in working on this?" And I was. I think he wanted my take on it because the common ancestor of pinnipeds was known to be a terrestrial carnivore. Not exactly a dog but a bear or something like that. A raccoon. A weasel. Something in there. So he wanted somebody to study the specimen. He goes, "Hey, are you interested in it?" I'm like, "Oh, my God." This was great. I was already at this time at San Diego State.

While at San Diego State, the other thing that kind of intervened is that David Archibald, who I know you've spoken with, a colleague of mine at Berkeley a few years ahead of me, but he was already there teaching mammalogy. Now, San Diego State at that time, well, the biology department is not too large but it's small enough so that there wasn't any way that they could afford to have two paleontologists there unless they did quite different things. So David continued and expanded his work. He always has worked very closely with Bill on mammalian biostratigraphy and evolution, that sort of thing. And so that's the time when I kind of looked around, and I was already going to study this pinniped skeleton. This was now the middle to late eighties. And then I went, "Hey, wait a second. If I work on marine mammals, that's really different than what Dave does." I was teaching human anatomy but then I developed a course on marine mammals and the rest is kind of history. Then they realized, "Hey, we need two people that are mammalogists because they do different things." So that was my ticket to staying there. That and I could teach human anatomy.

01-00:26:24
Burnett: So in career paths it's not just scientific curiosity, it's also the—

01-00:26:31
Berta: Had to be a little bit strategic about it. If I was going to stay there I could see that I needed to do something different. I had to be able to offer something different in terms of research. It wasn't a big UC. It was a small Cal State school. We've always had a tradition of doing research. But still, you don't have a luxury of hiring two paleontologists working in very closely related areas unless you are offering different classes and your research is focused in different directions.

01-00:27:06
Burnett: The question of this phocine ancestor was really interesting to you, as well, right?

01-00:27:12
Berta: Yeah, yeah. It was. It was. It was.

01-00:27:16
Burnett: And it always has—

01-00:27:16
Berta: Well, also, I could see very early on that if I really wanted to make a name for myself, it was better to go into an area where there were relatively few people working. I just wasn't into the competition. There was just so much to do out there that I kind of said this is going to be a better choice. And so I continued to work on marine mammals. Even now I don't really do so much paleontology anymore. And part of that's because I'm in a biology department. So even though I had all these great classes that Bill taught and I had a really strong background in paleontology, I go to a CSU, a Cal State University, they don't want me to teach vertebrate paleontology. They want me to teach, well,

human anatomy, but then this marine mammal became an interest. San Diego, close to the water, on the water really. It's captive. They're there. There's Sea World there, there's all kinds of opportunities. So then I kind of change a little bit. My interest is more evolutionary biology of marine mammals.

01-00:28:28
Burnett: It's so interesting that it was ignored by the life sciences community or by the paleontological community because the seals were a really key organism for Lamarck and these other natural historians from the late eighteenth century because they speculated about its—

01-00:28:49
Berta: Vertebrate paleontology here at Berkeley, there were some scientists here that did describe some of the fossil marine mammals. Well, Larry Barnes, who's a colleague of mine who was another Berkeley student and had a long career at LA County Museum working in marine mammal paleontology. But there were relatively few of us, I guess is the point.

01-00:29:18
Burnett: Right, right. And so you dove into this area and conducted research. Can you talk a little bit about the research questions that you had about it?

01-00:29:34
Berta: Well, and I will also say that early on I did all the work myself. It wasn't that I couldn't teach students how to do comparative biology but I don't know, I guess I was thinking it was just such specialized knowledge. And we don't offer a PhD. We only recently did in evolutionary biology and it's a joint PhD with UC Riverside. With few exceptions we can't have standalone PhDs, and that's just a separation between the UCs and the Cal State schools. But I'm not sure where I was going with that. Let's see. So yeah. The kinds of research questions were very basic, systematic relationships. I finished describing that skeleton, the pinniped skeleton, the seal skeleton, and then there was other material at Smithsonian. I got a National Science Foundation grant, a couple of them, to work on that material. And so I did a lot of, I would say, alpha level taxonomy, figuring out exactly what the fossil animals were, fossil seals, how they were related to the modern seals. And then I started involving students more into the research and then became more interested in—

Well, the students I was getting, too, they didn't have geology backgrounds. I was in a biology department. So anatomy. I taught anatomy, I taught the marine mammals class. I always had an interest in it. I kind of had to retool myself a bit. I had the opportunity to dissect specimens at San Diego and so that's what I've done more recently with students. And so it's been kind of a combination. You figure out the evolutionary relationships. I was working on seals, then I started working on whales, and then you can ask really interesting questions. Like, well, okay, now that we know how they're related what can we say about the evolution of feeding or evolution of locomotion. And that's

kind of been my thing, is to ask what I think are interesting, I should say, biological questions.

01-00:31:50
Burnett: So by being constrained by institutional and teaching obligations, it exposes you to areas that you might not have thought about before.

01-00:32:03
Berta: Yeah. Actually, I would say in these cases these constraints were very good because, since I wasn't going to be teaching vertebrate paleontology, I had to kind of make use of what was there. And we're on the water, there's lots of other scientists at Scripps and UCSD. The Navy had a big program. So I became kind of involved with the marine mammal community. And, of course, that's an easy sell to students. Although many of them, and then all of them, were interested in behavior. And I said, "Oh, my gosh. You know how many people are doing that? You don't want to do that. You want to come and do what I'm doing where there's not so much competition." So, again, I was taking what I had in the past and trying to suggest projects that they could work on. And in doing so it really opened up what I could do. That's probably been the most incredible thing. From talking to Bill and seeing many of his students, I knew certainly for that period of time, they've gone on and graduated and gone on doing great things. And they're colleagues. I see them at meetings. But having students that do all these different things kind of expands your horizons, right? He was such a great mentor to me that I try to do that and I've been successful at having a large number of students.

01-00:33:29
Burnett: And it sounds like the mentorship is not about replication.

01-00:33:33
Berta: No.

01-00:33:34
Burnett: It is about variation. He wants you to go out and try something new, try something different.

01-00:33:38
Berta: Absolutely. Yeah.

01-00:33:41
Burnett: You mentioned that he pointed you in the direction of Dick Tedford, who was able to give you more direct advice.

01-00:33:51
Berta: Yeah, more hands-on advice because Tedford actually went to grad school here at Berkeley with Bill.

01-00:33:59
Burnett: Right, that's right. Yeah.

01-00:34:01
Berta: But he was at the American Museum and he was an expert in carnivore systematics. So he was really the best person that I could have been connected with.

01-00:34:14
Burnett: And could you talk to Bill about some of these questions even though he wasn't necessarily specialized in that area? Some of the larger questions?

01-00:34:23
Berta: Yeah, he was a great sounding board. Yeah. He knew the field really well, had a very good background. Of course, he was at Berkeley himself. So he was able to sort of ask the right kind of questions and point me in the right direction and suggest, "Here's an interesting something. The way you're looking at this, you might want to think about something else." Or "Here's a grant you could apply for." But in the end it was not easy to get a job and he was always willing to write a recommendation letter.

01-00:34:59
Burnett: Yeah, absolutely.

01-00:35:01
Berta: And he wrote a lot for all of us. There's no question about that.

01-00:35:03
Burnett: Right. Yeah. No, absolutely. And the other graduate students were probably also a resource for you and you were a resource for them?

01-00:35:13
Berta: Yes. Oh, yes. As many of us would say, we learned as much from our fellow graduate students as we did from our PIs, our mentors. Really, having that camaraderie and sharing stories and things. Yeah. We took classes together.

01-00:35:33
Burnett: Yeah. Now, did you have an office space for the graduate students? What was the physical space for sharing ideas?

01-00:35:41
Berta: Yeah. Different faculty members had their graduate students. We had an office. A Clemens graduate student site. Mike Novacek, myself, Larry Marshall, and one other guy whose name I can't remember. I think all of us were Clemens except for maybe the guy wasn't a Clemens student. Maybe he was a Savage student. So Don Savage, of course, had the office next door to this grad student lab and then the office next door to that was Clemens's office.

01-00:36:19
Burnett: Okay. So you bookended? [laughter]

01-00:36:20
Berta: Yeah, yeah.

01-00:36:21
Burnett: Or they bookended your room.

01-00:36:22
Berta: So Clemens's students were sort of separated. I guess maybe now, when I think about it, I don't really know what the arrangements were. But, like I said, David was a few years ahead of me with a different group of grad students. Yeah.

01-00:36:42
Burnett: So moving ahead back to the period at SDSU. You spoke about how students were interested in behavior and you wanted to point them—

01-00:36:57
Berta: Acoustics, yeah.

01-00:36:58
Burnett: Right. And you wanted to point them into exploring these evolutionary relationships. But I imagine that the evolutionary questions were also becoming more involved in ecological questions and so forth. Could you talk a little bit about how that happens in the eighties and nineties? Does that—

01-00:37:21
Berta: That's true to some degree. Well, I wasn't just at that time, though, looking at fossils. Of course I was trying to work out relationships of the modern and fossil representatives, right. And then around that time molecular biology became very strong. Although I could see that I wasn't going to do that myself, I did have some students that were more interested in looking at gene sequence data. So then that's where I would collaborate with different people at San Diego State and then more recently now at Riverside. And so kind of include the fossils. That would be the work that I would do with colleagues and then I would connect with people doing the gene sequence work. But I had some students that really thought that was really interesting and wanted to go that way. But you're right. The integration of evolution and ecology was also in the background.

So the kinds of questions we were working on, I guess in a way some of it now has come full circle because some of the work that I've been doing is more, well, how do these baleen whales feed exactly? I can look at these morphological characters and tell you, well, the common ancestor fed in this way and then they developed these throat plates and then they started bulk feeding, where they would take large aggregates of prey. That's one part of it. But then to actually start looking at diet, that involves ecology. And then starting to even tease apart the anatomy and start to look at, well, how exactly does this baleen trap the food. And so I'm not personally doing this work but some colleagues set-up—engineers more—and they make a little model of a whale and the baleen that would sit on the mouth and then they use different flow regimes and then they trace different beads coming at different flow,

different water speeds and things, and then they try to simulate how it might work. So that's one way to get at it.

The other way is now that there are some very fancy, cameras—critter cams are what they are called. There hasn't yet been a camera that they've put in the mouth of one of these whales but that will be the next step and then they'll really be able to see how it happens. But anyway, they can put these cameras on that can track the animals, the seals, the whales for great distances and record all of this really interesting information about where do they go exactly and how do they dive down? And they have accelerometers that show the pitch and the different angles of the animals when they're under the water.

01-00:40:22
Burnett: Yeah. It sounds like there are enormous challenges, right?

01-00:40:27
Berta: Yes. Logistically very difficult—

01-00:40:29
Burnett: Logistically and in terms of—

01-00:40:33
Berta: The technology.

01-00:40:33
Burnett: In terms of the technology. And when you're thinking of taking data from extant species, genetic data, for example, and fitting that with the fossil record. When I was going through some of your papers, a lot of the paper is about how to fit the data, right?

01-00:41:03
Berta: Yes.

01-00:41:03
Burnett: Because there are these thorny problems you're talking about. There's this genetic record and then there's a limited fossil record.

01-00:41:16
Berta: Well, sometimes there are conflicts in the data is what you're getting at, I think. And so sometimes you might get a hypothesis that the molecular data favors and then a different hypothesis that's based on morphology. Sometimes when it doesn't work out is where it's really more interesting anyway. And so then you have to go, "Okay, well, like why might this be the case?" And there are now different ways, different kinds of analyses that you can apply to the data to kind of tease out what might be causing the conflict in the data. Yeah.

01-00:41:49
Burnett: So you did some research on the evolution of the pinnipeds. Then you really got into whale evolution, the ancestors of whales and the cetaceans. And you had a long run of large, significant, I think, NSF grants from 2002 to 2014.

Can you talk about how you justified to the NSF this research program and what the challenges were and how the research program rolled out over the course of those two grant cycles.

01-00:42:34
Berta: Right. Yeah. We were very successful early on. And actually, the very first grant I had I was actually a lecturer at San Diego State. I didn't go into this but really briefly, I was a lecturer. I wasn't hired as an assistant professor, which was a little difficult. Because I was hired to teach anatomy. So at any time they could have said, "Okay, you're out," because I was really temporary faculty. I was full-time but temporary. I think it shocked everybody because here I was, this temporary faculty member, and I got this quite large at the time NSF grant. And I think that was one of the things that probably helped me stay there, was just because they could say, "Well, wow, if NSF is investing in her she must be doing some research that's worth funding," and that's how that came about. But we were successful because we delivered what we said we would do. And I was able to work with collections that had not been studied well previously. And so most of the work was on fossil material that came from the Smithsonian and then later from the San Diego Museum. And I forged a very good partnership with a colleague, Tom Deméré, who was curator at San Diego Museum. And because of all the development in San Diego, they were always—and it happened to be that the Bay was much larger and there was a lot of housing developments, a lot of marine mammal fossils that were being collected. And so Tom Deméré had the collections and I had the students and we worked out a deal. We collaborated together and were able to just do a lot of amazing things in the nineties and up to now really.

01-00:44:27
Burnett: There's a grant for 2002 to 2007 on baleen feeding. Can you talk about what some of the tentative conclusions were from the bulk of research that came out of that project?

01-00:44:42
Berta: Right. Well, we'd already worked out the evolutionary relationships of well-preserved fossils and modern Mysticeti. And, again, we used a total evidence approach, so we had genetic data as well as morphology data. And then just zeroing in on specific feeding characters and trying to figure out, okay, well, when did this bulk feeding evolve? And we figured out. And we did some work, just basic anatomy and sort of looking at how did the jaw open and close, what are the jaw moving muscles? And I had another student that looked at the hyoid apparatus here, the tongue muscles are connected. So that's obviously going to be really important in feeding, especially in suction feeding. And then we were able to sort of build on the evolutionary story.

01-00:45:38
Burnett: And in one of your papers there's a really eloquent, it's almost an ode to this type of feeding, that this is this pretty unique type of feeding and it's an extremely efficient organism, right?

01-00:45:55
Berta: Right. Right, right.

01-00:45:47
Burnett: And it allows them to just prey on, I think, with one sort of feeding gulp—

01-00:46:04
Berta: Yeah. Gulp feeding, right.

01-00:46:04
Burnett: —the size of a swimming pool.

01-00:46:07
Berta: Yeah, exactly.

01-00:46:09
Burnett: And you get all of the prey in that.

01-00:46:09
Berta: Yeah. So the question really was driving it. And I think NSF is always interested in how are you really going to sell this. And I guess I had a lot of experience at being able to tell other people about my research. So I kind of knew how it had to be sold. At that time there weren't a lot of people doing that. They were interested in it but they didn't have the background to really be able to look at the fossils the way that our group was looking at them. So there were kinds of lots of things that came together at the right time, I guess.

01-00:46:47
Burnett: An important aspect of science and certainly of paleontology is being able to convince a wider audience of the value of the work. So there's a tremendous marketing angle or hustling angle to the scientific research.

01-00:47:01
Berta: Right. And then I also recognize the value of collaborators and so I would get collaborators that did things that I didn't know how to do. And they would do a great job of working with the students really. And so the whole partnership was very successful.

01-00:47:14
Burnett: Could you talk a little bit about the latest one, which was the evolution of the auditory complex of the mysticetes

01-00:47:26
Berta: That was mostly done with a post-doc, Eric Ekdale, a grad student of Tim Rowe (another Clemens PhD) who has so far been unable to find a job in paleontology. He was a graduate student of Tim Rose in Texas. But he drove

that research. He was really kind of a specialist on the ear region. And even more he was a specialist in using a new technology, relatively new, CT scanning. We know a lot about hearing in toothed whales. They echolocate. But we know a lot less about how Mysticetes hear. Not that our group figured out how that happens. But we needed to sort of set the stage for studies. So we did basic descriptive anatomy of the ear regions of fossil and modern mysticete whales.

01-00:48:21
Burnett: And I guess one of your most recent papers is in the *Journal of Vertebrate Paleontology*, "A Reevaluation of the *Pliophoca etrusca*." I hope I'm pronouncing that correctly.

01-00:48:32
Berta: Yeah, that's right. Yeah, that was going back to pinnipeds because I had a couple—you have a student and they come and they want to do a project. Now, again, master students, they might have a little bit of research but not really so much. So it's more of a hands-on thing. You really have to spend more time kind of helping them focus on their project. Some of them would come to me from geology backgrounds and I would put them doing different kinds of projects, working more with the fossils, say, than somebody that came with a strong biology background that was interested in anatomy. So you just find the right project for the student and do that. This particular project I was on a Fulbright in Italy for several months. A long time ago when I was there, another maybe ten years before that, I had seen this fossil seal and I had talked to one of the people about who's working on it. And they said, "Well, nobody right now." But then there was a master's student that came along and did some work with it and then that's what I decided to do as my research project. I wanted to go to back to Italy and kind of work on the specimen.

And it turned out monk seals are pretty interesting. One of them has been hunted to extinction, the Caribbean monk seal. So their evolution was an interesting question, since there are two living species, a Hawaiian one and Mediterranean one. Their biogeography was of interest, as well as their evolution, and then I had the opportunity to work with colleagues in Italy and do a very detailed description of the fossil monk seal.

01-00:50:11
Burnett: What's striking about that paper is the marshalling of different kinds of evidence. In that there's incredible statistical analysis but there's also very high-resolution color images of the fossils and filled in the areas of the missing parts. And different alternatives for the biogeographical—

01-00:50:41
Berta: Yeah, the biogeography. Kind of different hypotheses for how they may have gotten where they are today. Yeah.

01-00:50:46
Burnett: Right. It's probably our most recent example in this whole series of the state of the art of paleontology and biogeography. So it's an interesting document in that sense. So just to close off, could you talk a little bit generally about how your training at UCMP shaped your research as a scientist and your career?

01-00:51:21
Berta: Okay. Well, I had a number of courses in paleontology but I also had a number of courses in zoology. And one of the takeaways was just the fabulous library here. Libraries. And also the faculty. There was a sort of historical element to whatever it was you were working on. We would research something and then think about another way to research it. So it was always very clear that you had to sort of follow a path. And I think I learned that from Bill and from some of the other colleagues. That no matter what you did you needed to make sure that you had really sussed out all of the research that had been done up to the point that you were going to take off and do the work. I just had an appreciation for literature, for libraries, for references. And, of course, back in those days there wasn't all this information you could get online. You had to go to the library. All the time involved in doing that, read through it. So it's really changed quite a bit today.

01-00:52:37
Burnett: Right, right. But the key is to be really meticulous about the historiography—

01-00:52:40
Berta: Very meticulous.

01-00:52:42
Burnett: —of the existing literature, the literature review.

01-00:52:45
Berta: Right. So I've written several books. The third edition of *Marine Mammals: Evolutionary Biology* is about to come out in another couple of weeks. That brought together a lot of my training, researching and making sure that—the other thing I always try to present to students is balance. There are two sides to issues. Make sure you present the evidence for both sides of a hypothesis and then evaluate what's the interpretation. And it might change if you get more information. And so it takes them away from thinking like, "Oh, okay. You mean it's not static?" Like sometimes we think classification is that way. But if your classification is built on a phylogenetic tree you read the classification from the tree by the way you've got—we don't use order, family, and all those other classifications any longer. We just indent to show closeness of relationship or nested hierarchies. Others have probably talked about that. That was a big revelation. Yeah. But when I was a student, if I reflect back on it now, there's no way you could imagine where the field would be. It's remarkable really.

01-00:54:12
Burnett: Yeah, yeah. And the different types of evidence that are being used. But also your own career. Did you know that it was going to go in that particular direction?

01-00:54:19
Berta: Never have any idea. No.

01-00:54:23
Burnett: So we are shaped, and in a sense buffeted by these forces.

01-00:54:29
Berta: Yeah, in a way that is just very difficult to predict. You can't really predict. Some of it's serendipity, some of it's strategy.

01-00:54:35
Burnett: Well, it sounds through serendipity and strategy that you have achieved an enormous amount. I want to thank you for taking the time to sit with us.

01-00:54:42
Berta: You're very welcome.

[End of Interview]

Oral History Center
The Bancroft Library

University of California
Berkeley, California

The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology:

Lowell Dingus

Interviews conducted by
Paul Burnett
in 2015

Since 1954 the Oral History Center of the Bancroft Library, formerly the Regional Oral History Office, has been interviewing leading participants in or well-placed witnesses to major events in the development of Northern California, the West, and the nation. Oral History is a method of collecting historical information through tape-recorded interviews between a narrator with firsthand knowledge of historically significant events and a well-informed interviewer, with the goal of preserving substantive additions to the historical record. The tape recording is transcribed, lightly edited for continuity and clarity, and reviewed by the interviewee. The corrected manuscript is bound with photographs and illustrative materials and placed in The Bancroft Library at the University of California, Berkeley, and in other research collections for scholarly use. Because it is primary material, oral history is not intended to present the final, verified, or complete narrative of events. It is a spoken account, offered by the interviewee in response to questioning, and as such it is reflective, partisan, deeply involved, and irreplaceable.

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It is recommended that this oral history be cited as follows:

Lowell Dingus "Lowell Dingus: The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology" conducted by Paul Burnett interviewer in 2015, Oral History Center of the Bancroft Library, The Bancroft Library, University of California, Berkeley, 2017.

Lowell Dingus at Hell Creek Formation

Lowell Dingus is President and CEO of InfoQuest Foundation, has been the director of paleontology exhibits at the American Museum of Natural History and the Yale Peabody Museum, and is the author of several popular books on dinosaurs and the history of paleontology. He received his PhD from UC Berkeley in 1983.

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Audio File 1

Interview #1 May 5, 2015
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01-00:00:00
Burnett: This is Paul Burnett interviewing Dr. Lowell Dingus for the UCMP Bill Clemens Oral History Project. It's May 5, 2015 and we're here at the Bancroft Library at Berkeley, University of California Berkeley. Welcome, Dr. Dingus. Let's start at the beginning. Can you tell me a little bit about where you were born and where you grew up.

01-00:00:27
Dingus: Grew up in southern California in a little city called San Marino, just below Pasadena. Was born at the Hollywood Presbyterian Hospital when my folks were living a bit earlier in Lancaster, California. My dad was a school coach and administrator and so we kind of followed him around wherever he landed as I was growing up as a kid.

01-00:00:56
Burnett: And can you tell me a little about when you first became interested in science? Is it right from the very beginning or did it develop a little later?

01-00:01:06
Dingus: It was pretty early on. My parents had grown up in Kansas and most summers for our vacation we'd drive from Los Angeles back to the Kansas City area. And we'd take a different route almost every year. And one of the fun things was sitting with dad plotting out our route and figuring out the itinerary and the mileage and all of that. One of the summers during this trip we happened to go through Vernal, Utah, and stop at Dinosaur National Monument. It was back when there was just a scant pavilion over the big slanted outcrop with the bones exposed on it. And it was a hellacious day. It was like 105 outside and probably 115 under the pavilion. And my dad and I were basically the only persons in the exhibit area looking at the outcrop. And one of the preparators (as in fossil preparation) was working on one of the bones there, chipping away. And he looked around to see if anyone was there and he saw us. And he said to my dad, "You know, I chipped this piece off and I can't figure out where it fits back on. Here, I'll throw it across and you give it to your son." And so he threw it and dad caught it and put it in my palm and I was hooked.

01-00:02:48
Burnett: Just like that.

01-00:02:50
Dingus: And I was probably seven or eight years old, something like that. And I just never outgrew it.

01-00:02:57
Burnett: Yeah, yeah. That's a common thread. Some scientists come to it later. But it feels like most schoolkids, at a certain point they get exposed to dinosaurs and

that kind of thing. And, as you said, you didn't grow out of it. But you instead matured into paleontology.

01-00:03:18
Dingus: I'm not sure everybody would agree. My family's been waiting fifty years for me to get a real job. But it's just kind of the way it turned out. Dinosaurs were popular for kids back then. You certainly came across them in your schoolwork as a youngster but nothing like it is today. We were nerds back then. I can remember my parents throwing parties and their friends would come up to me and ask me, "Well, what are you going to be? A doctor, lawyer?" And I said, "No, I'm going to be a paleontologist." And they just looked like, "What are you talking about?" And so it didn't have the socially romantic connotation that it does today after the *Jurassic Park* movies and all the popular culture cultural spinoffs that have come since.

01-00:04:21
Burnett: Right, right. So you were an outlier in terms of career ambitions in your community a little bit.

01-00:04:30
Dingus: Seemed to be. Yeah, very much so.

01-00:04:34
Burnett: So that thread continued right on. So into high school you were preparing to become a paleontologist?

01-00:04:41
Dingus: More or less. It wobbled a bit from time to time. But there was nothing else that really arose that seemed serious competition to my mind. I was fortunate in that as I was graduating from high school one of dad's former students in the little community where we'd grown up, and actually one of Bill's students before, was the curator of vertebrate paleontology at the LA County Museum. So my dad was able to get in touch with him and tell him that I was interested in seeing about becoming a paleontologist. So I was able to serve kind of a volunteer internship at the museum over the summer between my high school and my college years. And that pretty much cemented it for me.

01-00:05:48
Burnett: Right, right. And so you went to Riverside, right, for your—

01-00:05:53
Dingus: I started at Redlands.

01-00:05:54
Burnett: Oh, okay.

01-00:05:55
Dingus: Yeah. But it became clear, and I was advised by some of the younger profs there, that if I really wanted to pursue paleontology and vertebrate paleontology in particular, I should transfer to Riverside where there was a

vertebrate paleontologist, Mike Woodburne, who was also a good friend and colleague of Bill's.

01-00:06:22
Burnett: Were you able to stay at home at that time? How far is that from—

01-00:06:28
Dingus: It's sixty, seventy miles out from where I lived in LA. So no, I wasn't commuting. I was staying on the campuses there.

01-00:06:36
Burnett: Okay. And did you go on field expeditions during your undergrad for geology or—

01-00:06:43
Dingus: Absolutely. Mike Woodburne would run lots of weekend field trips out to the Mojave Desert and some of the localities there, basically fossil mammals, places like Barstow and the Cady Mountains and other places that weren't far from Riverside to get to. So both at Riverside and also while I was an intern there at LACM, Dave Whistler, the curator there, worked out in the Mojave also, and so got some chances to see what it was all about, which was really the romantic part for me as a kid just thinking about trying to pursue a career.

01-00:07:35
Burnett: Yeah. Well, many paleontologists talk about the nature of the field practice being an important component in terms of the meaning of the work. And I imagine it was that way for you.

01-00:07:52
Dingus: Absolutely. There are a lot of paleontologists these days that don't go to the field, that spend the great majority, if not all of their time, working in the museum and traveling around to different museums to study specimens in the collections. I was never one of those. From the start my parents used to drive me out into the Mojave Desert. We'd go to rock shows and gem and mineral shows and get those little pamphlets showing where you can go out and there's an (Exxon) (an "X" on the map) here, you're supposed to be able to find crystals or trilobites or whatever. And so a number of times my parents, who were always very supportive of me in terms of what I wanted to do, would throw me in the backseat and we'd beat the hell out of the car on some dirt road out in the Mojave. Whether we found it or not, it was always an adventure and it kind of set the stage of understanding the joys and some of the troubles that you can get into doing fieldwork.

01-00:09:05
Burnett: Right. That sounds like an amazing support from your family. I think a lot of parents, when they think of supporting their kids, it's getting them to do their homework. [laughter] But this is something different. This is a real investment of time and enthusiasm.

01-00:09:21
Dingus: No question. No question. For both my brother and myself, they were always very supportive of what we wanted to do. And their only admonition was if you're going to do it, be the best you can at it. And as long as it was something reasonably legitimate, then they were always supportive. And I can remember coming back from one of these trips out in the Mojave. It had been on a pretty rough road. But the sedan we had as a family car didn't have any clearance for that or anything. So we got back in the driveway and mom went into the house and dad said, "Come here. Let's take a look under the car." And so we both crawled under there and it was like bombs had gone off underneath the vehicle. And so he said, "Well, I'm going to have to take this in and get it fixed." And then he looked over at me and he said, "I don't think mom needs to know about this." [laughter] I was like, "Yeah, okay, Dad. Got your back."

01-00:10:33
Burnett: Right, right. A little bonding experience over—

01-00:10:36
Dingus: Absolutely.

01-00:10:36
Burnett: —the sacrifices needed in order to get this work done.

01-00:10:40
Dingus: Right.

01-00:10:43
Burnett: So UC Berkeley must have been a natural goal, I would think, if you're in California already and you're interested in paleontology. Did you have other choices in mind?

01-00:10:56
Dingus: I really didn't. It's a long time ago so I can't really recall. I don't recall applying to any other places. I may have. But because of the close connection between Mike Woodburne and Bill Clemens, they had discussed my candidacy. And it just so happened that one of Bill's students who had been in charge of helping to run the field project out in the Jordan, Montana area was finishing up and moving on. You've talked to him, Dave Archibald. And so I was kind of the next person in line after Dave to come in and help Bill manage the field project out in the Hell Creek area around Jordan.

01-00:11:59
Burnett: And you've had that field experience. Like you're already—

01-00:12:03
Dingus: I'd had field experience. I'd had field courses. Bill is interested in the geology but he's more interested in the animals.

01-00:12:17
Burnett: Yeah, sure. Sure.

01-00:12:18
Dingus: And that's very natural. You can go into paleontology in either of those two directions. My background had been geologic. And so it was a good fit between us in the sense that I could really focus on tracing the beds around and trying to figure out how old they were and how the localities fit within the sequence with one another and do it at a finer level of resolution than had been tried before. Dave did a lot of work on the fauna out there as well as a lot of mapping in some particular areas. And so he was a great help and I just kind of picked up where he had left off and tried to refine it further.

01-00:13:14
Burnett: Without knowing too much about paleontology you might assume that that kind of work had been going on much earlier or for a long time. But it's really only in the last fifty years that we've gotten a really good handle on when fossils were deposited and what happens to fossils over time and where they can be shifted around and so forth. So that was this really important work that you were doing in a complimentary fashion with what Bill was doing in terms of actually looking at the mammal fossils.

01-00:13:47
Dingus: Yeah. And that was kind of Bill's plan from the start, that I became involved with his project. And it made a lot of sense. He had learned a lot. Paleontologists had been finding fossils out there since around the turn of the century. So there had been a lot of fossil collecting done. But there hadn't been a lot of geologic work done in association with the paleontologic work. There are some coal layers out there and so the USGS and other folks had paid a lot of attention to that. And in general they knew what kind of fossils were coming from where and Bill and Dave's work certainly refined that a lot. And the work that I was doing was basically a continuation of the temporal refinement of how that all fit together.

01-00:14:55
Burnett: Right, right. And so you did a master of science just for the record? Masters of science at Riverside in geology.

01-00:15:05
Dingus: Yes. Yeah. I worked at a locality up on the Warm Springs Reservation in Oregon and collected and described some fossil mammals from up there before I came as a doctoral student here under Bill.

01-00:15:19
Burnett: Yeah. And so you came out in '79, is that right, or '80?

01-00:15:24
Dingus: Yeah. I started in the fall of '78. And the first field season that I was with Bill up in Montana was the summer of '79.

01-00:15:39
Burnett: And in terms of casting about for a dissertation project, I suppose you, more than a lot of other people, you kind of arrive at a particular dramatic moment

in the history of paleontology, right? And I don't want to overstate it. I want you to tell the story. So how did the impact hypothesis fit into your graduate career?

01-00:16:10
Dingus: No pun intended, it had a big impact. Basically I was just beginning my studies at Berkeley when Walter and Luis Alvarez and their colleagues published their studies from Gubbio about this clay layer between the marine Cretaceous and Tertiary or Paleogene sediments above where they had analyzed that little clay layer and found that it contained rich (enriched?) levels of iridium. And so there was a good deal of interest in this. And the tie with Bill's work and my work was that the sections, the layers of sediments that spanned the Cretaceous -Tertiary boundary out in Montana are thought to be amongst the most complete of any terrestrial section. Marine units can be much more complete because there's a more continual silting down of sediment from the water, whereas on land, especially in fluvial or river flood plain deposits like the Hell Creek and the Tullock or the Fort Union represent out in Montana, you have rivers going across that cut through the overbank sediments and can create little unconformities and complicate the stratigraphy a bunch. But generally it was thought that the area out where Bill and I were working had one of the more complete series of paleontological records, both for fossil mammals, for dinosaurs, for fossil plants, pollen of anywhere in the world. And so that's why there [in our field area] the Alvarez hypothesis had such an intersection with our work to see whether it could be discovered, a similar layer, an iridium-rich layer that might represent the fallout of an impact. Might be discovered not only in the more complete marine sections but also in a terrestrial section that recorded the same period in a very different environment.

01-00:18:55
Burnett: And did the Alvarez hypothesis have a kind of public splash before it ever came to be debated amongst expert paleontologists? Or how was it received by the paleontological community that you were involved in?

01-00:19:18
Dingus: Originally it was received very skeptically, I think would be fair to say. But it being not only a serious scientific hypothesis but also a spectacular scientific hypothesis, it was quickly picked up by the press. And so as we began our discussions with the Alvarezes and their colleagues, it all kind of grew together over the years that I was involved in the discussions.

01-00:20:03
Burnett: And there were seminars held. I think Bill hosted I think the first one. Is that right?

01-00:20:12
Dingus: Yeah. We held them in the paleo department in our classroom/seminar room. And we'd hold them once a week. We kind of alternated them talking about

what they had discovered and how they'd gone about analyzing their samples and what they seemed to represent to them, and then, at first, us kind of introducing them to our field area and what the patterns of faunal distribution within the rock layers were. So that it all started out on a very collegial basis and remained so for several weeks. So all of this time the publicity was building in terms of the public's initial awareness of the Alvarez's impact hypothesis in general.

01-00:21:26
Burnett: And I suppose there are a number of issues, there are a number of burdens and one of them might be that it offers a simple explanation, right? It offers a single explanation and that's attractive. Certainly travels well through the press. "Asteroid kills dinosaurs" and that's it.

01-00:21:46
Dingus: Sounds more like a movie than it does a scientific hypothesis and it certainly had a very high appeal with the public. And it was, of course, at a time when we'd landed on the moon, we'd become much more aware of what was out there in our solar system. And you could see impact craters on a lot of the objects within our solar system. And so it was very, very attractive to the general public. And as you say, a seemingly very parsimonious explanation for what had happened to the non-avian dinosaurs, all the dinosaurs that aren't birds.

01-00:22:42
Burnett: Right. But this didn't seem to be entirely consonant with what you and Bill and others had been working on. It wasn't the question you had asked when you looked at the rocks essentially.

01-00:22:55
Dingus: Yeah. Up to that point there had been other explanations and some really hairball theories about what had happened to the dinosaurs, the non-avian ones. Everything from constipation to magnetic changes in the earth. There were just a whole slew of them. Dietary problems. The mammals ate their eggs and they all died. Some of these were testable; some of them weren't testable. And if it's not testable then it's really not a scientific issue that we ought to waste a lot of time with. So this, along with some variation of volcanic hypotheses, as well as hypotheses involving the pullback of the shallow seaways that lapped up onto the continent time and again throughout the Cretaceous, and with the retreat of the seaways, the climate changing quite dramatically. The volcanic hypothesis, the climatic hypothesis, were really the two dominant ones that were being kicked around when the asteroid impact hypothesis was proposed.

01-00:24:37
Burnett: As a geologist graduate student working in a paleontology program with a premier paleontologist faced with this, did that shape your choice of dissertation topic? Did that move you in a different direction or not?

01-00:24:55
Dingus: Well, let me tell you, I knew what a Nobel laureate was. I'd never met one. I'm not sure I'd ever even seen one before I met Luis. And he was a very powerful, very authoritative, very articulate individual. And I think Bill and I, in a sense we were playing the classic role of skeptics. We looked at the research and the records from our field area and were trying to see how we could use those to test the hypothesis of such a cross-cutting final fatal scenario and what would that look like in the rocks and does the geologic and paleontologic record, as we understood it from our field area, seem to conform or does it seem to contradict that kind of a scenario? In a way Bill's work and my work kind of became decoupled in the sense that most of the arguments that Bill was making were from a paleobiological point of view. After a few weeks when Luis and Walter and their colleagues started to become more frustrated with our continued skepticism and Luis started making statements about how he didn't have a lot of time to waste on this and he had other important things to do, so why weren't we just agreeing that what they had found was convincing and just go along with us. And it started to become more and more personal and politically driven rather than driven by the ideal of science and investigation and, "Oh, that's interesting that contradicts—what might we learn from that?"

01-00:27:47
Burnett: Can I ask you what you mean by political?

01-00:27:50
Dingus: Political in the sense that these are powerful people. And that going up against them can have repercussions. And if it becomes personal in addition to that—well, you try to keep that under the surface—it starts to develop an atmosphere of conflict. And so the work becomes not one that's easy and collegial—

01-00:28:34
Burnett: And collaborative in a sense—

01-00:28:36
Dingus: —and collaborative but rather competitive and aggressive. And Bill dealt with it in the ways that he dealt with it. And I dealt with it by trying to figure out a way to ask a different kind of question than "was it the asteroid or was it something else?" Because from my perspective, I didn't really have the data to say whether it was or it wasn't. And so the main thrust of my dissertation really became, "Well, how finally can we tell time? How precisely can we tell time in these rocks that span the Cretaceous-Tertiary boundary?" And the way I did that was they had published their scenario and based on their modeling they thought that the effects of the impact and the killing mechanisms, whether it be short-term cooling from the dust knocking out the sunlight to other kinds of acid rain and so forth, that that would have taken place within a century or so. Well, when I was a student down at Riverside I had done work with a professor that was an advisor of mine down there, Pete Sadler, who had

developed a model of how often sediment gets preserved in stratigraphic sections or sequences of rock units in different environments. And I had taken that and thought, “Wow, that would be interesting,” because there was a whole other controversy going on at the time that Niles Eldredge and Steve Gould had developed about how species generate called punctuated equilibrium. And so I had taken Pete’s model and applied it to some of the rock sequences where supporters of punctuated equilibria thought they had seen big changes in short periods of time. And seen how often we could expect based on a probabilistic model of having every century represented, every millennium represented, every 10,000 years, every one-million-year segment. And so I thought, well, one way I could ask the question within the context of the Cretaceous-Tertiary debate and the asteroid debate differently is, let’s look at some of these rock sequences that people think have the best, the most continuous record. And if this is supposed to have happened within a century, do we really have a chance of precisely recognizing and having preserved in those sequences every century so that we know that, okay, it happened and it’s likely that we have a rock record that’s giving us snapshots of every century. And the likelihood after running the models was very low that you would have that kind of temporal resolution. So that was how my dissertation played into the overall debate and how I felt like I could contribute something rather than simply arguing for or against the extinction and say, “Well, let’s assume an impact occurred,” which in the thirty-five years since has been widely documented that it did. But would we really be able to distinguish it? And that last sample of fossil material, whether it be pollen or microscopic marine organisms or dinosaurs, would we have that in the century before and then in the mammals and the plants that came after the impact, do we have any real chance, a good chance of seeing that last century before and the first century after? And the rock record isn’t built like that, especially in terrestrial sequences on land because of all the erosion that occurs that we can see going on in our landscapes today around the world.

01-00:33:50
Burnett: It was a great response. It’s an epistemological response to the asteroid hypothesis. You’re asking a question, “What can we know based on the evidence that we have as paleontologists?” And I think you wrote at some point that in one of the areas, I think it’s the San Juan Basin, that has the best record, only one of every 200 centuries is represented.

01-00:34:16
Dingus: Well, I’m glad to see that someone’s read that paper. And to be honest with you, I’d forgotten the numbers so I was hoping you weren’t going to ask me, well, what chances were there? Well, I don’t know. I’d have to look back at the paper. But yes. The point was that the impact may have happened. And, in fact, there’s good evidence that it did. But that’s not really the question. The question is what would the effect of the impact be within the timeframe that the killing mechanisms were thought to have happened and can we see that in the record? And my answer was it’s not likely. It’s not impossible. So whether

the impact caused the extinction or not, it may well have, but it's not something that we can test adequately with the record that we have yet.

01-00:35:20
Burnett: Right, right. And so at the very least it engendered new research. It meaning the asteroid hypothesis. And new questions about resolution and this resulted in advances.

01-00:35:42
Dingus: Well, I'm not sure my dissertation really played much of a role in that. But yes, certainly, the impact hypothesis and the succeeding debate has really generated a whole lot of new research interests, research technologies, research questions and from that perspective it's been an incredibly fruitful exercise that in some ways has done more for multidisciplinary study of paleontologists with other scientific colleagues than almost any other single—

01-00:36:39
Burnett: Event or—

01-00:36:40
Dingus: Yeah. Debate or event or hypothesis that I can think of within my lifetime anyway.

01-00:36:48
Burnett: And you also wrote that you were invested in this for, I think you said something like seventeen years. That you were working on these questions. And then you finally laid it to rest I think in order to move on with the book in the nineties. And I think on the other side, the asteroid hypothesis, they found the Chicxulub—I don't know if I pronounced—

01-00:37:12
Dingus: Chicxulub.

01-00:37:13
Burnett: Chicxulub crater in the Yucatan Peninsula that was their "smoking gun". And so there was a resolution in the sense that the paleontologists were right about what they could know given the fossil record and the asteroid-hypothesis defenders were vindicated in terms of the evidence that the impact happened. And then the question is what influence did that have. And that's still being debated to some degree. So this is—

01-00:37:45
Dingus: That's what I understand. Not being part of it anymore and intentionally so, yes. Just yesterday when I was up at the faculty club, there are a few news sites that I look at every day if I can to see what's going on. And I found it very ironic that there was an article on one of the news sites about the impact hypothesis and people were talking about how the big volcanic event of the time, the Deccan Traps over in India, whether it's one of the pulses of volcanic activity related to it that was essentially simultaneous as far as we can tell with the impact, was the impact a trigger for Deccan volcanism in

some way? Well, in some ways we've come a long way. In some ways we're still arguing over the very same thing that we started arguing about thirty-five years ago. So I guess that's just the nature of the problem and the difficulty of dealing with an incomplete geological and paleontological record. And one of the ways I tend to talk about it with people is, look, I was alive when Kennedy was shot. You were alive. We've got movies of it. You can see it. Yet people still argue about what exactly happened.

01-00:39:36
Burnett: Right. What its impact was, right?

01-00:39:38
Dingus: And what its impact was. Who was really responsible? How many people were really involved? So that was when I was in seventh grade. And a lot of us were alive watching that live. You take a somewhat analogous event, an extinction event of a sort, you put it back sixty-six million years, and it's not really surprising that there might be some difference of opinion. If we can't figure out exactly how the Kennedy assassination went off, then why should we expect everyone to agree about an extinction event that happened sixty-six million years ago when nobody was there to watch it?

01-00:40:35
Burnett: Right. Well, one of the things that was not extinguished, thankfully, was your enthusiasm for paleontology and for that kind of research. Can you talk about your career subsequently? You've mentioned that you didn't go into the classically academic path, although there are multiple paths that one can take in this kind of work. Can you talk about the direction that you took next and the work that really inspired you and drove you on?

01-00:41:08
Dingus: Well, Bill was a big part of that. And coming out of the asteroid debate I was really seeking a haven somewhere else. I had decided that I didn't really want to pursue a career that involved that level of stress and politics of science. And one job that came open that Bill alerted me to and gave me a recommendation for was they were beginning a renovation over at the California Academy of Sciences for a hall of evolutionary history. And so I took that. About the time that the planning phase was finished for that, and they were going to go into production, I got a letter from someone at the AMNH, I'm today still not sure who, saying that they were going to start a fossil hall renovation back there. And so I called a couple of people that I knew back there and asked if it was really worth my time to come back and talk to them about it. And they said sure. So those pieces just fell into place kind of serendipitously, that when I was looking for an exit from academia, an off ramp, there were ways to do that. And from the start I really enjoyed that kind of work and those kinds of projects. And doing those kinds of projects allowed me to not only do them and provide resources for the public about what all this research that we do as academics in paleontology is really all about, but also to just have an opportunity to do the kinds of scientific research and go on some field

expeditions as almost more of a hobby than a career. The expeditions to Mongolia, the expeditions to Argentina and stuff, without having to really fight for the money. So it kind of turned out for me personally to blossom into the best of all worlds for me.

01-00:44:00
Burnett: It sounds like an incredible career.

01-00:44:05
Dingus: It's different. It's certainly not what I anticipated when I started out as a student with Bill. But I'm not at all disappointed with the way things have gone.

01-00:44:17
Burnett: I've done some museum work, in fact, to prepare for a celebration of Charles Darwin's birth and the publication of the *Origin of Species*. And it's a different skill and it's a very challenging skill to take very complex scientific research and transform it into something that can be understood by the public, a public that may not have a lot of time to invest. Can you talk a little bit about the challenges of museum work, of collections and public outreach?

01-00:44:55
Dingus: Well, certainly these collections are analogous to libraries of fossils, of rocks for the research that we do. And whereas most visitors to museums recognize the value of the museum as a conduit for that information, they often don't recognize that the real research goes on right in that same museum that they're walking through. And so I think we need to do a better job of letting the public know that. After all, our tax dollars go to the government, thereby to NSF. And if we don't make—and other governmental institutions certainly —and if we don't make the effort as paleontologists, whether you're an academic or a person in exhibitions or education, if we don't make the effort to explain to the public why we go out on these trips, why we have these specimens in our basements, then they have no responsibility to understand why we should be asking them for money. And so to my mind it's really about that. And so I think it's as interesting a challenge as many of the scientific projects that I've worked on. I think it's as interesting a challenge to take something like cladistics or the evolutionary trees and how relationships between different groups of organisms are related to one another, and try to explain that to a public in a physically spatial way as well as in simple language. And if they want to understand that, if they want to make the effort to follow that, fine. If not I'm not offended at all. If they just want to walk through and see the big dinosaur specimens that's fine, too. And read about how long the skeleton is and how many teeth are in the jaw of a tyrannosaur, that's fine. But for those of the visitors who are interested I feel a responsibility to try to explain why we have this stuff and how we're trying to study it and how that might relate to their world and their lives. So I've always found that to be an interesting challenge, to be kind of a translator of sorts for

some of what I've done and a lot of what others have done, others of my colleagues.

01-00:47:57
Burnett: Well, there are several different audiences that you're deeply invested in and one of them is children. You've written award-winning books for children on evolutionary history and is that because of your encounters in the museum? Have you learned that that was a constituency you wanted to grip or were you thinking about your own childhood and your own enthusiasm?

01-00:48:31
Dingus: Yeah. I was going to say it's more of the latter. Gosh, it's hard in a way. Kids are naturally interested in paleontology now because of the whole genre of popular culture that's grown up around the field. But that doesn't really explain what we're trying to do as scientists. That's science fiction. That's not science. And so I think about myself as a kid, what I would have liked to have known more about before I jumped in the pool and decided I wanted to become a paleontologist. I was trying to come up with some examples, some stories, and a style of writing that would help explain to children what science meant to me, how I see science working, how you can come up with any crazy idea but have you found anything in the fossils that you can use to test your idea that you think a *Stegosaurus* was pink? And what the difference is between some of the things you hear in the popular press and some of the things we try to more rigorously test and tease out in our scientific studies. Because I think the public generally doesn't have a great understanding of what science is and I think that's because we as scientists don't do a very good job of explaining what science is to the public. So to some degree much of my writing has been geared toward helping those who are interested to get a little more insight about what science is. Whether they become a paleontologist, whether they become some other kind of scientist, whether they go into something other completely than science. But just so that hopefully it's made some very small contribution to the scientific literacy of our society.

01-00:51:05
Burnett: Another important audience for you is adults but also adults who are interested in the history of science. So you have written about Barnum Brown, you've written about a number of different stories that illustrate the history of science. From your historical perspective, can you talk a little bit about what you see as the most dramatic changes and advancements in paleontology since you started in your work?

01-00:51:46
Dingus: I would have to say it's basically been technologically driven. Whether it's a concept like cladistics or other kinds of phylogenetic systematics for which we can now use computers to help do the analyses, whether it's about instruments that have been developed since I started that can analyze chemistries of rocks and all kinds of things that we had no ability to do back

when I started. It's primarily a technological revolution. Now, we've certainly gotten into a lot of areas for collecting fossils that we weren't in back when I started, areas in China, areas in Mongolia, parts of the old Soviet empire that were not available to western scientists back then. And that's been a big boon and been very fun to be a part of, a small part of. But I think the biggest changes have been the technological advancements that we have to bring to bear upon the data and the specimens that we collect. There's certainly been a lot of intellectual revolutions that go along with those but I think it's the technology that's really made those ideas and enabled us to explore those ideas in so much more of an exhaustive and powerful way.

01-00:53:46
Burnett: Right, right. New levels of resolution and they enable new ways of seeing the data, right? I think that's what you're saying, too.

01-00:53:55
Dingus: Right, right. Because they just open up whole new fields and avenues of inquiry. And it's going to change a lot of what we've done. A lot of stuff that I've said I'm sure is going to be wrong and that's just the way it is. You make a tentative statement based on the data that you collect and the methodologies that you use to analyze it and that makes a statement. And someone comes along later and looks at that and says, "Well, I'm not sure about that." They collect other data and they run their own analyses and come up with a different answer that's probably better than the one that you came up with at the time. That's just part of the process.

01-00:54:54
Burnett: Right, right. All those statements are essential for the process to continue.

01-00:54:57
Dingus: In the end science really isn't about truth. And I think that's something that the public and unfortunately a lot of scientists don't seem to understand. We're not looking for the truth. We're looking for the most reasonable explanation based upon the data we have and the method of analysis that we bring to bear upon the data. So you ask a question, you collect the data, you run your analysis based on some methodology and you get an answer. And you've probably incorporated more data and looked at it in a methodologically more rigorous way than someone did before and that's why you get a better answer. Is it the truth? Who knows? Who knows? But it becomes the standard upon which the next study is going to be done with more data and other—

01-00:56:09
Burnett: Or a different direction.

01-00:56:10
Dingus: Yeah, or whatever. And that just moves everything forward. They have your foundation upon which to build their next analysis. And that's what should be great about science.

01-00:56:26
Burnett: Right, right. Dr. Dingus, I want to thank you for taking the time to sit with us and talk.

01-00:56:30
Dingus: Sure. Absolutely. My pleasure.

[End of Interview]

Oral History Center
The Bancroft Library

University of California
Berkeley, California

The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology:

Marisol Montellano

Interviews conducted by
Paul Burnett
in 2015

Since 1954 the Oral History Center of the Bancroft Library, formerly the Regional Oral History Office, has been interviewing leading participants in or well-placed witnesses to major events in the development of Northern California, the West, and the nation. Oral History is a method of collecting historical information through tape-recorded interviews between a narrator with firsthand knowledge of historically significant events and a well-informed interviewer, with the goal of preserving substantive additions to the historical record. The tape recording is transcribed, lightly edited for continuity and clarity, and reviewed by the interviewee. The corrected manuscript is bound with photographs and illustrative materials and placed in The Bancroft Library at the University of California, Berkeley, and in other research collections for scholarly use. Because it is primary material, oral history is not intended to present the final, verified, or complete narrative of events. It is a spoken account, offered by the interviewee in response to questioning, and as such it is reflective, partisan, deeply involved, and irreplaceable.

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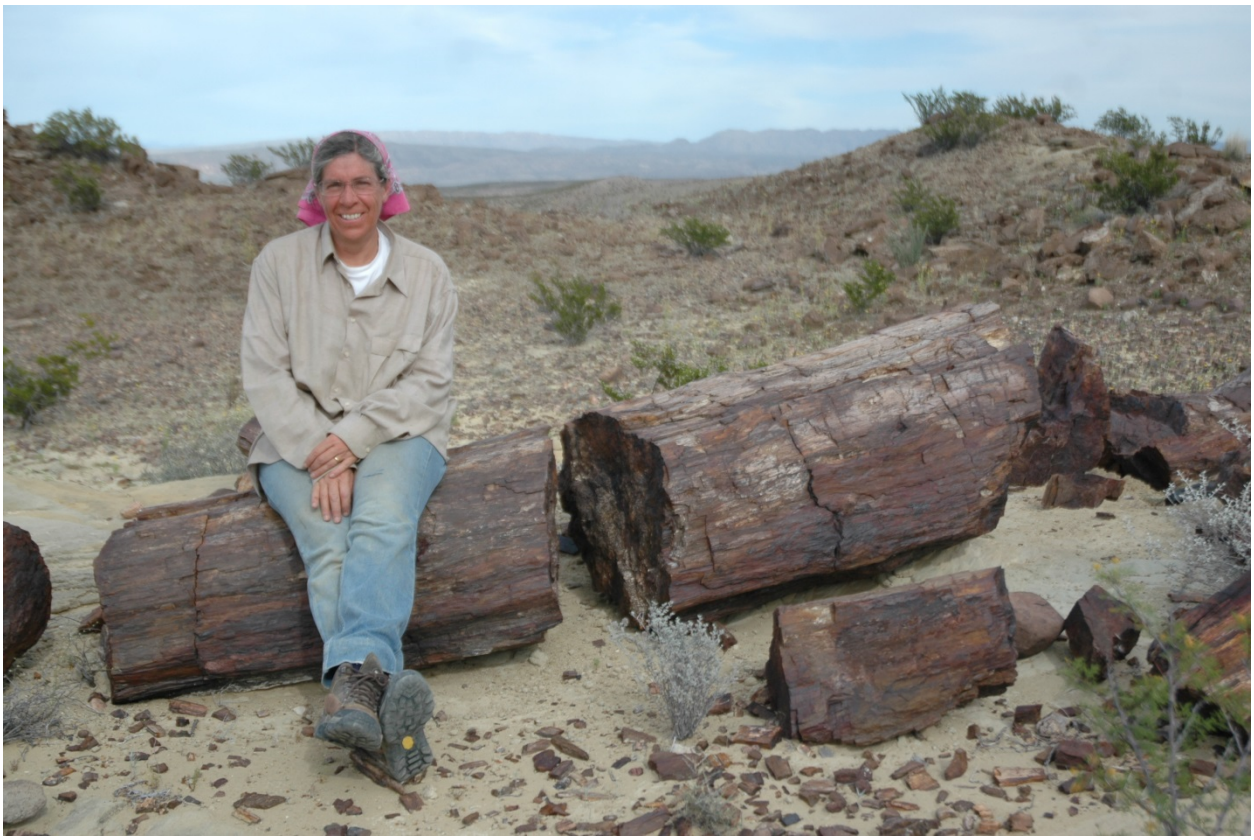
It is recommended that this oral history be cited as follows:

Marisol Montellano "Marisol Montellano: The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology" conducted by Paul Burnett interviewer in 2015, Oral History Center of the Bancroft Library, The Bancroft Library, University of California, Berkeley, 2017.



Marisol Montellano, 2006

Marisol Montellano is Professor, Department of Paleontology at the Universidad Nacional Autónoma de México. She received her PhD from UC Berkeley in 1986.

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01-00:00:07
Burnett: This is Paul Burnett, interviewing Dr. Marisol Montellano. This is file one of the Bill Clemens UCMP Oral History Project—the student interviews. It is February 2, 2015, and we're here in the Bancroft Library. So, Marisol, you are a professor of vertebrate paleontology at UNAM.

01-00:00:41
Montellano: Yes, I'm a researcher at the Institute of *Geologiá* at the UNAM, and also, I'm a professor at the *Facultad de Ciencias*. In Mexico, we have different kind of positions for teachers and for researchers, what we call "researchers." So, mainly, I am a researcher at the Institute of *Geología*, at the department of paleontology.

01-00:01:07
Burnett: You've come down to speak with us today about the history of paleontology, because you've been part of that history, and your work with Dr. Bill Clemens at the University of California, Berkeley in the 1980s. First, I want to ask you a more general question about the interest in science. So, when did your interest in science begin? In particular, paleontology?

01-00:01:40
Montellano: Well, I guess that I was never captured by humanities—arts, and so on. I have been always more scientific-field-related. When I was a kid, I wanted to be an astronaut because Valentina Tereshkova was the first astronaut woman, so I wanted to be an astronaut. Sometimes, I wanted to be more like psychology or biology. That idea came out, I guess, when I was in high school, when I had to apply for the university in Mexico. I applied for biology. While I was in biology, I never liked very much laboratories. I preferred to do fieldwork. In our curriculum in Mexico, we have the course, an obligatory course, of paleontology.

01-00:02:49
Burnett: A required course in paleontology?

01-00:02:50
Montellano: Yes, exactly. So, this professor, Dr. Oscar Carranza was my professor down there. Like I say, "Well it could be—I like a lot of vertebrates." In fact, I started to work in herpetology and ornithology, but I was not really very good in mammalogy either because all the fieldwork is during the night, and I'm not so good. [laughter] I am more a day person!

01-00:03:26
Burnett: So, this is fieldwork in biology so you're looking at living mammals and they're nocturnal?

01-00:03:31
Montellano: All vertebrates, I like them. Fishes, no, but reptiles, I like them very much, but it was difficult to really capture them. The mammals, the work was in the night, no, that was not for me. For fossils, they are there, they don't move, and do work during the day. So, that was one of the reasons I said, "Well, I would like to try to do some of my bachelor's dissertation in paleontology." Then, I talk with Oscar Carranza and he said, "Okay, please go to the field for one field season in Guanajuato, and you can see. If you like it, you can stay. If you don't, at least you have tried." That worked pretty well. In fact, during that field trip, it was Harley Garbani. Harley Garbani was one of the main friends and colleague of Dr. William Clemens. So, he was a very close friend and also, he was a person who shared with Oscar Carranza a lot of field trips. He was very important for both men. Then, I went and I participated in that field trip in Guanajuato.

01-00:05:01
Burnett: Where is Guanajuato?

01-00:05:03
Montellano: It's in the central part of Mexico and it's very important because of the fossils that are coming out from that area. They are helping us to understand the great American biotic interchange during the late Miocene/Pliocene. So, that worked pretty nicely. I remember very much Harley Garbani in those days because he was always a very nice man and he was always encouraging you to continue. I was so tired, it was so hot, I was thinking this is like at two o'clock in the afternoon, when it was so hot, he always said, "This is the time when you can separate women from girls." It was so frustrating because he was always finding the very nice and great things, and me, nothing. But I decided I liked this job, and then, that's the reason that I started vertebrate paleontology. Then, I did my bachelor's degree in vertebrate paleontology with some fossils from Guanajuato with Oscar Carranza as my professor and my advisor. When I was finishing my bachelor's, I continue my master's degree in Mexico, and I continued helping and being part of this project in Guanajuato.

I was trying to decide—I wanted to go for a PhD and then Oscar and Harley told me, "Why you don't go to Berkeley?" I was like, where is Berkeley? [laughter] In Mexico, there was nothing being done with Mesozoic mammals. They said, "Maybe you can start that research in Mexico after you get your PhD," and then that is what really happened. During those times, I used to practice rowing, so in 1981, I came to United States. It was Long Beach, I think, there was this competition of rowing. Then, after the competition, I came to Berkeley—it was in December or something like that—to know the campus and to meet Dr. Clemens. Harley and Oscar made the contact, and also, made me contact Dr. Clemens by letter because during those days, there was not email. It was expensive, also, to make telephone calls. So, then I just appeared and showed up at Berkeley. I liked the campus very much and I met Dr. Clemens. He was so friendly and so warm that I said, "Hm, okay, I like it."

Then, I applied to Berkeley and he accepted me, so that was the reason that I came to Berkeley from ’82 till ’86.

01-00:08:25
Burnett: Can we back up a little bit and talk a bit more about your distaste for laboratory work and your enthusiasm for fieldwork, even though it was frustrating? Why is fieldwork appealing to you, or was it appealing to you, at the time?

01-00:08:44
Montellano: No, it is, it is. I think that I’m more like an outdoor person. I like to be outside. I enjoy the landscapes. I like to listen to all the noises of the living things. I like to see the stars in the night, to see the sunrise, the sunset, that I think that is the best time of day, the sunset, after collecting and prospecting, I enjoyed very much that special moment of the day. In the afternoon, when the sun sets, I think that is a great time. After you are sweat, after you are frustrated or very happy because of the discoveries, it doesn’t matter, and I start to do the cooking and everything, and to chat and to make all the jokes with the rest of the crew, I think that that has something. In the laboratory, maybe because I’m not so abstract in my thoughts, I think that that’s the reason that I don’t like very much laboratory. Laboratory, I mean more like chemical—to clean and prepare fossils, I like it. Sometimes, I’m not so patient, but at the same time, it’s like a puzzle and I like to do puzzles. So, that’s part of everything. I think that field trips, if you have a great team—that I have been, and I have been very lucky to have people who are just participating in my field trips—I have my basic team, one is another student of Oscar Carranza, {René HernándezHernandez?}, and the other is Gerardo Alvarez, that he started also with Rene a myself mi?{inaudible}, working with Oscar in Guanajuato. He’s an administrative in Mexico. He’s our laboratory technician, now. Then, they are part now of my team, and the three of us is like, I can go with them, wherever!

01-00:11:07
Burnett: How many years have you been working with this?

01-00:11:09
Montellano: With them? Since 1978, ’79? Yes, more or less. Then, we know each other so perfectly. It’s like, “Oh, today is not a good day for him.” “Oh, he can understand that joke or he cannot,” or, “Something is worrying him,” or, “Oh, he found something—look how he’s just standing,” and so on. Then, we have been working for so long time together, the three of us, that it’s a perfect team. Now, with all the new students and so on, we are like, I would say, the basic team. The basic package. [laughter]

01-00:12:00
Burnett: At this time in the late seventies, how many women were in the field of paleontology that you recognize? When you went out to digs, how many women were in this?

01-00:12:12
Montellano: Well, still I think that in this field, there is not a lot of women. I don't know. In Mexico, I have been told that I am the first one, the first woman in vertebrate paleontology, because there are some women who have been working with microfossils or invertebrate, plants. With vertebrates, I guess that to do the prospecting and to be strong enough and understand all the difficulties in the field, it's not so easy. Maybe because I was more like physically strong, I don't know, I didn't have any problems. Although, working in Mexico in the field and dealing with men from the field, with *campesinos* and with that kind of person, depending on the part of Mexico, for example, I like much, much better to work in the northern part of Mexico because they treat the women more like, we will say "tête-à-tête," we're like on the same level. In Central Mexico and in the southern part of Mexico, they don't speak to me, although I am the leader of the research or the field team. I don't fight that anymore. Before, when I was twenty years old, twenty-five, I was just like, near to kill them. It was just like, "Why is he not talking to me? Why are you not addressing, why you are not answering to me, why you are answering to the technician?" Now, it's just like, "Okay, I don't care." I think that one gets older and one is like, "That's no reason to fight." That's like that.

01-00:14:20
Burnett: It won't change in that particular area for a while, yeah.

01-00:14:26
Montellano: It was complicated for the people, *campesinos* and so on, to treat me because I was not like their women. They cannot treat me like their women, but at the same time, I was like an outsider, but with another kind of knowledge and with another kind of life, power, sometimes. For them, they treat me sometimes like a teacher. Then, the teachers in the field or in the countryside, they have a special status, then it was easier for them to treat me like a teacher. There were all these things going on. Now, it has changed a little bit in the countryside in Mexico. In the northern part, it always has been more equal. Then, I never have any problem if I speak out, if I ask for things, for directions, that was not a problem.

01-00:15:28
Burnett: That's because in the northern part of Mexico, there's a lot more circulation, cultural circulation or economic circulation of people? Exposure?

01-00:15:37
Montellano: They are different cultures, completely different, maybe because also they don't have so many influences or background of Indians. In the central part of Mexico and in the southern part is when you have the highest percentage of Indians, of all these ethnic groups, that they are more like, we call them *machista* culture, where the women is not worth—. In the north, no, you don't have that influence. Maybe that could be also part of the reason that both of them, they are hardworking women and men. They are more direct, also, in their way of talking. They are more direct. In the central and southern part of

Mexico, you never know if they are really telling you the truth, or it's just like, trying to be a little bit—not to trust so much. [laughter]

01-00:16:46
Burnett: Right. They're trying themselves to figure out who you are and where you fit, and they have to do some adjustments, culturally, to meet you in a common place.

01-00:16:58
Montellano: I am blonde, so sometimes it's like, "You're too blonde to be Mexican," and all these kind of things, I have to deal.

01-00:17:07
Burnett: Right, you're an outsider in many ways, many different levels.

01-00:17:11
Montellano: In the world of vertebrate paleontology, there haven't been so many. I think that now, you can see a lot—at least, a lot of students. I guess that in each country and in each culture, you have all these issues about women being like, not the leaders, but more like to have a position of the chief of the department, but that's like, all over the world. When I came here to Berkeley, we were three or four women in vertebrate paleontology. In that moment, in Mesozoic mammals, where is Dr. Clemens' and now one of my specialties, it was Zofia Kielan-Jaworowska, that was like the great woman in vertebrate paleontology and in Mesozoic mammals. So, there are many more now. Students, I have a lot of women. My students are women, but at the same time, how far they can go, and to have a position like mine, to be a leader or to be the chief of the department of an institution? I don't know.

01-00:18:34
Burnett: As you said, that is a problem across the board, in the sciences.

01-00:18:38
Montellano: All over the world—it's not only in Mexico. I think that is all over the world.

01-00:18:43
Burnett: And in different sciences, too, absolutely. So, we have determined your interest in fieldwork and your enthusiasm for fieldwork. You came to UC Berkeley in 1982 and this is right at a time in a kind of revolution in the life sciences at Berkeley, the development of the Valley Life Sciences Building and integrative biology. There's tons of stuff happening, another kind of revolution happening, in paleontology, too. It's increasing connection to other sciences, whereas before, it wasn't. As much as I understand it, that's what happened. Can you talk about that time? What was that like at Berkeley and what was happening in paleontology?

01-00:19:36
Montellano: Well, when I arrived here in '82, I think that the main issue, at least in the earth sciences building, where we were just like, being part—now, I just visit outside the building of the earth sciences and I'm like, "Oh, that was my

office"—it was very exciting. I have to tell you that the visit has something, touched [me]. I remember that the main issue was about the K-T Boundary. That was what I really lived during those days. In the same building, it was Alvarez and it was Clemens, leading two different positions. I must say that both of them, they were gentlemen—both—Clemens and Alvarez, Walter Alvarez. Luis Alvarez is another person, completely different. Both of them, they were really gentlemen, and they were very polite. They tried to always discuss, on scientific grounds. I think that that was very important, in those days. There were many seminars in Cretaceous-Tertiary Boundary. There were all these field trips organized where a lot of specialists from different fields, like plants, geochemistry. In Montana, we worked together and they were just discussing all these sections, to see the iridium in the stratigraphy and so on, and what has happened during the Cretaceous-Tertiary Boundary. In fact, in those days, there were a lot of students who were just trying to prove or disprove that it was a catastrophic event for the live, or it was a gradual.

There were a lot of students, not only from Berkeley, but also from other universities, that they were doing sedimentology and geochemistry and looking all these sections, that they are complete, and making all these comparison studies. Then, that was, I think, in those days, I think that that was like the main objective, it was like the main topic. In biology, I'm a biologist, so I didn't have any problem having biology in paleontology. My background is in biology. I took several courses in biology here in this building, in the life-science building, some courses in geology, so just like to have an interdisciplinary education. Because I used to have in Mexico, each year we used to go to the field, like, for one month, one month and a half. I had some experience already in the field. So, that was maybe what helped me not to be just a very new one, and "What?" It was like I already had some experience. You are right, during those days, there was all this going on. Also, all about the dating and about the geochemistry. In biology, I think about extinction, that really shook a lot, what's the meaning of extinction?

01-00:23:29
Burnett: Do you mean the quantitative studies, the Stephen Gould kind of stuff, as well?

01-00:23:34
Montellano: Everything! I think that everything because extinction for paleontologists is so common because we talk about extinction all the time, but for biology, extinction, that was a big issue, but how to quantify. At the same time, I think to understand that there are two different kinds of time—it's not the same biological time as geological time. To get that difference in everybody's mind is not easy. I teach biology to students and I teach paleobiology. When I'm talking about, like, one million years is nothing in geological time, but for biology, how many things have originated, got extinct, whatever? That dimension, I think it also helps to understand that they are two different kinds of ideas or timings. That helps, because depending on that, if it's catastrophic,

how can you prove it? If it's gradual, how can you prove it? Then, you have to deal a lot with dating, stratigraphy, sedimentology, taphonomy, so many disciplines that really, before, nobody really paid a lot of attention to. Because dinosaurs have always been an It sort of has been always a magnet to everybody. Dinosaurs are always liked by everybody, all the kids—

01-00:25:23
Burnett: There's enthusiasm for it.

01-00:25:25
Montellano: Always, always, but not only dinosaurs got extinct. If you believe that birds are dinosaurs with feathers, they didn't go extinct. There were all these discussions during that time.

01-00:25:45
Burnett: What seems to be coming in from other sciences into paleontology is this question, not of individual extinction, not individual species, but I guess the K-T Boundary extinction debate is very much about, there's this catastrophic decimation of whole families of species, genera, orders, but some are and some aren't, right? So, there's a question of how extinction works on whole ecosystems? I noticed in your large collection of published papers, as you get closer to the present, to 2015, there's paleo, something called paleo-ecology. So, is that coming into the mix? Are people trying to look at extinction biologically, in terms of groups of organisms that go extinct, and what are the drivers, in essence, of extinction?

01-00:26:52
Montellano: I think that that's so difficult to do. Well, the Cretaceous-Tertiary discussion or controversy, it started a new way of reviewing, studying, analyzing all the extinctions because the largest extinction is not the K-T Boundary—it's the Permian-Triassic that is the mother of extinctions. This one, it has its appeal because of dinosaurs. We must say that. I think that although it's one of the closer to our times, at the same time, it's like you don't have anything that you can compare with the recent times. So, that makes more difficult to understand, which is also, we don't know really all the diversity. Then, if you don't know all the diversity of each ecosystem, then it's so difficult to really understand, okay, we get rid of this, then what's going to happen? You don't know all the connections and all the relationships. More or less in a generalized way, you can say these are the trophic levels, and maybe this has to be like this, but you don't know if there is another opportunistic. Then, there is a lot of things going on, but at the same time, I think that this is the kind of things that are just triggering all these studies now. We can see all these controversies, if some kinds of dinosaurs, they are just like active carnivores or they are the predators or they are only eating these animals. Like, all these issues, how they can handle, how much they have to eat? Those kind of things now, you have like all these positions that are so extreme. I think that that's the good thing about paleontology, that not everything is 100 percent sure.

01-00:29:35
Burnett: It's not settled.

01-00:29:39
Montellano: It's moving each time. If we can see the position of the dinosaurs since they first were discovered until now, it's amazing how the position—only the position—of each of these dinosaurs has been transforming the drawings. It's very interesting.

01-00:29:58
Burnett: Oh, phyletically, you mean, in their position?

01-00:30:01
Montellano: The same animal. For example, one of the largest dinosaurs, where they were just like lizards, so they have to be on four legs, but now, they are just standing. Where is the tail? Is it hanging or they are just like, close to the ground? They are making all these calls. It's different because we are learning much more and we're having all these, I think, tools to develop all these models. But we have to think that they are models only, and you have to keep that in mind.

01-00:30:45
Burnett: In the nineteenth century, there was this tremendous confidence with people like Georges Cuvier. They said he could take one bone, like a foot bone, and reconstruct the entire dinosaur based on other models that he'd put together. There was this perhaps overconfidence? [laughter] I guess what has happened with the avalanche of new information, it's a humbling experience, continually. We're realizing we don't know as much as we thought, but we're learning so much more. We are putting together narratives of the evolution of these creatures. So, there's so much going on at this time, it must have been absolutely electric for you at this time. So, can we dive into a little bit of your specific research that you started? How did you pick a topic? Tell me a little bit about your work with Dr. Clemens.

01-00:31:50
Montellano: When I came here, all my education and all my research was on vertebrate paleontology, but with Cenozoic mammals, with Pliocene/Pleistocene mammals. When I came here, the idea, and also, the reason that my university supports me and I had a scholarship from my university to come over here, is to specialize in Mesozoic mammals. It was only two or three records of Mesozoic mammals from Baja, California, that Jay Lillegraven and Dr. Clemens did, and that was it. It was the only thing that was known of Mesozoic mammals from Mexico. When I came here, that was the idea that I was going to be educated and I was going to try to get some experience on Mesozoic mammals, and to learn about that. For me, it was a challenge because I have never worked so much with microscopes. Mesozoic mammals, they are so small that you have to work all the time with microscopes.

01-00:32:57
Burnett: These are microfossils?

01-00:32:58
Montellano: Yes, they are microfossils. They are very little. What we planned for my PhD was after my first year here, I returned to Mexico, and do some prospecting in some areas in Mexico where some dinosaur has been found, and collect some material to check if we were able to get some Mesozoic mammals. After that field trip, we didn't find anything, so because I had this scholarship, Dr. Clemens suggested that I study the mammals of the Judith River Formation. That was material that was already collected and that was already picked up, so I could just start right away to study the material. Then, I started to work on that material and it's what I did for my PhD, to describe all this material from the Judith River Formation.

01-00:34:12
Burnett: That's in North-Central Montana, is that right? That's different from the Hell Creek, which is Southeastern?

01-00:34:19
Montellano: Oh, yes. This is older than Hell Creek. In fact, it's very interesting because, more or less, the material that now I'm getting from Baja, California, where I'm just leading this project, is more or less of the same age. I was like, oh, okay, that's interesting. I went to the field with Mark Goodwin and Kyoko Kishi. I went for the field trip and they showed me the localities from where the material was coming. I spent some weeks in the field with Dr. Clemens and with the crews, just to get to know a little bit the area and the different localities that they have, that Berkeley has in Montana.

01-00:35:23
Burnett: What is the age range of both the Judith River Formation fossils and the ones in Baja, roughly how many millions of years?

01-00:35:31
Montellano: Well, it's like more or less, seventy million years ago. So, it was interesting because when I returned to Mexico and I was hired by my university, it was looking for places in the northern part of Mexico. I was leading some projects, trying to find mammals, Mesozoic mammals—no luck. We were getting a lot of other things. Not only dinosaurs, but crocodiles and fishes and other vertebrates. Then, I was very lucky because here, one of the last graduate students of Dr. Clemens, Dr. Gregory Wilson, we're getting in touch. I don't remember exactly how, but we got in touch, and then we get together and we sent in this proposal to the UC Mexus [University of California Institute for Mexico and the United States] project program. Then, we started a project in Baja, California, where the crews of Los Angeles County Museum where Dr. Clemens, J. A. Lillegraven, Morris, Harley Garbani, all those people were working in the sixties and early seventies, and then we returned to that area. Since then, we are working together in Baja, California, where we're finding now mammals. Finally, after all these years, we're finding all these mammals.

01-00:37:19
Burnett: Why was it so difficult, do you think, to find the mammals? Were they just comparatively rare in that period?

01-00:37:26
Montellano: Well, I guess that with microvertebrates, although it looks perfect—you say, "This looks perfect!"—the grain size, the lithology, everything, also with its taphonomy, you have a reason to explain. But in Baja, California, it was not easy, but at least we were finding isolated teeth, so that was like, okay, at least something. Since then, we have been returning each year to collect sediment, to do some screen-washing, and to pick up all these microfossils. Fortunately, we have got some mammals, so that works very well for us now. Also, to survive in the system, in the academic world, I have to do other things, I have to publish some. I have been working with other fossils, with mammals, mainly, from Plio/Pleistocene mammals.

01-00:38:45
Burnett: During the development of your research for your PhD, what were the research questions that you were concerned with, and in discussion with Dr. Clemens, what were you trying to prove with your dissertation research?

01-00:39:05
Montellano: Well, I think that there was not really a question because the situation in those days, it was that very little was known about the diversity of mammals during the Judithian times. So, it was more paleontological in that way, we can put it. It was to know the diversity of mammals—what kind of mammals, they look more similar to which ones, to those that were described from Canada or from the southern areas, like San Juan Basin in New Mexico, and so on. In those moments, I think that the [book] *Mesozoic Mammals* started a new era because if you read the book of Lillegraven and Clemens and Kielan-Jaworowska, *Mesozoic Mammals*, it was in '79, it was the whole knowledge of Mesozoic mammals.

01-00:40:20
Burnett: Was in this book?

01-00:40:22
Montellano: It was in that book, and there were not a lot. Well, there were many groups, but the collections were not really very large. I think that there was a rising interest in Mesozoic mammals during that time, to know what has happened, where were they, what was the geographic distribution, where was the relationships, how old were they, what was the adaptations, everything, because we knew very little before 1979. That book, in fact, they have a phrase by Gaylord Simpson in the 1920s, 1930s, all the Mesozoic mammals known were able to fill a hat. That was the only thing. Now, if you go to Alberta, to Edmonton, if you go here in Berkeley, if you go to Wyoming, you go to all these places, you have huge collections of Mesozoic mammals. Cabinets full of all these, sometimes isolated, specimens. Then, it was just, I think, a boom to understand more about mammals. That was my dissertation,

only to describe which taxa were in the Judith River Formation in that locality and to try to figure out if there were some relationships among some of the taxa, if a faunal association, they were more similar to that of Alberta, in Canada, with all the collection and all the work of Dr. Richard Fox, compared to those of the southern part of the continent. Then, that was not something very fancy about that. [laughter]

01-00:42:52
Burnett: I'm going to apologize in advance for this question because I don't know if it's fair or not in a brief interview and for a general audience, but at that time, how does one determine association? What do you mean by "association" between fossil specimens?

01-00:43:16
Montellano: If you collect it in the same layer, then when we collect all the sediment, and in that sediment, you just do all the screen-washing and you pick up all the specimens, one can assume that it's an association. We don't know how much time is represented at the association. That's another question. One thing is to say that they are found together, but we don't know how many years that association represents—one thousand years, one million years, we don't know. We call it "association" because they are together.

01-00:44:03
Burnett: So, back to the early nineteenth century, you had this principle of faunal succession, right? If the layer is higher than the layer below, they're younger, right?

01-00:44:21
Montellano: The association is not assuming one of the super-position that is below is older than that that is above. If that has been modified, it's because something happened. Then, with that principle of stratigraphy, you can figure out that you are only talking about relative age. It's not absolute. That means I don't know how much time, but I know that that's older and that's younger. That's it.

01-00:44:54
Burnett: What happened with the K-T Boundary debates is that there was a call or a demand for much greater precision than they had before. Prior to that, it was hundreds of thousands of years, give or take. This was here, give or take three or four hundred thousand years. The debates called for much greater precision, it seems, at that time, and subsequently.

01-00:45:22
Montellano: And also, explanations. It's interesting, I think, when they published that paper, Alvarez and their colleagues, I think that, for example, the astronomers also got involved because the question was, okay, if somebody like [David] Raup and [Jack] Sepkoski suggested the periodicity of extinctions, what can cause that, then the astronomers say, "Oh, maybe we can have a binary sun. What is the probability of having a binary sun? Fifty percent?" Then, all these

kind of questions, I think that that was very interesting for the people who had to study this a little bit more from the sociological point of view. So many disciplines get involved—mathematicians also trying to figure out if the models of periodicity. When they tell you that you can make everything periodical, you can find always the ranges, you're like, "Okay," then it was, I think, very interesting. Maybe because it was a moment where a lot of sciences were just quite separated and they were starting to deal with one problem from different perspectives. Maybe that was what happened really in the eighties, in that point of view.

01-00:46:57
Burnett: An explosion of interdisciplinarity, yeah. Speaking with Bill Clemens, he's talked about interdisciplinarity being kind of a hallmark of Berkeley, right, that there was a lot of communication among disciplines even before the 1980s. What was your experience of the UC Museum of Paleontology? How did you interact or work with that institution?

01-00:47:26
Montellano: Well, I think that all of us who studied at Berkeley, you can distinguish [us] by the way that we write or propose our papers and so on. Always, they mention when you write something and it's like, who said that? Always to make a reference of someone, like to refer to someone who has said that. Not only because of ethics or so on, but always to try to support your ideas. That's very peculiar because you can see that you are saying something. I say, "Yes, but who said that?" Then, look for a reference. With Dr. Clemens, I learned always to try to look for another explanation. He has been always the kind of person that he has a broad interest on so many things, and he's always trying to be up to date in different disciplines. Not only in yours, like me in Mesozoic mammals, with him, sometimes I arrived at his office and I say, "Oh, it's because I'm looking for this." He always like, "Oh, just recently, this has been published," or, "Here, you can start to read about this," and so on. I found that very good and I liked that very much because when I teach, it's not only about that little thing that you know. You have to be very broad and to be up to date in a lot of things not only for your students, but also, for your interest. Then, you have a broad view or spectrum of everything. I have been always surprised because Dr. Clemens has all this variety of interests, and broad interest in so many things. Then, he was very interesting and it was very productive to talk with him about all these kind of things, and he was always open to new ideas. I think that he always was looking that you can see another possibility and be open to new explanations or to new data, new information, and how to include it in your own research. That, I think, was part of what I learned from him. He has always been very supportive. Now that I'm a researcher and some of my papers are being sent to him as a referee, I can distinguish when he's refereeing. [laughter]

01-00:50:48
Burnett: It's not blind. You *know*?

01-00:50:52
Montellano: It's because the way that he asks for questions or asks why you don't do this? He's always very positive and propositive, we will say. Like, "Why you don't try this?" Or, "Have you seen this?" Or, "I will suggest that you look for this paper." He's always like that, and I think that that's very important, also, in our field. Instead of saying, "No, I don't like it and that's it," it's more helpful for everybody, and it's also the science advance. It's like having all these kind of referees and all these kinds of comments. I can distinguish them immediately, like this is Dr. Clemens' comment.

01-00:51:44
Burnett: Because they're more constructive, they're more helpful, more productive? They're bursting with new ideas?

01-00:51:46
Montellano: Oh, much more. Oh, yes.

01-00:51:53
Burnett: It sounds to me that when you're talking about significant scientists, they have their own achievements, it's true, but what can be really important is this network, is that they are kind of a center of a network of great scientists who are communicating with each other. So, you mention this collaboration: you collaborated with one of his last graduate students, Greg Wilson, so I'm sensing that there's a network around him that continues. Is that a fair assessment?

01-00:52:37
Montellano: Oh, yes. In fact, when I was a graduate student, he sent me to work in Edmonton, Alberta, with Dr. Richard Fox. I think that he is one of the great characters, and he's a specialist in Mesozoic mammals. In fact, he started the department and the collection of vertebrate paleontology in Alberta, in Canada. He sent me to him, to check some material and so on. Dr. Fox, believe it or not, now I have spent my sabbatical year with him. Not sabbatical year—it was a sabbatical semester because I cannot stand the winter. That's too cold for me. I spent two sabbatical semesters with him and now I'm spending the summers working with him in Edmonton, Alberta, working with his collection, and learning a lot from him also because he's, like Clemens, this kind of person who is very generous and they are very constructive, as you say. I'm learning a lot. So, now, I'm spending some time with Dr. Fox in Canada, just working on his collection and working with him. Intellectually and scientifically, they are very similar in the way that they are generous, they are very similar. In characters, they are very different, but that's another issue. That's another thing.

01-00:54:29
Burnett: That's another interview, right? [laughter]

01-00:54:34
Montellano: No, no, no, I am very grateful to both of them because they have been very generous to me. Then, that's like the network, and I can feel that that happens. I think that in science and in all the areas, it's like that. Now, with my students, it's the same. Sometimes, I say, "Why you don't get in touch with this person? Tell them that you are my student, and maybe the person can help us. So, he can direct us to someone who can help you to develop these kind of things," and so on. That works like that.

01-00:55:07
Burnett: Science is social.

01-00:55:10
Montellano: In that sense, yes.

01-00:55:13
Burnett: It's about people who know each other and these connections. You have become, I imagine, a center of a network in Mexico as well, and your students have gone on. Do you take inspiration from this kind of culture that you grew up in—grew up, scientifically—do you pass that on in Mexico?

01-00:55:39
Montellano: Oh, sure. We're a little bit isolated in certain ways. We don't have the collections, we don't have a lot of things, but now, it's so easy to get in touch with people. Before, you had to write or phone, that was the only way. Now, it's like you can find the email of someone who knows where is that person who can get in touch with someone else by email. So, all these social networks, Facebook, LinkUp, or whatever they are called.

01-00:56:16
Burnett: LinkedIn, yeah. Social networking.

01-00:56:18
Montellano: All these kind of things. I don't use that very much but I need to understand a little bit how they work, and to be part of that. So, I think that we're more global. For example, to participate in all these meetings, although they have been growing so large. For example, the SVP [Society for Vertebrate Paleontology] meeting is really very large. Sometimes I say, "No, I'm not going to return." It's like one thousand people. Who is going? Because my session of Mesozoic mammals is still small, then I am able to get in touch with people, to talk with people, to meet all these colleagues from other countries. Always, I think that though you can get in touch by email, it's not the same thing. For example, when I meet Dr. Clemens in the meetings, to see how he's doing, to talk with his wife, how the family's doing. More like the personal issues, to see Mark Goodwin, to see people from Berkeley, from other universities, from another country, sometimes, is when you get in touch. It's not only by email; it's just more like the human contact, and more like another kind of interest. I think that that's a reason that all these meetings

continue going and going. The main object, a lot of time, is not only to present your job, but also the networking.

01-00:58:08
Burnett: It seems that there's a culture in paleontology that's different from other sciences, a lot, I think. I will advance this as a hypothesis and you can say what you like about it: that there's this work in the field, intense work together, you get to know each other really well. In other sciences, you might have a white lab coat, a suit and tie, a degree of formality, *Herr Doktor Professor*, those kinds of appellations. I don't get that sense from paleontologists. There's a degree of warmth and informality that is almost immediately apparent to an outsider. So, I don't know if that's true or not, but it stands to reason, I think, if you just describe—as you described—what it's like to be in the field, there is something profoundly social and sociable about the interactions in the paleontological community.

01-00:59:22
Montellano: I think to be in the field is not so easy because in the field, we always say that the copper inside of you appears.

01-00:59:41
Burnett: The copper?

01-00:59:43
Montellano: We say, "*reluce el cobre*." That means that when you are in the city or you are in the school, whatever, there are some manners and there are some social rules. Then, everything got easy, you see? You can understand, but when you are in the field and it's hot like hell and there is not a lot of water and you are just walking and walking and you haven't found nothing for the whole day, your mood is just like, "Don't talk to me." If later, you have to share your water because you have to do a jacket [casting a fossil in plaster], because other person, something happened, you cannot be so selfish. You have to help the other person. The other person, they cannot be like that—you have to help carry things, you have to help cooking, although you are very tired, wash dishes, all these kind of things that it takes a lot of your—. We are part of a group and we have to survive like a group. You are not by yourself. That's a reason to show the copper. I know a person who I will never go with to the field, *no way*, and that's the test for the students sometimes. If they can stand to be in these conditions, that you don't take the shower for two weeks, that you are so tired that you don't sleep well because the coyote was howling, because the rooster was starting so early, and you have to wake up and put the water on for the coffee, all these kind of things is not very easy. It takes a lot of stress in your daily life. So, not everybody can stand that.

01-01:02:05
Burnett: So, it sounds like Dr. Clemens, he seems to have both—he's got the flexibility to roll with things when they're difficult in the field and he can also be really

warm and formal, in some ways. So, that's kind of an inspiration, too, he can move in all these different worlds easily.

01-01:02:26
Montellano: I think that is one thing also to learn, but it also has to be part of your personality. That's part of your personality also—not everybody can stand that. One thing that I learned from him in the field is that you are not in the field to suffer. You don't have to be like, eating from cans. You don't have to really think that you are in hell. It has to be a nice experience and to have enough...

01-01:03:11
Burnett: Balance, maybe?

01-01:03:12
Montellano: No. It would be like commodities? I don't think that it would be "commodity" in English.

01-01:03:22
Burnett: "Amenities," yeah.

01-01:03:25
Montellano: Yes! It's not for suffering—if you can have fresh food and you can cook whatever you want and you can sleep enough and at noon you can stop and have a nice meal and talk and drink water, so it's not hell. You have to enjoy being in the field because I know some people that I say I will never go with those to the field. It's because it's survival, cans and crackers, and you're like, "No way, there's no need." If you can have all these kind of things, I don't know why you cannot spend time just cooking and relaxing and talking and being well-fed and have a good sleep—well, more or less, but to have enough sleep. In his [Clemens'] camps and in the way that he always feels, I think, about field trips, that's fieldwork.

01-01:04:29
Burnett: It's part of it, in order to do it, because it is difficult, because it is challenging, there has to be a kind of compensatory, the work culture and a life culture that is more enjoyable, as you said.

01-01:04:46
Montellano: It's enjoyable, so why not do it? If you can, why not do it? Why not? [laughter]

01-01:04:57
Burnett: I wanted to circulate back just to one question I had: you talked about the relative isolation of the Mexican paleontological community, that they don't have the degree of collections of the United States or another European country, but I notice that UCMP, for example, has digitized collections to an extent. Has that proved to be useful or is that going to be useful in the future for universities and programs that have less access to collections? Is that going to help?

01-01:05:38
Montellano: I think that all these now, with all the things online, has helped a lot. When I started, to get papers [journal articles]—and I'm talking from my university, where the collection that we have in our library is one of the best ones in geology and so on. In those times, we had to wait until that volume arrived to have a copy of that. The way that we used to do the bibliography research, we had to wait for all these volumes, indexes, and so on. Okay, after one year, maybe, you have it. It was the only way to do it. It was the only way that you can be up to date. Now, for example, my university has improved a lot and we are subscribed to a lot of these online services. We're very lucky, at least in my university, which is the largest in Mexico, we have access to a lot of journals and to download a lot of papers. That helps a lot because you can have access to what has just been published last week. Then, all the online things that there are now, a lot of museums that are also sharing all their catalogues and so on, they are very useful. I think also, at the same time, that you know what is in the collection, where it is, but at the same time, you have to have a look at it.

01-01:07:45
Burnett: You can't just look at a photograph.

01-01:07:47
Montellano: No way. Also, neither in the descriptions, that that's one thing, for example, now with Dr. Fox, his descriptions, as well as Clemens', they are more or less of the same age, are just superb because you can follow. You can see the specimens. It's just what they are describing. But other ones, you're like, "Did he see *what*? Where is that? What he is calling *that*?" [laughter] Then, you never can be sure. With those, I know that if they describe it, it's because it's there. To have a look of the specimens by yourself, you cannot substitute that with nothing. Also, now that there are, for example, all these 3-D...

01-01:08:45
Burnett: Right, holographic.

01-01:08:47
Montellano: It's just like, "Yes, but there is something." Maybe it's because it was your education, maybe because of your eyes, maybe because it was your subjectivity or objectivity, whatever.

01-01:09:07
Burnett: Right, but your way of seeing, your perception, it might also be tactile? Are you touching the teeth?

01-01:09:15
Montellano: No, no, you can't. Well, those of Mesozoic mammals, no, it has to be in a pin, or it has to be so tiny, but you have to look at them.

01-01:09:25
Burnett: There's a resolution that you get from being with the physical entity that you cannot get, no matter how good the photograph.

01-01:09:33
Montellano: If it's very good, maybe you can, because it's getting— I think that that is changing also the importance of going through the collections. That's the reason also, I guess, that the collections and the museums, they are very important not only because they are recipients of all the patrimony or all the fossils of certain areas or country or a state or whatever, but also, one of the reasons for being is to share what they have with other museums, so with the staff and other researchers, to see the material by itself.

01-01:10:22
Burnett: Right. That's very, very important. Well, I want to thank you very much for taking the time to talk with us. I look forward to learning more about UCMP and the career of Dr. Clemens as we move through this project. Thank you for your time.

01-01:10:40
Montellano: Okay, thank you very much.

[End of Interview]

Oral History Center
The Bancroft Library

University of California
Berkeley, California

The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology:

Zhe-Xi Luo

Interviews conducted by
Paul Burnett
in 2015

Since 1954 the Oral History Center of the Bancroft Library, formerly the Regional Oral History Office, has been interviewing leading participants in or well-placed witnesses to major events in the development of Northern California, the West, and the nation. Oral History is a method of collecting historical information through tape-recorded interviews between a narrator with firsthand knowledge of historically significant events and a well-informed interviewer, with the goal of preserving substantive additions to the historical record. The tape recording is transcribed, lightly edited for continuity and clarity, and reviewed by the interviewee. The corrected manuscript is bound with photographs and illustrative materials and placed in The Bancroft Library at the University of California, Berkeley, and in other research collections for scholarly use. Because it is primary material, oral history is not intended to present the final, verified, or complete narrative of events. It is a spoken account, offered by the interviewee in response to questioning, and as such it is reflective, partisan, deeply involved, and irreplaceable.

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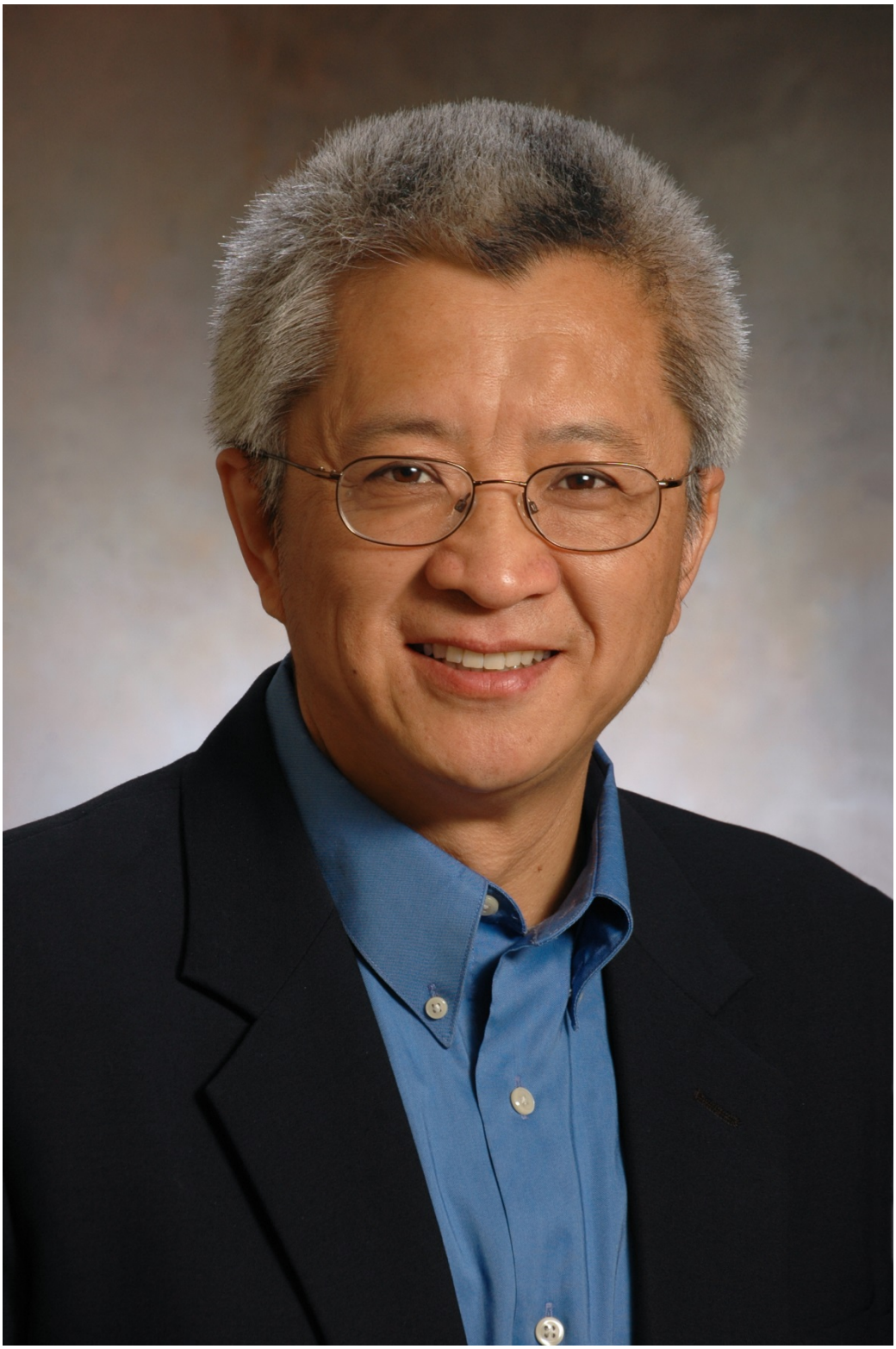
It is recommended that this oral history be cited as follows:

Zhe-Xi Luo "Zhe-Xi Luo: The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology" conducted by Paul Burnett interviewer in 2015, Oral History Center of the Bancroft Library, The Bancroft Library, University of California, Berkeley, 2017.



Zhe-Xi Luo, photo courtesy University of Chicago

Zhe-Xi Luo is Professor, Department of Organismal Biology and Anatomy, and a member of the Committee on Evolutionary Biology at the University of Chicago. He received his PhD from UC Berkeley in 1989.

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Interview #1 April 14, 2015
Begin Audio File 1 Luo_Zhe-Xi_01_04-14-15_stereo.mp3

01-00:00:00
Burnett: This is Paul Burnett interviewing Dr. Zhe-Xi Luo for the UCMP Bill Clemens Oral History Project. It's April 14, 2015 and we're here at the Bancroft Library. Dr. Luo, welcome.

01-00:00:20
Luo: Thank you. Glad to be here.

01-00:00:23
Burnett: First of all, I wanted to ask about how you became interested in science and how you became interested in paleontology. But first, why don't you tell me a little bit about where you grew up.

01-00:00:37
Luo: I grew up in Sichuan Province and so I'm a Chinese native. And in 1982, after my undergraduate degree, I came to United States and I landed, entirely by luck, in Berkeley, California. So my interest in paleontology did not really start until college, okay. And I want to give you the context. My years of growing up is in the tail end of the Cultural Revolution in China, which, by historical consensus, ended in 1976. And so I was among the first bunch of high school graduates who were able to take this national entrance exam to get into university. That system was in China decades before but in the Cultural Revolution it was suspended. The path of Chairman Mao was changed, with the end of the Cultural Revolution, this new system to enlist students by merit exam got reintroduced. So I was a beneficiary of that. So my interest in paleontology entirely came by accident. And this was the first opportunity for many youngsters to get into a university after the Cultural Revolution. You are so excited and you just want to go somewhere. I was admitted to Nanjing University to study geology and paleontology but I never filled that major or that university in my examination form. So it's arbitrarily assigned to me. I never looked back because that's a great new life for me. So I happily went to Nanjing for four years of study and after that I happened to receive a Chinese government fellowship to study for PhD in paleontology in United States. And several of my Chinese mentors in the academic community highly recommended that I came to California, to work with Bill Clemens. I did and so that was how I started my serious interest.

01-00:03:22
Burnett: Can I ask you, was there a connection between your mentors and Berkeley before?

01-00:03:31
Luo: Yes, very much so. There was a very important, earlier episode before the founding of People's Republic of China. There were several Chinese scientists who were trained or studied in the field of paleontology in the United States (in the 1940s). One very important person was Minchen Chow who studied

vertebrate paleontology in America. And in 1979, Minchen Chow led a delegation of Chinese vertebrate paleontologists to visit United States. And Berkeley, California, is one of their stops. So Minchen Chow got to know Bill at a more personal level. And then, if I remember right, it's around 1980 Bill went to China with American Museum curator Malcolm McKenna and there Bill visited many fossil sites and also got to know the Chinese colleagues better. And it's really through that early interaction between Bill and the Chinese paleontologists, especially two of my mentors, that my mentors recommended that I come to California.

01-00:04:53
Burnett: Yeah. And was that one of the first Chinese-American collaborations in 1980 because I guess the détente is the early 1970s and there's the beginnings of openings of trade in the late seventies. Is this one of the first exchanges paleontologically speaking?

01-00:05:16
Luo: It is not the first exchange. The two American academics who went to China first, right after Cultural Revolution finished, was Philip Gingerich of University of Michigan. He was the very first person in vertebrate paleontology to go to China. And the other person was Bill Schopf, who was a professor of paleobiology at UCLA. He was also in China before the end of 1970s. These are just a couple of years after Cultural Revolution. I think the very first Chinese paleontology delegation of scientists visited United States, it was 1979. So about one or two years afterwards, Bill went to visit China either in 1980 or 1981. So these exchange visits were very important early dialogue for people to get to know one another at a more personal level. Not yet a full-scale scientific collaboration. This all happened in the period of rapid change of international relations.

01-00:06:48
Burnett: Right, right. It's so quick.

01-00:06:50
Luo: I came here in 1982, was a direct product of that early exchange. Of course, these are all my mentors, professors, and I was just a twenty-three-year-old student.

01-00:07:05
Burnett: Yeah, yeah. So you were excited to be there and you arrived at Berkeley and started your graduate program.

01-00:07:12
Luo: That's right.

01-00:07:14
Burnett: So were you working with Bill immediately or did you take time to discover your direction?

01-00:07:22
Luo: I was admitted to other universities. I was admitted to University of Michigan. If I went there I would have worked with Philip Gingerich. I was admitted to Johns Hopkins University where there was very strong paleobiology program. But of all my choices, California is my number one choice. So that is why I came here. I basically came to work with Bill. Even though I had never met him, okay. In a way you ask a very interesting questions. That's probably appropriate for most of the American graduate applicants, how you got interested in that and why. And I did not even ask those questions. I saw a very exciting opportunity, without even a second thought just went.

01-00:08:25
Burnett: Right, right. But you had had training in paleontology before?

01-00:08:31
Luo: In geology, in paleontology, but not in the particular discipline that Bill Clemens is famous for, and that is study vertebrate paleontology and, of all things, to study mammals. Of all mammalian history, to study the early mammal history.

01-00:08:48
Burnett: That's right. Right. And it's a very exciting time in vertebrate paleontology, in mammalian paleontology. After you did coursework and after you embarked on your research project, what was the question or problem that you decided to focus on?

01-00:09:15
Luo: I was primarily interested in morphological evolution. For Bill's primary research agenda back then, it was this broader mammalian faunal evolution of Cretaceous-Tertiary transition. Back then we call it Cretaceous-Tertiary extinction event, the K-T event. But we now call it the Cretaceous-Paleogene transition. And Bill's primary concern around that time was to get a full understanding how this extinction event unraveled and a very big part of the picture is after dinosaur extinction, or even prior to the dinosaur extinction, how did mammalian fauna respond to that enormous ecological crisis. So Bill's main interest back then was to understand the placement of the major fossil sites in the geological transition, characterize the basic fauna, and essentially almost every one of Bill Clemens's students took one piece of that work. I was fascinated about it but for my own immediate interest I was always intrigued by the morphological evolution. Less so about how the mammalian fauna transition went through that global crisis. Okay. So one is about broader picture of mammalian fauna evolution and one is to focus on the morphological part of it. Eventually, through multiple rounds of interaction, I did a hybrid PhD thesis project. I did one part that Bill definitely wanted me to do and which I eventually benefitted from because I acquired that general skill set and interest, and that is to work on a particular mammal taxon. Back then we considered them to be ungulate mammals but they are now considered to be the some extinct mammals. They just acquired some

herbivorous adaption. The particular species is called Protungulatum. Ungulatum means ungulate. Protungulatum means proto-ancestral ungulate. And that was one species that I studied under Bill's overall guidance. But it's also based on Bill's scientific collection. Bill's team has been working on this long before I was even an undergraduate student. So Bill and his team collected a lot of fossils. And so I went into the UCMP fossil collection as a research assistant, helped to sort the collections. So I worked a piece of mammalian skull structure called petrosal and that's a piece of ear bone. I loved that part. Eventually I wrote half of my thesis on those ear bones and half of the thesis on the teeth or dental fossils of the mammals that are very important for that general transition.

In the end I really benefitted from that PhD thesis project as my morphological evolutionary interest was essentially built one step at a time to where I am now. Many years later, for the next major episode of fossil mammal discoveries in China, the first thing to study about them are their teeth. And so my early work and training with Bill Clemens about that, the very basic studies of mammalian dentition, benefited me enormously.

There is also a very interesting aspect about it — Bill Clemens's study or attempt to accurately characterize this whole vertebrate evolutionary process across that important Cretaceous to Paleogene transition was under the debate whether the asteroid was a major cause for the extinction. I saw the scientific debate and I was very much influenced by this kind of down-to-earth scientific attitude. And that is, whatever the largest scientific question you have framed, you have to bring that scientific question to some empirical test. For paleontologists this empirical test will have to come from fossils and will have to come from this imperfect fossil record that are fraught with preservation issues, how the fossils get buried in the fossil site, what are the geological relationship in the field, how one site is related to another site. And I got a sense that, even for the largest scientific questions, there are many important details. Any of those aspects can be very informative, but simultaneously I got this realistic expectation about how much we can do to address some of the scientific questions. And this gave me the training to figure out the best way and how to realistically approach it whenever I encounter a broader scientific question as a practicing scientist.

01-00:16:02
Burnett: Right, right. But, as always, with big scientific questions there's this question of epistemology, right? How do we know something that big? And the physicists could say, "Well, we know this. This iridium layer tells us that it happened at this time." For paleontology, the rock fossil record is incomplete. So species might disappear, a million, two million years on either side. And so the question is, what is your test of that? What's the evidence? And so you were enchanted by the carefulness of Bill's approach, it seems. That he approached things with what others have said, a kind of humility epistemologically towards understanding the complexity of the fossil record.

01-00:17:02
Luo: Correct. One of the best qualities of a scientist is really to know what you don't know. And you have a fair assessment of uncertainty. And many of Bill Clemens's counterarguments about the bolide [asteroid impact] hypothesis of extinction in the K/Pg event is really from that fundamentally empirical attitude. And from the fossil record you definitely can see that not all aspects of the terrestrial vertebrate fossil record in North America—which is one of the best for studying the K/Pg transition—not all aspects of it are fully consistent with the bolide hypothesis. The influence of his role model is more of this very pragmatic empirical approach and to argue that no matter how big your new idea is, ultimately it had to be borne out by a piece of our empirical evidence. The other is really to have a fair assessment about uncertainty when you're approaching the big scientific questions. Now, who won, who lost on that debate is besides the issue. The issue is really through this kind of dialogue or proposition of hypothesis, testing of hypothesis to learn a scientific approach.

01-00:18:47
Burnett: And it sounds like it was in the end beneficial, that paleontology grew in its methodologies in partial response to the impact hypothesis.

01-00:18:59
Luo: That's right.

01-00:18:59
Burnett: Or some could say it was derailed, it was put off its track a little bit. But it seems to have responded with new techniques, new questions, and developed in a new way fundamentally.

01-00:17:17
Luo: Yeah. My entire time here, Bill Clemens completely focused on that larger quest. So far as the graduate study goes, the big questions that your mentor is interested in can serve as a bigger backdrop. But given how in our field PhD project usually develops, we also each have our own immediate focus. And this morphological evolution focus was a benefit to me. And I benefited also from my graduate student cohort. I would also name the people who I really benefited from are Timothy Rowe, who is now a professor at University of Texas at Austin, and Lowell Dingus. His meticulous approach to just very simple tasks really impressed me. And also they are the older graduate students and so they are far more articulate and I felt that I was a starry-eyed youngster coming from China, not only getting to a foreign country for the first time in my life, also getting into scientific research for the first time. I really learned a lot from my fellow graduate students. So I also feel very thankful for the overall ambience and my lucky association with many very capable graduate students who, it turns out, to be much better than me.

01-00:21:21
Burnett: Well, coming from China to the United States can be an adjustment. Did they help you adjust to landing in the United States? It's a different culture, it's a different world, I suppose.

01-00:21:38
Luo: Well, when you come to a university and study, this is all the same, American or foreign students. The first thing you worry about is your financial support, right? So in that regard the Museum of Paleontology, back then still also the Department of Paleontology, really supported me after the first year. And then in the way of cultural adjustments, my fellow graduate students were so welcoming. But I would definitely credit, and I feel very thankful, for Dorothy and Bill Clemens. They really took care of me. And when I landed here I stayed with Bill and Dorothy for about four or five days, until I adjusted to the local time, and then I moved into a different place where I lived. I'd go to their house very often, get invited for Thanksgiving dinner, Christmas party. So I'd get to know Will Clemens very well, Bill's son. I met, I think, all of Bill's daughters at Bill's house. I think Bill was not treating me specially. Bill is always very generous with all his students and I benefited as part of it.

01-00:23:04
Burnett: Yeah. I see.

01-00:23:07
Luo: So that really helped me. And I very much cherish my Berkeley experience. My fellow PhD students are a very social bundle, too, and that's enormously encouraging and helpful.

01-00:23:22
Burnett: Good, good. And you said you worked with the collections that were at UCMP—

01-00:23:29
Luo: That's right.

01-00:23:29
Burnett: —in your work. Did you also, as part of your training, go to the field? Did you go to Montana or places like that for your training?

01-00:23:40
Luo: Yes. I went three times, okay. When I first got here I already missed the field season of 1982. And then in 1983 it was my first time to go into the field with Bill Clemens. I had one funny experience. Bill's team had already left, okay. Mark Goodwin, who is now assistant director of the museum [UCMP], was to go later. So I ride with Mark and we drove out from San Francisco Bay Area toward Montana. Mark drove, on first day, through the afternoon and night, and we got all the way into Idaho close to the Yellowstone. That was an enormous long drive. Then we camped by the roadside because we were so tired. And I eventually slept in Mark's jeep. And Mark has a dog, and by the time I woke up the next morning I realize I have hugged Mark's dog and slept

for a whole night. So that was my first trip. And the first season is also very educational for me. In that 1983 field season Bill Clemens hosted a very important field conference with all major proponents of the impact hypothesis and also the major critics of that hypothesis, all in Montana to study, to evaluate the geological sections, to look at the fossils, and to discuss how the Montana terrestrial Cretaceous-Paleogene transition can bear, support, or falsify the asteroid hypothesis. So I met Walter Alvarez, a very important person in this debate, and I also met Eugene Shoemaker, a very important person in this debate. And I also, first time, met my later collaborator, Zofia Kielan-Jaworowska, a very important Polish paleontologist who is also a long-time collaborator on this important book that Bill co-edited. And then I met for the first time Malcolm McKenna, Bill's long-time friend, a very important scientist in my field, curator at American Museum of Natural History. And I also met Leo Hickey, who is a paleobotanist, who together with Bill Clemens, was the early critics of the impact hypothesis, who later would become the Yale Peabody Museum director.

01-00:27:04
Burnett: That sounds like a wonderfully—

01-00:27:06
Luo: It was a wonderful experience for me to be in that field conference. That's part of our general field season out in Montana.

01-00:27:17
Burnett: But it was extraordinary. It was a special event convened to enhance the research and to solve the questions.

01-00:27:28
Luo: Or to debate.

01-00:27:28
Burnett: Or debate, yeah.

01-00:27:30
Luo: And I did not think much of it. But, you know, the aftermath stayed with me. Many years after I moved on with my career, when I reflected on the important thing that had happened in my own training—training probably is not a right word—enlightening of my mind in those early times, that was a very important academic occasion. The other very interesting important occasion was—it's about a year after that. Bill Clemens and Walter Alvarez co-organized a seminar series that invited all the major scientists involved in this debate to come to Berkeley to give seminars. That was a very exciting debate, too.

01-00:28:37
Burnett: And what year was that? That was eighty—

01-00:28:38
Luo: It should be about '84 or '85.

01-00:28:48
Burnett: So you were there and were a witness to the great debates—

01-00:28:52
Luo: That's right.

01-00:28:53
Burnett: —that were central to paleontology in the world at that time.

01-00:28:56
Luo: I was a beginning graduate student. I was not particularly knowledgeable, okay. I have nothing to contribute around that time. I'd basically just hang around and listen and watch. Whereas the older graduate students, I remember particularly Lowell Dingus, who was very much part of it, participating in the discussion. The reason I said that Lowell Dingus influenced me is he is about four or five years ahead of me. By the time I get in Berkeley, he is about to graduate. He did his whole PhD thesis on those faunal and the geological problems in northeastern Montana. The other very interesting person was David Archibald. David around that time was an assistant professor at Yale and so those two older students of Bill's really gave me a sense of what a role a young scientist can play when the great titans are disputing one another.

01-00:30:12
Burnett: Right, right. The "apex predators" of the paleontological world. So you completed your dissertation that was partly about the Protungulatum.

01-00:30:25
Luo: Yeah. Partly also about the petrosal bone of the mammalian skull that contains the ear structure.

01-00:30:44
Burnett: Right. And you were interested in the extinction debate, but your general focus is on morphological evolution, the evolution of structure.

01-00:30:41
Luo: That's right.

01-00:30:42
Burnett: Right. And so you complete that in 1989 and immediately you have a post-doctoral position at Harvard. Can you talk a little bit about that? With Professor Alfred W. ["Fuzz"] Crompton?

01-00:30:57
Luo: Yeah. That also had a context with Berkeley. One of the most important fossil sites ever for dealing with early mammal evolution is in Yunnan, southwestern China, and this is the Lufeng Basin. And this site produced some of the earliest known—what we now call mammaliaforms, or near-mammals, already anatomically like mammals, but not quite yet placed in the living mammal clades. And the fossil mammals from the early Jurassic of southwestern China are very, very important. When I was in Berkeley, a

senior Chinese scientist, her name is Sun Ai-Lin, visited Berkeley UCMP. And I was generally trying to help with her visit, to be an interpreter and show her around. And she is well-known senior scientist, and I was just a student. And then she is the one who suggested that she had these fossils, and she early on worked with Crompton. She was looking for somebody younger with better language skill and perhaps western training to work with Crompton. So Sun Ai-Lin introduced me to Crompton after I met her here in Berkeley. That was the initial contact that she helped to provide. And later on, I started to discuss with my post-doctoral mentor, Crompton, maybe the last year or so when I was in graduate school. So about the time I finished the PhD thesis I went to Harvard to work with Crompton primarily on the Chinese fossils. Whereas the project that I worked under Bill that fulfilled my PhD thesis is entirely on the Montana fossils. Okay. And this Yunnan fossil work gave me an important, different perspective because of the two different kinds of projects. And I felt that I benefited from both.

01-00:33:36
Burnett: Were they the same period?

01-00:33:38
Luo: No. The Chinese fossils I worked on as a post-doctoral fellow was from early Jurassic.

01-00:33:47
Burnett: Oh, I see.

01-00:33:47
Luo: Whereas most of the UCMP fossil that I worked under Bill and literally accumulated by Bill's team was from latest Cretaceous. So they are about 130 million years apart.

01-00:34:03
Burnett: Right, right. But also a different geographic space, different types of fossils and it broadened your horizons in terms of research questions with respect to morphology.

01-00:34:17
Luo: That's right.

01-00:34:20
Burnett: And so you completed the Harvard post-doc and then you got your first position. You were assistant professor.

01-00:34:27
Luo: At the College of Charleston. And it was the first bunch of applications that I sent out, and with my first job interview, I was offered a job and so I happily went. Okay. I worked in Charleston for about four years and then I received this curator's appointment at Carnegie Museum. This is where I spent longest part of my career. But for logistic reasons, my family had a second child born,

so I postponed my start at Carnegie Museum for about a year. And eventually I went from South Carolina to Pittsburgh, Pennsylvania in 1996.

01-00:35:17
Burnett: Yeah, yeah. And that's a tremendous collection, is it not?

01-00:35:22
Luo: It is a very important institution in vertebrate paleontology, both historically and also in the way of very active current research. So I was thrilled that I would get a full-time research scientist position as a museum curator. I enjoyed it. Much of my career was really with Carnegie Museum, with the support from my colleagues, and with the research resources. And it was also in perfect timing, when China's fossil discovery just exploded. And so it was a good time for me. Most of my substantive research collaboration with various Chinese institutions and with many colleagues and friends really started big-time when I first moved to Carnegie museum.

01-00:36:22
Burnett: Yeah. I was looking through your CV and you led field research in Gansu Province.

01-00:36:33
Luo: That was a very interesting time, yes.

01-00:36:34
Burnett: Yeah. And this is a year after. It says in 1997. So a year after you received your position at Carnegie you begin to lead this field research. Can you talk about that research a little bit in Gansu Province?

01-00:36:52
Luo: Well, early on I already went to southwestern China on the Early Jurassic fossil studies.

01-00:37:01
Burnett: It's part of that work.

01-00:37:02
Luo: It was during the Yunnan project with Crompton, while morphological study was ongoing, some of my Chinese colleagues recommended that this Cretaceous site in northwestern China in Gansu Province would be worth exploring. And around that time I was very lucky. I received a National Science Foundation CAREER award, so I got some funding and so I was looking at what new projects parallel to my existing project that I can launch and do something different. So I picked early Cretaceous site in northwestern China with my collaborators. Logistically it was possible for us to get there, do the fieldwork. It did not discover too many fossil mammals. But we did have some very interesting dinosaurs. So I worked with some collaborators. And I myself was not the primary scientist studying those dinosaurs but I definitely led the team that essentially acquired some of the dinosaur fossils. While we were doing that, a major discovery started to unravel and that is the

feathered dinosaur sites in northeastern China. And coming with these very important eye-catching feathered dinosaurs are also some very interesting fossil mammals. So this episode of my study started sometime around 1997 basically until this day. Okay. And there were so many discoveries that changed our perceptions from those field sites. So I was very lucky, when and where I was in China in full-scale collaboration on some different works, that this important discovery from a different part of China really became available.

01-00:39:32
Burnett: You wrote an article about this period. Something like the number of fossils discovered since 1980 is ten times the number of fossils discovered before 1980.

01-00:39:50
Luo: Yeah. And so in a way it's the sheer volume of this discovery that changed our perception today about the early mammal evolution as compared to when I started under Bill Clemens.

01-00:40:08
Burnett: Yeah, yeah. And Bill and his team was responsible for growing the number of fossil discoveries.

01-00:40:16
Luo: In his own time.

01-00:40:18
Burnett: In his own time, yeah.

01-00:40:20
Luo: In his own time. If you want to characterize in such a manner for historical narrative, I would put it this way, okay. Before Bill Clemens's time it was mostly a handful of individual specimens for most of the early mammals. And it's right around the time of Bill Clemens's PhD thesis work, Bill and his cohort of PhD students here at Berkeley started to use the screen washing technique and that really transcend the collection to a next better level with hundreds of specimens. And in the case of UCMP collection around late 1970s, early eighties, there are a couple of thousand specimens, which varies depending on how you count. And that was a phenomenal growth of Bill's time, when Bill was actively pursuing the research collecting. Of course, we already discussed that later the Clemens team's attention is really to place these fossil vertebrates in the broader context of the important Cretaceous-Tertiary transition. Thus amassing additional new fossils was no longer the main emphasis. Okay. So if we go from that point forward, around the time when Bill Clemens wrote that textbook, the Mesozoic mammal book and the first—

01-00:42:05
Burnett: Yeah, the volumes, I guess.

01-00:42:06
Luo: Yeah, *The First Two-thirds of Mammalian History* book, also published by University of California Press, I think it's about eighty, ninety, a hundred genera known at that time.[1] That's around the time when I started my graduate study. Okay. By the time I wrote the 2007 review there are about 320, 340, 350. So for my intellectual growth from a first-year, entering PhD student to a museum curator with ten years of research history behind me, the early mammalian fossil record basically tripled. Okay. Now, more than just numbers. It's really the kinds of fossils we have found, the kinds of evolutionary adaptations, ecological specialization that we came to realize with these newly discovered fossils that started to change our perspective about this early mammalian evolutionary history. I'm quite proud of it. The reason being we ourselves are mammals. Were it not for those numerous evolutionary experiments, numerous explorations of different ecological niches in the Mesozoic, and that we happened to survive those extinctions up to this day, there wouldn't be you and me talking.

01-00:43:53
Burnett: That's right.

01-00:43:55
Luo: So this rich diverse early mammalian history fundamentally is relevant to our overall understanding of biological evolution.

01-00:44:06
Burnett: And didn't you and other scholars talk about early assumptions in mammalian history? That because the fossil record was so limited and because of genetic tests of existing mammals showed that mammals are probably much older than their earliest fossils. There was debate about the extent of evolution in this missing record of mammals.

01-00:44:38
Luo: Yeah, that's right.

01-00:44:41
Burnett: And can you talk about what we learned since that time about the likely evolutionary patterns in early mammalian evolution? The diversity, for example. Is it greater than we thought, is it less than we thought?

01-00:44:56
Luo: It's definitely much greater than we thought, okay. And let me back step a little bit on the context. A very exciting aspect during my career's time is the size of the fossil discovery. I love fossil discovery. But parallel to that is a general approach of using DNA sequences to infer mammalian evolution. This is just impacting my field in unimaginable terms. Very exciting. One of the fundamental assumptions, using molecular sequences of extant mammals

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[1] J.A. Lillegraven et al., *Mesozoic Mammals: The First Two-thirds of Mammalian History* (Berkeley: UC Press, 1979).

to backtrack the early mammalian evolutionary history is a technical procedure called fossil calibration.

01-00:45:55
Burnett: Fossil calibration?

01-00:45:57
Luo: Yeah, fossil calibration of the molecular clock. And because there's enormous interest molecular evolutionists to use their kind of data, and for their kind of research approach to understand evolutionary history of our group, they need to interact, they need to incorporate the fossil data as the calibration points for the basis of their molecular time estimate. And this all of a sudden made my kind of research somehow relevant also to theirs. So I was very lucky in that my kind of work gained an unexpected and broader audience. Now, it is really in the context of that intellectual dialogue, one particular research hypothesis surfaced. And that is, overall, when you use the molecules, especially DNA sequences, to estimate the timing for evolutionary diversification, they tend to be much older than the earliest known fossils that can be attributed to certain lineages. And so the general assumption of some of the molecular evolutionists, not all, some, was they are estimating the time of divergence correctly but paleontologists may not have found the divergent morphology to really attribute the more fragmentary fossils to the correct lineages. And one extension from that discussion was some molecular evolutionists argued that there was a decoupling, of genealogical split versus ecological diversification. So the expectation or the prediction would be early mammal fossils cannot be really assessed with regard to molecular evolution or resolving this time discrepancy. But lo and behold, when better fossils become available, we demonstrated that the earliest mammals are just as ecologically diverse as some of the modern mammals.

01-00:48:46
Burnett: So what does that mean? So the decoupling is not true?

01-00:48:49
Luo: Decoupling is not true.

01-00:48:52
Burnett: Okay.

01-00:48:52
Luo: There is no decoupling of ecological specialization from genealogical split in the earliest mammal record, which was used to account for this gap expected by molecular time estimate.

01-00:49:11
Burnett: Right, right. Wow, that's fascinating. So I want to capture some of the other types of work. You were a curator at the Carnegie Museum of Natural History. You were in charge of the collections and managing the collections. You were

assistant and associate, then full curator, and at the same time you were also a professor at University of Pittsburgh.

01-00:49:40
Luo: Yeah, adjunct professor, yeah.

01-00:49:41
Burnett: Right. So you were also teaching as part of that.

01-00:49:44
Luo: That's right, that's right.

01-00:49:47
Burnett: And a lot of education outreach was part of your—

01-00:49:53
Luo: Part of my job. To be a successful science educator I think one thing you have to have is your own interest in it. You want to do it. The other would be what you do as a pure scientific inquiry, happens to attract a public interest. For example, if you work on dinosaurs, many lay audiences will be fascinated about your work. Mammal research is never as hot as dinosaur but nonetheless the general public is interested in fossil discoveries, and in paleontology. And it only takes two minutes to convince somebody our earliest history in mammalian evolution actually is relevant to us. Okay. So it's really thanks to that public interest in the kind of work we have done. So I have quite a bit of this public outreach. That does not form my primary focus, but it's something worth giving, so to speak. And also we are in such positions that we ought to be mindful. We want to promote our science to the general society both for the benefit of society and also for the benefit of our science.

01-00:51:32
Burnett: Absolutely. And you also then have fairly recently made another career transition. Can you talk about your move from the Carnegie Museum of Natural History?

01-00:51:47
Luo: I was happy at Carnegie Museum of Natural History. For quite some time I was the associate director of the museum in charge of science research and collections. It's just out of the blue, that the University of Chicago just made an offer to me to become a professor at the University of Chicago. And I had sixteen very productive years at the Carnegie Museum. I'm into that age. And if I want to move at all this will be my time, so I moved. Now, I've been with University of Chicago for a little bit over three years now.

01-00:50:35
Burnett: And you're doing teaching and research and are you connected at all with the collections there?

01-00:52:42
Luo: University of Chicago does not have a museum. We are, at University of Chicago and also in appropriate fields, receiving this scientific associate or research associate joint appointment with the Field Museum of Natural History, which is just four or five miles from the University of Chicago. So I have adjunct affiliation at the museum. This is, in a sense, a continuation of my former Museum affiliation. Most of my work really having to do with research, and secondarily for teaching. I enjoy teaching, so I guess I'm just lucky. When people feel good about themselves, they get paid to do what they want to do. So I can characterize my job, both at the Carnegie Museum and at the University of Chicago in that way.

01-00:52:44
Burnett: Absolutely. Before we finish, we've talked about some of the big changes in paleontology, research, and the evolutionary biology implications of new discoveries and new kinds of analysis. Are there other big changes in scientific practice in paleontology that you would like to speak about that have happened in recent years, in the last 15 or so years?

01-00:54:11
Luo: In the last fifteen or so years some of the biggest changes are molecular biology and the understanding of development with the development genetics came to impact morphological science. And, of course, a lot of those developmental studies are about mammals because we want to cure the human diseases. And many of those research results are so scintillating we can potentially incorporate the developmental biology hypothesis as a way to inform the morphological evolution of mammals. So I would rank the general field of evo-devo, evolutionary developmental biology, as one of the new and most exciting emerging fields that really galvanized the morphological evolutionary study of fossils. So I like that. The other aspect is really we start to see so many different ecological specializations in early mammals. It's not that we previously ignored it. It's just that the earlier fossils were not good enough for us to establish this level of scientific inference - but now we have it. So there's integration of the mammalian morphological evolution with the paleoecological study is also another very important development.

01-00:55:59
Burnett: Studying the structure to understand diet or—

01-00:56:04
Luo: Locomotor function, how a particular species exploited its own niches. And then when you've got enough of an understanding of diverse species, how they respectively explored their niches, and then you have a full understanding of how morphological evolution in a broader context, beyond just mapping morphological transformation in and of itself. That's of value, but with a paleoecological context, with an evolutionary developmental biology insight, the picture would become a whole lot richer.

01-00:56:50
Burnett: Dr. Luo, I want to thank you very much for taking the time to sit and speak with us.

01-00:56:53
Luo: Wonderful. I'm happy to have a chance to chat.

[End of Interview]

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The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology:

Charles Marshall, Curator and Professor, University of California Museum of Paleontology

Interviews conducted by
Paul Burnett
in 2016

Since 1954 the Oral History Center of the Bancroft Library, formerly the Regional Oral History Office, has been interviewing leading participants in or well-placed witnesses to major events in the development of Northern California, the West, and the nation. Oral History is a method of collecting historical information through tape-recorded interviews between a narrator with firsthand knowledge of historically significant events and a well-informed interviewer, with the goal of preserving substantive additions to the historical record. The tape recording is transcribed, lightly edited for continuity and clarity, and reviewed by the interviewee. The corrected manuscript is bound with photographs and illustrative materials and placed in The Bancroft Library at the University of California, Berkeley, and in other research collections for scholarly use. Because it is primary material, oral history is not intended to present the final, verified, or complete narrative of events. It is a spoken account, offered by the interviewee in response to questioning, and as such it is reflective, partisan, deeply involved, and irreplaceable.

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It is recommended that this oral history be cited as follows:

Charles Marshall "Charles Marshall: The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology" conducted by Paul Burnett interviewer in 2015, Oral History Center of the Bancroft Library, The Bancroft Library, University of California, Berkeley, 2017.

Charles Marshall

Dr. Charles R. Marshall is Curator of the University of California Museum of Paleontology and Professor in the Department of Integrative Biology at UC Berkeley. His research concerns the integration of paleontological and molecular phylogenetic data to ask new questions about the history of life.

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Interview 1: November 11, 2016
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01-00:00:14
Burnett: This is Paul Burnett interviewing Dr. Charles Marshall for the Bill Clemens UCMP Oral History Project. It's Friday, November 11, 2016. And we're here at the Valley Life Sciences Building at UC Berkeley. Dr. Marshall, welcome. I wanted to start in this abbreviated life history with a question about your background, where you were born, where you grew up. Can you tell me a little bit about your family background?

01-00:00:43
Marshall: So I was born in Canberra, Australia, the nation's capital. A tiny town at the time. I also went to the Australian National University in Canberra.

01-00:00:51
Burnett: What year were you born?

01-00:00:54
Marshall: Sixty-one.

01-00:00:54
Burnett: Sixty-one, okay. And can you tell me a little bit about when you first became interested in science?

01-00:01:02
Marshall: Yeah, that's pretty easy. I'm a dinosaur addict. So, at four or five, *The How and Why Book of Dinosaurs* was key. And we spent a year in the US when I was three-and-a-half to four-and-a-half and Sinclair Oil made a handbook where you could make a little diorama. My father helped me out, you put it up on books, and used little magnets underneath to move the dinosaurs around inside. I remember staring into this world. Of course, it had volcanoes and palm trees and other things that don't belong, but I was completely mesmerized. I understood that the world has been completely different and I understood that the only way to know it was through science, through the fossil record. And so I became addicted to both paleontology and science—I can remember that moment quite vividly even now. I was probably about four-and-a-half.

01-00:01:51
Burnett: Yeah. So even at an early age science came to mean looking at both direct but mostly indirect evidence at what happened or happens in the world?

01-00:02:06
Marshall: Yes, but my focus was on the vividness and reality of those past worlds. And so I wasn't really interested in fossils for their own sake. I was incredibly fascinated by worlds past and life past.

01-00:02:20
Burnett: Yeah. It inspires the imagination, especially for young children, I think. I recall being fascinated by dinosaurs, as well, and I think many kids are addicted to it, too.

01-00:02:28
Marshall: Yes. And I got a real shock when I was an undergraduate. I used to do my photocopying in the maths department because it was cheaper. I did a lot of mathematics. And I was photocopying trilobites for a project I was doing and I've got a math friend who came over and he recoiled in horror, like, "Ew, dead things!" They looked like cockroaches to him. And it occurred to me I'd never seen fossils as dead things before. I'd always seen them as living things.

01-00:03:01
Burnett: [laughter] So unlike a lot of kids who grew up—the common denominator for a lot of the interviews we've been doing is that the scientists persist and pursue these passions and that the passions change. But you nevertheless continued on and studied at Canberra?

01-00:03:26
Marshall: Yes.

01-00:03:28
Burnett: And that's in the early 1980s. And you got a bachelor's in math, paleontology, and zoology.

01-00:03:36
Marshall: Right.

01-00:03:37
Burnett: So your interest in science, I imagine, changes, but paleontology and the study of that rich past is changing at that time. Can you set the scene a little bit about what was changing in paleontological research?

01-00:03:53
Marshall: In my undergraduate years?

01-00:03:55
Burnett: In your undergraduate years, yeah.

01-00:03:57
Marshall: Yes. So what happened is by the time I was ten or twelve I kind of got bored with paleontology. I'd read all the books that had been written, (jokingly) all three of them, by E.H. Colbert, and then I ran into a BBC documentary on astronomy and it's the same thing: fantastical things in deep, deep time. So I actually went to undergraduate to be an astronomer but I thought, "Let's try geology and paleontology again." In high school we had a creationist teacher. Put me off completely. Actually, I didn't take it. And so I said, "Let's try paleontology now at an undergraduate level." And the instruction was so good

and the discipline had come so far. And part of it was the foundation of *Paleobiology*. Yes, I'm pointing—

01-00:04:42
Burnett: Oh, you're pointing to the journal back there. Yeah.

01-00:04:44
Marshall: Yeah. And so for the first time, larger-scale synthetic studies became possible. There was biomechanics, and molecular phylogenies already, but now synoptic analyses of what the fossil record means. And so that really captured my attention, on top of just purely empirical paleontological analysis. We had tremendous labs in second-year geology, a three-hour and a two-hour lab a week, always drawing, always drawing, with sixteen-week semesters. As a student I had a lot more hands-on drawing experience of fossils than most US students ever had.

01-00:05:24
Burnett: Wow.

01-00:05:25
Marshall: And in first year we had eight three-hour labs, drawing archaeocyathids, trilobites, ontogenies of various groups.

01-00:05:33
Burnett: Did that have an impact on you, that kind of tactile approach?

01-00:05:37
Marshall: Yes. I'm sort of known as an analyst or a theorist but at heart I feel myself to be a morphologist and just constantly drawing the material, the actual physical form is what delights my eye. And so I did zoology because I didn't feel I knew enough about vertebrate skeletons. And, again, we had six-hour labs where, again, we were drawing, doing dissections, and always drawing. And when you draw, you learn. You see. You don't really see properly if you're not drawing. So actually I have a very rich foundation in observational paleontology. I also spent eight months as a preparator, preparing fossil lungfish from the Gogo Formation with acid preps.

01-00:06:20
Burnett: Wow. Do you think there's something about the almost kinesthetic learning? Actually there's this physical involvement and the slowness of it allows you to observe more closely and meticulously?

01-00:06:34
Marshall: Yes. Maybe the act of drawing slows you enough. But it also focuses you point-by-point-by-point. And so you sort of grasp the whole by the individual parts because pen has to go to paper. Yeah, and I still draw a lot. I still write a lot by hand on pad. Most of my family are very artistic. I'm the least artistic of all of them. So my sister's an artist. My father's was artistic, and was always making technical drawings. My brother's a carpenter. So there was a lot of tactility. I'm the least tactile of the family.

01-00:07:11
Burnett: Tactile and visual, it seems, as well, in terms of an orie—

01-00:07:14
Marshall: We're all deeply visual. Yeah.

01-00:07:15
Burnett: Right. And so you complete your BA—is it called a bachelor of arts?

01-00:07:23
Marshall: No, it's bachelors of science.

01-00:07:24
Burnett: Bachelor of science in 1984 in Australia and then you cross over to the University of Chicago.

01-00:07:29
Marshall: Yes.

01-00:07:30
Burnett: Did you apply to a number of different places or—

01-00:07:33
Marshall: [laughter] That's pretty funny. So it was in '83 that I finished my bachelor's degree and then the question is what do I do next. And I kind of liked this research thing. Remember, it was the days before—there's no internet. There's no e-mail. Everything had to be done by snail mail. And I was unbelievably naïve. So I thought, "Well, if the University of Chicago, where I went, would accept me, they all would, and if they rejected me, they all would." The Australian universities are mostly government and it's kind of like the University of California – they're pretty uniform in the way they operate, in their standards. So I applied to just one place, the University of Chicago.

01-00:08:06
Burnett: Wow.

01-00:08:06
Marshall: The application was expensive and I was writing my honor's thesis. It's a whole year for us in Australia, an extra year on top of the bachelor's degree, and I was finishing up my work on fossil lungfish. And with the thought of this horrendous application process, I said, "I'm only going to do one of these. To hell with it." So they gave me a fellowship at Chicago and it was really funny. They invited me to their office and asked, "So where else did you apply? Harvard, Yale, Princeton?" And I said, "Oh, no. Just Chicago." And they all laughed, enjoying my sense of humor, and said, "Oh, no. Seriously, where else did you apply?" And I said, "Just Chicago." And you should have seen their faces. They were clearly thinking, "Oh, no, we've just wasted one of our prestigious fellowships on this country bumpkin." [laughter]

01-00:08:44
Burnett: Oh, no.

01-00:08:46
Marshall: [laughter] I could see it in their faces. They regretted having awarded me this four-year fellowship.

01-00:08:51
Burnett: Was it the attraction to the Field Museum? Was the reputation of Chicago such that it attracted your interest?

01-00:08:58
Marshall: There were two things about Chicago. One of them, was that it was the seat of a new approach, the paleobiological approach of analytic questions in paleobiology. The second one is I didn't have any idea of what I really wanted to do and I looked at the faculty list and it was Jim Hopson, Len Radinsky, Lombard, Van Valen, Raup, Sepkoski, LaBarbara. It just went on and on. I thought, "I don't have to know ahead of time what I want to do. I can go there and I can shop around." I knew I needed to do more classes. So as I'm doing the classes I thought, "And I can shape my intellectual direction in my own way at a place like Chicago." And that was really the draw of the place. And then, of course, the Field Museum was part of that calculation.

01-00:09:41
Burnett: Right, right. And did you eventually get attracted to a particular mind there, a particular approach that drew you in?

01-00:09:47
Marshall: I had met David Raup. He had come out to Australia to give talks on the bolide impact with Alvarez. He was recommended to me by my undergraduate mentor, who's well-known internationally, K.S.W. Campbell; most people knew him. I'd done my honors work on fossil lungfish. Chicago was unusual in the sense that the faculty refused to identify any of us as 'belonging' to them. They viewed us as junior colleagues who were like free electrons in a metal, free to move among the faculty, the metaphorical nuclei. And so when I interviewed with Dave Raup in Australia he said to me, "Hmm, you've got a math, physics, chemistry, zoology background. You might make a hell of a geochemist. We have Bob Aller at Chicago. He might be wonderful to work with." And I understood exactly what he was saying, "I'm not going to own you. You're not going to own me. Your job is to make the most of the Chicago environment and perhaps think in ways that you've never thought before, nor anyone else." And so that also was enormously attractive.

01-00:10:47
Burnett: Yeah. That sounds like a wonderful environment in which to flourish. And so you eventually became attracted to a kind of particular field, I guess.

01-00:11:03
Marshall: So I've always been a little unusual as a paleontologist because I approach my paleontology kind of like a physical scientist and so I identify problems to

work on, rather than taxa or time periods. And so a question for me was how to compensate for the incompleteness of the fossil record? Molecular clocks were going really strongly but no one had actually put any error bars on the dating. And I was also interested in molecular versus morphological phylogenies. Which was right? And so I thought, "I want to pick a group that I can do DNA on that has few enough taxa that I can kind of keep them in my head." Gastropods and bivalves are too big for me. The group had to have a rich fossil record. And a complex morphology so there's morphology to work with. And with a North American focused geography so I can have easy access to the material., with shallow enough divergence time that the only good DNA technique of the day could be used. And I preferred a group that appealed to me. And that group turned out to be sand dollars. I made a checklist and sand dollars were the group—I'd worked on echinoderms before, as well, which was a plus.

01-00:12:04
Burnett: I'm trying to remember when is the first time that—because there's an a-ha moment when the molecular dating of organisms reveals that the earliest known fossil is much later than the clock would indicate. I'm trying to recall who that was or when, but roughly when does that sort of — almost a clash, I suppose, between—there's the fossil data and then there's this molecular dating that reveals a striking contrast.

01-00:12:49
Marshall: So initially, of course, the fossil record is actually used to calibrate the [phylogenetic] trees and if the fossil's too young the tree is calibrated too young. So in that sense there was no clash whatsoever.

01-00:13:00
Burnett: Oh, it's kind of a tautology.

01-00:13:03
Marshall: So that's initially. And there's the work done here at Berkeley, actually, with Allan Wilson and Vince Sarich with the origin of humans, where they showed that humans diverged from the other great apes in the order of five million years ago, not fifteen million years ago. Stunning discovery. And that we were placed up with chimp and gorilla and not below chimp, gorilla, and orangutan. Then people started to look at the origin of mammalian orders. And it is interesting because the early molecular clock people understood the fossil record is incomplete. And I talked to some of those guys when I did my molecular training at National Institutes of Health, where they said, "Well, surely, okay, the mammals originate mostly just after the end of Cretaceous. But the fossil record's incomplete and it might be hard to recognize their ancestors. So it wouldn't surprise us if the last common ancestor of all living mammals lived, say, eighty million years ago." So Morris Goodman and people like Raoul Benveniste, and Stephen O'Brien actually used eighty million years for a long time. So I think they're actually quite sensible about it. But it was just sort of—

01-00:14:05
Burnett: Ballpark.

01-00:14:06
Marshall: Ballpark's the word I'm looking for. It was just like I've got to pick a number but the earliest fossil seems a little unreasonable to me. And then it got more controversial as more molecular clock studies came in. Birds and mammals all originating in the Mesozoic and yet the fossil record seemed to show their origin after the Mesozoic. And then angiosperms way, way deep in the Triassic, maybe the Permian. Some of the molecular clock dates looked sort of plausible and some of them looked not even vaguely plausible. And it got worse and worse with people publishing dates for origin of land plants in the latest pre-Cambrian. Now that seems insane. And so then a schism sort of opened up as more studies came in with very different results.

01-00:14:47
Burnett: And roughly when does that schism open up, would you say?

01-00:14:52
Marshall: I would say it opened properly, is that the word, in maybe the early nineties. Yes, that's my sense of when it got most—

01-00:15:06
Burnett: Vibrant or—

01-00:15:10
Marshall: Yeah, most obvious that there was a problem here.

01-00:15:11
Burnett: Yeah, yeah. And so you're thinking about this as an epistemological problem. How can we know this with some certainty?

01-00:15:22
Marshall: Right. So for my PhD I was trying to date the time of origin of my sand dollars. And the last chapter of my thesis I still have not published. My molecular clock indicated a very shallow divergence of the sand dollars. But if that was correct the rate of evolution was like a hundred times humans and ten times regular sea urchins, which seemed daft. And then I started to piece together that the problem with the fossil record of sand dollars; it's not just the gappiness of the sand dollar fossil record itself, there's the gappiness of the rock record and then they were phylogenetically hard-to-place fossils, that if they were in, would double the estimated age and if they were out, wouldn't. And then it looked like there were geographic gaps because all the sister groups were in South America and all of my fossils are in North America. So I think the fossil record was telling me when they migrated into North America, not when they diverged from their common ancestors in South America. And so I thought, "Oh, this is awful. The calibration problem is really, really hard." And so I switched then, at about the time I began as an Assistant Professor because I thought confidence intervals in stratigraphic ranges will work better for mass extinctions. If origins are times of small populations in remote places

with no rock, it will be hard to get a measure of the time of origin. But if you've got a taxon that's widespread and fossilizes well and there was a genuine mass extinction, then its disappearance anywhere is a good reflection of its disappearance most everywhere. So I can go to the places where the fossil record is good, and use that information as a good proxy for reality.

01-00:16:59
Burnett: It's a baseline.

01-00:17:01
Marshall: And so I switched to thinking about using confidence intervals in terms of assessing the suddenness (or not) of mass extinctions and gave up the origins thing for a while. It was just too hard.

01-00:17:14
Burnett: So that became your dissertation?

01-00:17:18
Marshall: The dissertation was the origins of the sand dollars and the bench DNA work to measure the similarities of their genomes.

01-00:17:25
Burnett: Right. And by the early nineties you're developing this further to think about—

01-00:17:29
Marshall: So early nineties I was at UCLA. I was developing the extinction component and doing a lot more with molecular phylogenies and very interested in the topology of phylogenies, molecules versus morphology. Well, I started that with my PhD, through my post-doc and early Assistant Professor years at UCLA.

01-00:17:50
Burnett: Your PhD is in evolutionary biology and there's this work that you're focusing on in the early nineties. I'm very curious about this Dollo's Law paper. And Dollo's Law, the basic principle is that evolution is not reversible and, in fact, you're looking at the ways in which there's evidence for the reactivation of dormant genes, if I have it, in a ballpark, correct. So can you talk a little bit about that research and how that influenced you or shaped you?

01-00:18:40
Marshall: Yeah. So this is what happened, Steve Gould published a paper while I was an undergraduate. It was probably like '83. One of his essays was on hen's teeth and horses tail. And the hen's teeth was reporting on some experiments where it looked like people had reactivated tooth development in birds that haven't had teeth in the order of sixty, seventy, eighty million years. And so you go, "Wow, how can that happen?" The genes are still just sitting there ready to be activated.? Now, we had a rule in Australia with Steve Gould. If you read his essays, you waited three days and then you started to think about it critically, on the grounds that he wrote so well he could probably convince you that your

grandmother was an axe murderer. We're Australians after all. So let's get over the verbiage, which was so compelling, and think about the content. So, jumping forward more than a decade I was a post-doc in Rudy Raff's evo-devo [evolutionary-developmental biology] lab. He'd given a talk on the sea urchin he works on where it had gone from developing this complex pluteus larvae, which then metamorphizes into an adult sea urchin, to direct development pretty much going straight from egg to an adult sea urchin. And he'd given a talk at Michigan State and one of the grad students asked, "Well, how do you know evolution isn't going the other way? How do you know it's not re-evolving the pluteus as opposed to the pluteus being lost in the direct developers?" Now, Rudy being Rudy, unlike a lot of people, entertained the impossible and he asked me when he got back, could we work out the probability of actually re-evolving a pluteus? And we had a morning discussion and I realized I could do it (sort of). And so with his wife, a *Drosophila* geneticist, I was able to write down an equation for what that would look like, reactivation. And so the question is if you took a gene and stole its promotors, its on switch, and let it sit for *x*-million years and then gave it its switches back again, what's the probability that gene would still function? For example, the enamelin gene that might be used for hen's teeth. And so I worked out how to solve that.

And it turns out the genes don't last very long. Short genes in slow evolving organisms that don't have much constraint, yeah, maybe after two or three million years there's a half chance that it's still working. Long genes with high constraint in fast-evolving species like *Drosophila*, hundred thousand years and they are shredded, via point mutations, insertions and deletions. Eighty million years for the hen's tooth, I don't think so. So we argued that it couldn't have happened. Someone had looked for enamelin in the chick genome and hadn't found it. And then it occurred to me a really stupid mistake that they had made and that Steve had missed. The figure that everybody has of the hen's tooth is a *molar*. What's a chick doing producing a *molar*? If I'm activating the chick genes by using mouse mesenchyme, yes, it's a mouse signal but it should be producing a dinosaur-looking tooth, not a molar. It's got to be contamination. And it was later shown to be contamination and there was a brilliant post-doc doing in Rudy's lab doing similar experiments on *Xenopus* showing that contamination affected a lot of these early embryological experiments. So it was really fun.

01-00:22:05
Burnett: That's fascinating.

01-00:22:06
Marshall: The title's of the paper is brilliant. Not my creation. It was Rudy Raff's. "On Dollo's Law and the Death and Resurrection of Genes."

01-00:22:12
Burnett: But there are these kind of ur-genes, these highly conserved genes that are a part of it. So are there examples that could push that limit?

01-00:22:28
Marshall: Right. So the question is if the gene served some other function then they can be retained for a long time. And if innovation is not about new genes, and most of it isn't, it's about rewiring, maybe you could rewire them again in the right way to reproduce a structure. So that's where the hen's tooth is interesting, because it involves the gene enamelin, which isn't used for anything else. So there's a classic paper I was going to publish at UCLA, which I didn't. Dave Hillis and colleagues had published a paper showing that the rhodopsin gene, a light receptor protein in cave crawdads— yabbies to us back home—crawdads, was more conserved than neutral drift. And yet they're in the pitch black. They've got no reason to have rhodopsin. And so I thought, "Oh, that's odd. I bet you've got the date of the caves wrong." Caves usually are very young. If the caves are very, very old then you'd expect more differences to accumulate, and Hillis had measured lots of differences. They didn't have many differences. I double-checked it. No, they got the age of the caves right. So then I went to FlyBase and looked up rhodopsin and it turns out rhodopsin is necessary for the formation of the optic cup and the optic cup is necessary for the formation of the forebrain. No rhodopsin; no forebrain. Ah! So rhodopsin is conserved genetically so the brain develops even if it's not conserved from the point of view of light reception. And so that retards the rate at which rhodopsin decays. So then genes can be held longer than you might expect based on the simple model that I developed from the Dollo's Law paper. Yeah. So it's really complicated and interesting.

01-00:24:08
Burnett: [laughter] Yes, it is. Absolutely. Let's dive into the science as needed, but I also want to track your career. So we've already ended up with you at UCLA. So you are hired as an assistant professor in Earth and Space Sciences in 1989.

01-00:24:29
Marshall: Yes.

01-00:24:30
Burnett: And you're producing these papers, like the Dollo's Law paper, and you make Associate in '93.

01-00:24:41
Marshall: Yes. What happened is in '89 I went to do my post-doc with Rudy.

01-00:24:45
Burnett: That's right, that's right.

01-00:24:46
Marshall: And so UCLA gave me two years off, which is great. So I arrived at UCLA in '91 and then, for better or worse, they gave me tenure after two years. [laughter] That was kind of a shock. Yeah.

01-00:24:58
Burnett: Well, they wanted to keep you, obviously, right?

01-00:25:00
Marshall: Yes. I think that was what was going on. Yeah.

01-00:25:02
Burnett: And so in the nineties, what is captivating to you in terms of research programs once the Dollo's Law paper is done? What's drawing you?

01-00:25:20
Marshall: The Dollo's was kind of a freebie, which is, "Oh, I think I know how to solve this and it looks really interesting. Let's do it." I was interested mostly in identifying whether or not mass extinctions were sudden or not, such as the end Cretaceous. And I was interested in molecules versus morphology in terms of the topology of phylogenies. I can talk about that if you'd like.

01-00:25:44
Burnett: Yeah, yeah.

01-00:25:44
Marshall: So for my sand dollars the molecular tree differed from the morphological tree and I was able to convince myself that the morphology was fooling us and the molecules were right. But I looked into molecular phylogenies of amniotes, birds, mammals, crocs, turtles, and I was able to convince myself with some analytic tools that the molecules were wrong and the morphology was right. And then more molecular data that was coming in, starting to flood in, which started to support the morphology after all. And so I realized that either morphology or molecules could be right or be wrong. It depends on the case, it depends on how close the branches points are in real time. So having a paleontological perspective is critical, because you need that temporal perspective.

01-00:26:24
Burnett: That's a huge challenge because what you identified earlier on at the beginning of this conversation is this absence of a baseline, right. If you're using the fossil record to construct the phylogenetic trees there's this kind of tautology that can be conserved. So you're using one to interrogate the other. But if you don't have a baseline, what helps you to determine that that was the reverse case?

01-00:27:00
Marshall: Yeah. So in the case of the sand dollars, I developed a bootstrapping technique that established the DNA topology really looked good. It was DNA-DNA hybridization, which is crude, but a 1 percent difference in the genome is about 10 million base pairs. It's a lot. It's hard to see how it could be wrong. But when I looked at the morphological tree, the only node that was brought in conflict by the DNA had the least morphological support in terms of number of characters. And those characters were really iffy: whether some of the spines were a little bit bent or not bent and they were intermediates in some of the taxa. There was a fluid filled sac on the end of another type of spine. But sometimes there was just a thickened epithelium. Well, is that a

separate character, or just a reduced sac? So I was able to show that the weakest part of the tree morphologically was the only part that was challenged by the molecules.

For the amniotes what happened is I actually photocopied all of the sequences and went through each base by hand one-by-one and I found this incredible bias in the sequences. It was dominated by T to C changes, lots and lots of them, and monophyletic birds, lot of T to C changes. Monophyletic mammals, lots of T to C changes. Really long branches, lots of T to C changes supporting bird and mammal as each other's closest relatives, which makes no sense paleontologically. So then I went through and there's only one A to T change in the whole data set. That supported the correct tree, the morphological one. And so then I used a weighted-parsimony program that Walter Fitch developed and showed if you down weight sites that occur frequently and upweight sites that occur rarely you've got the paleontological tree with the DNA data.

01-00:28:47
Burnett: Wow.

01-00:28:47
Marshall: So it's by developing procedures. There's a signature of what's wrong in the molecular tree alone and that starts to convince people that you've discovered something. So I'm not conditioning now on the morphology, which was heartfelt, sort of has to be right compared to the molecules, but there should be a signature in the molecules themselves. So the same with the morphology. If the morphology is wrong there should be a signature in the morphology that it's wrong. If there's something wrong with the molecules there should be a signature in the molecules that it's wrong. And so it was developing that sort of analytic way of thinking that led me into how to resolve that problem.

01-00:29:24
Burnett: Right, right. And if you identify a bias then you filter for it, you control for it, and then you proceed from there.

01-00:29:34
Marshall: Right, exactly. And so then what I did then with the molecular tree for the sand dollars that was putting two groups together, actually the California *Dendraster* with another species, that didn't make a lot of sense. But then I put an undergraduate on it and we found some characters that supported the DNA. That's kind of cheating, but I believe they're sister groups now – there was morphology which had been missed before. And what had happened is one of the so-called out groups had bounced high in the molecular tree. And I think what had happened, is that it had become secondarily reduced. And when things are secondarily reduced they fall to the bottom of the tree. Cladistics is biased in the sense that if it doesn't know where something goes, it usually goes to the bottom. It goes to the *bottom*. Because I haven't got any evidence to put it anywhere else. And so, ah-ha! But most of the time when

morphology is wrong it's because something has become so highly specialized you can't see where it goes. That was the problem of placing birds for a long time. Or it falls to the bottom. And so I started to develop an intuition for when parsimony gets fooled with morphology.

01-00:30:37
Burnett: There's this incredible lip service paid to interdisciplinarity. You hear it all the time and people use it in their marketing, right.

01-00:30:48
Marshall: Yeah, right.

01-00:30:48
Burnett: And it's like, "thinking outside the box." But what you hear in stories of good science is, over and over, that people are bringing training in one domain, an awareness of problems in one scientific domain, and they're thinking critically about how that applies in another domain. It seems to be a feature of your career, that you've had training in multiple domains. You're a polymath and you're interested in all these different facets. But that's where you get the questions, it seems.

01-00:31:26
Marshall: Yes. And to me it's sort of a matter of primary commitment. My favorite philosopher of science is a fellow called [Michael] Polanyi and he talks about—what's it called? It's called personal knowledge. And he emphasizes the role of personal knowledge rather than objectivity. He defines objectivity actually as making an object of what you know. I know where I live. You don't. So in this scientific world of two, that is not objective. I draw a map. I give it to you. I've made my knowledge an object and then we can set up a test. You get to my home, let yourself in, and describe something that I know is true. So now I've made what I know objective. So that to me is what objectivity is, not about truth *per se*, but about transferring my knowledge in a way that doesn't involve me. His argument is, though, is that ultimately truth is determined by the personal, by your commitment. And so what I find when you get these debates opening up, you get people who are *only* committed to the morphology, so they kind of know the molecules have to be wrong or they're *only* committed to the molecules, so they kind of know the morphology has to be wrong. So the question is where's your primary commitment? Is it to your tool? Is it to your comfort zone? Or is it to the problem? What are the relationships between the major groups of vertebrate animals and how did that happen? And so I think Polanyi captures that in a way that, say, no other philosopher of science does. I don't know whether that helped shape me or whether it just resonated with me when I read it as a grad student at Chicago.

01-00:33:00
Burnett: Right, right. And it seems that there is such an enthusiasm during this time, the revelation of biases in the sciences, confirmation biases of all kinds.

01-00:33:15
Marshall: Yes. And even earlier, the Kuhnian Revolution, Thomas Kuhn, paradigms, and recognizing that knowledge is often encapsulated within a framework and that often big changes occur when the framework moves. Now, again, Chicago was amazing that way under Raup. The first course we did with him was really pretty funny. We went to his house, the beer came out and he said, "Okay, this is the University of Chicago, one of the world's greatest institutions, a thousand A.D. We can congratulate ourselves on all the things we know. Run the clock forward a thousand years. How many of those wonderful things we knew are going to turn out to be wrong?" And now the point – he asked "What makes you think it's any different now?" And so the question is what are the things we think are true that one might suspect in the long run will turn out not to be true? And then what to read, what courses to take? Raup's approach, Chicago's approach, was different from most. Why should they start us graduate students off in the rut that they're already in? So he said, "We can set you a whole bunch of courses to take and a whole bunch of readings to do but why should we start you off in a rut that we're already in and don't recognize anymore? So we're not going to set you any required readings and no required courses. Your job is to just go explore." And then he paused and said, "Well, you'd better read Darwin." [laughter] Fair enough. So the attitude was one of full openness. And in some respects, I realized what made Raup great was the empowerment that gave us implicitly, that we, being fresh and new, have the potential to find those chinks in the armor that more established people don't recognize anymore. This was very different from my decade at Harvard, such a disappointment, where everybody was supposed to sit under the greatness of the Harvard professor.

01-00:35:07
Burnett: And a kind of burden and responsibility, too.

01-00:35:11
Marshall: We didn't experience it that way. It was very free and light. It wasn't like anything rested or hinged upon it. So there was no heaviness to it.

01-00:35:18
Burnett: Right, right. Explore and fall down and get back up and keep going.

01-00:35:35
Marshall: So I wanted to do the molecular stuff very deeply and I prepared this big pitch for why I should do this and so I went into his office in my first year. He said, simply, before I got going, "Hmm, good idea." I was so disappointed. I wanted to fight for this. [laughter]

01-00:35:42
Burnett: You were ready to go.

01-00:35:42
Marshall: [laughter] Now, Tom Schopf had passed away just before I'd got there, and had already started this. So in some respects Tom had broken that ground and I was following it, as it turned out.

01-00:35:51
Burnett: Wow. And you didn't even know that that was—

01-00:35:54
Marshall: Oh, I did. That's part of the reason I went to Chicago, is because of Tom. And then he passed away in April, I think, and I got there in August.

01-00:36:02
Burnett: Oh, my God.

01-00:35:02
Marshall: At age forty-eight or something. Yeah.

01-00:36:05
Burnett: That was a meaningful continuance of that research, not only epistemologically and—

01-00:36:15
Marshall: Yeah, so it's sort of—anyway, so that's—

01-00:36:15
Burnett: Yeah. I was struck in your 2003 paper, the nomothetism, I don't know if I've pronounced that correctly, but on the Cambrian explosion. It sounds like Gould was a bit of a touchstone for many of you.

01-00:36:36
Marshall: Yes.

01-00:36:37
Burnett: Touchstone, and also a point of friction maybe.

01-00:36:41
Marshall: Right. So Steve wrote this paper, I think it was the fifteenth year, I don't remember now, anniversary volume of *Paleobiology*, in about '86. Gould had a paper on a nomothetic versus ideographic paleobiology. And in some respects that's why I went to Chicago – what are the rules or laws, or to what extent are there rules and laws that affect the unfolding of life. Is it just a spectacularly unique history or are there actually generalities? And in some respects that's what I was interested in discovering. Incidentally, I felt Steve cheated completely when he wrote *Wonderful Life*,[2] where his nomothetic law was contingency. What a cop-out! Dumb luck. Chance. If *Pikaia* had gone extinct, then no us. Now, there's no question that I think that chance plays a major role but Steve gave up on nomothetism. My feeling about this was quite

[2] Stephen J. Gould, *Wonderful Life: The Burgess Shale and the Nature of History*, (New York: Norton, 1989).

clear. Let's look for the generalities. Let's run the risk of the statements of generality being too strong; let's be forced then to find the correct balance between the generalities that do exist and unique idiosyncratic events that account for the history of life? The end-of-Cretaceous mass extinction is clearly one of these idiosyncratic events. All right. Maybe no us if dinosaurs are still there. I don't know how it all unfolds. So there's no question that contingency matters. But don't give up on the nomothetic agenda. I thought Steve gave up on it completely. So the Cambrian explosion papers were an attempt on my part to find the nomothetic. At the time there were really daft ideas with the snowball Earths coming out of Harvard/MIT when I was there, that they *caused* the Cambrian explosion, which I thought was ridiculous. How could a major catastrophe generate a new level of organization? And Andy Knoll and Sean Carroll at the time had suggested that the Cambrian explosion was like the K/Pg extinction event. At the K/T extinction event, watch how the extinction of the dinosaurs opened up the space for mammals. So mammals were in the shadow of the dinosaurs. So Andy in a review with Sean Carroll, using the K/Pg as an analogy, argued that the extinction at the end of the Ediacaran opened up the ecological space for the Metazoans. Really, Andy? [Getting very animated] Do you *really* think that *Dickinsonia* and *Spriggina*, these little blobs with no appendages and no knobs and bobs on them, were keeping *Anomalocaris* down? I don't think so! So something fundamentally different happened. The K/Pg transition and the Cambrian explosion are not analogous. But then I realized as I confronted the ideas of the MIT/Harvard astrobiology team, of which I was a member, including John Grotzinger, Sam Bowring, that we had no theory of disparity. We had no theory of why things are different. We had documented really well the emergence of the phyla, but we don't have a theory for the disparity. Now I'd seen some computer simulations that involved developing fitness landscapes by Karl Niklas, a paleobotanist, looking at the origin of plants. And I thought, "Wait a second. If you increase the number of selective pressures, the complexity and number of solutions that a developmental program produces goes up dramatically." I thought, "Oh, golly, that looks like a theory of disparity, at last."

01-00:39:58
Burnett: Right, right. And that's where they're talking about functional tradeoffs, right?

01-00:40:04
Marshall: Yes.

01-00:40:07
Burnett: There are what you call frustrations.

01-00:40:09
Marshall: Yes.

01-00:40:12
Burnett: If an organism is optimized along multiple axes, that *drives* increased complexity.

01-00:40:25
Marshall: Yeah. So the argument is that there's this enormous latent potential in the genome, and I published papers with Jim Valentine on this, where most of the key genes, coming back to one of your earlier questions, look like they predate the differentiation of the phyla. So it's genes like the *Hox* genes that are part of the latent potential. If you challenge that genome in a simple way it produces a simple answer. If you challenge it in a series of complex ways it turns out it has the capacity to respond to those pressures, producing predators, burrowing, fast swimming, big jaws, etc. And so you pull out of the genome that potential for morphological richness quite quickly and that's what is called the Cambrian Explosion. So that was the fundamental idea, which led to the idea of genomic preadaptation that I published with Jim [Valentine].

01-00:41:12
Burnett: Wow.

01-00:41:16
Marshall: Yeah. So a final thing here, which is maybe just showing the sort of hyper-nerd I am. I wondered what controls the number of peaks, the number of optimal solutions. And I had this sense that maybe it 'goes like' the number of selective pressures factorial. Stupid idea. And then three weeks later I had a dream, which was a graph, of the number of peaks in Niklas' fitness landscape as he increased the number of selective pressures and *n* factorial, the number of selective pressures—and they're the same. I published that in 2014. They match quite well, the number of 'best' morphologies and the number of selective pressure factorial. I think it makes sense. If there are five things you have to optimize then there's five factorial ways of prioritizing that. If most of my investment is in reproduction, then no need to worry so much about light interception—so there's basically *n* factorial ways of saying, "Look, I'm going to invest most of it, say, in r-mode, reproduction, and produce lots of offspring and not protect them very well," et cetera, et cetera.

01-00:42:14
Burnett: So this is paleoecology, too, right?

01-00:42:16
Marshall: It's paleoecology, it's functional morphology. It's a lot of different things. I've always resisted names because I find they're binding.

01-00:42:27
Burnett: Yes, yes.

01-00:42:28
Marshall: But, yes, it involves paleoecology. Evolution is variation followed by selection. Doesn't sound very biological. Leigh Van Valen called it the control of development by ecology. I call evolution the filtering of

development by ecology. Development is where the variation comes from; ecology is the arena within which selection operates and these fitness landscapes capture both. There's the developmental potential for new morphologies that can be captured by a morphogenetic landscape that, for example, Dave Polly does so well, and then there's the ecological consequences of the morphologies, their fitness, and that's the height of the peak. A fitness landscape captures the morphogenetic potential and the inferred fitness of the each morphology. So fitness landscapes can capture the filtering of development by ecology in a really nice elegant way. And that's what Niklas had done experimentally in the computer with these plants. I thought, "Good God, he's solved the problem in a sense."

01-00:43:25
Burnett: The filtering of development by ecology. Well, from the dizzying heights of this research and the incredible conversations that you were involved in over these years, I do want to track a little bit—so you then became full professor of evolutionary biology at Harvard from 1999 to 2009.

01-00:43:51
Marshall: Yes, a full professor at UCLA and then transferred to Harvard as a—

01-00:43:54
Burnett: And then transferred. And also during that time in 2000 you become curator of the department of invertebrate paleo at the Museum of Comparative Zoology at Harvard. So that's your experience with museum work. And I'm wondering if you could talk about that as the segue to becoming the director of UCMP.

01-00:44:18
Marshall: Yeah. So I'm not known deeply for my specimen-based work [he says wryly]. Now, Dave Raup prided himself on never having described a fossil, which seemed a little silly to me. And so when I showed up at Chicago Jim Hopson showed me a Mazon Creek lungfish. I'd done my honors work with someone who was deeply field based, as well as large thinker, Ken Campbell back in Australia, and my other mentor, Richard Barwick. And Jim said, "Charles, would you like to look at this, see what it's worth?" And I thought, "You know, that would be fun and it would be nice to actually have a fossil description under my belt." So one of my papers as a grad student is actually a fossil description of this Mazon Creek lungfish. So at Harvard it was fun to sort of take charge of the collection. The collection manager was Fred Collier. It was the invertebrate collection. At Harvard the collections is all divided up. Plants, insects, vertebrates, invertebrates. So I was the curator of the invertebrate collection along with Steve Gould. And then Fred Collier managed the collection. He spent thirty-five years at the Smithsonian and then retired and then had another fifteen years at Harvard.

01-00:45:29
Burnett: Wow. So how did the transition to UCMP come about?

01-00:45:37
Marshall: So the transition at UCMP came: the directorship came open and Berkeley just has an energy and a vitality. And there was a feeling that it could do with an extra dimension. It's always been specimen based and should be. That's the heart of our discipline. Without the description of the fossils, their geological and environmental context, we have nothing. Going back to my diorama that I had when I was four or five. That empirical data is the foundation of it. I'm kind of the analyst or theorist that comes in over the top of that and tries to find the Nomothetic. We also had this incredible—we, I wasn't here yet—the education and outreach program. And so the web presence, the *Understanding Evolution*, the *Understanding Science*. And I'd met Judy Scotchmoor when I was at UCLA. She had come down to a short course that Bill Schopf organized that I was speaking in on the molecular revolutions and evolution. I was really impressed with her and the work that came out of UCMP. And so UCMP looked like it was a place you could do something. It had already done a lot. And then there was the price of moving, of being the Director of UCMP. But to me it wasn't a price at all. There's such an incredible foundation. So when I interviewed they showed me a third of the collection sitting out at the Clark Kerr campus in this derelict building. And they were very reluctant to show it to me because it was a disaster. Broken roof, rain coming through, floor buckling.

01-00:47:24
Burnett: Oh, my goodness.

01-00:47:25
Marshall: You really needed a hazmat suit because of the rat feces on the floor.

01-00:47:27
Burnett: Oh, my God.

01-00:47:30
Marshall: I think I'm good at organizing things. And when I walked in I said, "Oh, this is great. This is a world-class collection that needs rehousing." And so I said to them as part of my setup package, I said, "Those fossils need to be moved somewhere that I can get grants for them. If they stay here, I can't get a cent. You move them somewhere else, I reckon I can get a million dollars' worth of grants over a decade." In my first three years I got half a million dollars to rehouse the USGS collection and since then we've had a number of smaller grants, including in vertebrate paleontology. I like making more of what already exists. That's just one of the things that's built into me. And I thought, "Wow, UCMP, there's so much that can be done here."

01-00:48:17
Burnett: And so you were able, once you came here, you set about that project?

01-00:48:20
Marshall: That was one of the many things I did, yes. So I needed to put a collection improvement grant in. So as part of my setup package, although not assigned

to me directly, the campus put about $110,000 into moving the fossils out to the Regatta Boulevard facility. And Mark Goodwin here is the one who really led that whole charge. And so when I arrived I thought, "I need to pay back the $110,000,", And so one of my first priorities was to write a big collection improvement grant. But, ooh, I don't know the collection; I don't know what it needs. But I knew we had a big slug of USGS fossils that had come over. The cases were dilapidated, the doors had been lost. The archival material was in the collection and open and deteriorating. We needed new cases. I thought, "Great, the grant's limit is half-a-million dollars. Cases are expensive. Ah, I can easily write a grant for roughly half-a-million dollars with the deterioration of the material. And there was no invertebrate collections person. And someone said to me, "Well, then, who's going to write the grant?" I said, "I will. That's my job." And so it got rejected once and then we got it. The overhead on that was about $120,000 so I paid back the campus—my conscience was clean. I made sure I pointed it out—

01-00:49:39
Burnett: Of course.

01-00:49:40
Marshall: —to the appropriate Vice Chancellor.

01-00:49:43
Burnett: Well, so you got here and one of the hallmarks of UCMP and paleontology here is that there is a deep interest in interdisciplinarity. Is that something that also attracted you?

01-00:50:01
Marshall: Ah. I'd forgotten about that. So my PhD is from the Committee on Evolutionary Biology but I was housed in the geology department, where most of us non-vertebrate people were. And then my first job at UCLA was in the geology department. And at Harvard University I was fifty/fifty geology and biology and then I started to switch my primary alliance from geology to evolutionary biology. But I'm interested in the evolution in deep time and that means knowing ecology and evolution. And so the fact that UCMP is embedded in Integrative Biology also is incredibly attractive. It's also the top-ranked evolutionary ecology program in the world. At Harvard there were essentially no ecologists. Here we've got dozens of them and are they good and they're open to answering questions. And so with that embeddedness, and with the other museums here, this place is amazing. Also, at most institutions, the museums struggle for recognition, but here we have standing. We're viewed as a highlight of the campus and that's just fantastic when it comes to fighting for resources.

01-00:51:13
Burnett: And I think you've continued the online presence. It's one of the first websites on the World Wide Web, I understand.

01-00:51:20
Marshall: Yeah. The number right here is in the top fifty. I think it might be less than that. I mean, it might be like top twenty or thirty, including big business, government, everything. We get about seventy million page downloads a year. Seventy million page downloads. It's unbelievable.

01-00:51:41
Burnett: The Oral History Center has some work to do. [laughter]

01-00:51:43
Marshall: Everybody has a lot of work to do. The campus has a lot of work to do. So what we're doing now is making sure we maintain the existing web resources, but we are now developing an *Understanding Global Change* (*UGC*) resource. Actually, it was Kevin Padian, Eugenie Scott at NCSE who went down to the Moore Foundation to suggest an understanding climate change web resource. And I thought it was great except that I thought that climate change isn't exactly our focus, so let's broaden it to global change, particularly biological responses. And the long and short, the Moore Foundation gave us a million dollars based on *Understanding Science*, *Understanding Evolution*. So we're getting pretty close to a launch of *UGC* and we're now talking to Google, HHMI, Moore, a whole bunch of other people, to try to keep that funding coming in.

01-00:52:43
Burnett: Brilliant.

01-00:52:44
Marshall: First we've got a brilliant post-doc here, Jessica Bean. We've come up with an intellectual framework for capturing all the interrelationships of all the major components of global change, from the geology, from the human, from the atmospheric to the oceanic and the teachers are loving it. It's hooked up to the NGSS standards, so you can dial up a standard and pull out the relationships. I think it's going to be incredibly effective. We're really excited about it.

01-00:53:13
Burnett: Brilliant. If we had continued talking about your scientific career, there does seem to be an interest in what you've just described. In 2015 you've got a paper on how stable are food webs in mass extinction events. There's obviously, in terms of beyond paleontology, there's this imperative to understand climate change and that's shaping the research. But you are able to respond and say we can make this a much bigger set of questions and I think that's a really great way to go.

01-00:53:48
Marshall: And provide a resource. So Tony Barnosky wrote me into his South American megafaunal research project, so I've done some modeling on climate/human interaction in the megafaunal extinctions, as well. But I've always been interested, and I don't know where this came from, in what are the sort of infrastructural needs that can facilitate the science, and that's partly what

attracted me to the directorship of UCMP, this incredible depth of collections, this incredible depth of commitment from incredible people. Like Bill Clemens is one of the greats. The web resources, the foundation of the Paleobiology Database and that seven-year effort, seventeen meetings in Santa Barbara to set that up. And so these infrastructural needs are part of that and so to me that's what the museum represents. And Berkeley can do that. Part of Berkeley's mission is to make a difference in the world, where a lot of universities missions are simply to be the best-ranked university. But being the best-ranked university doesn't necessarily mean making a difference to the world. It just means being the most prestigious by pretty narrow metrics. Number of Nobel Prizes. Oh, we happen to be first here anyway. Number of MacArthur award winners. Oh, we happen to be first anyway. Number of National Academy of Science—oh, we happen to be first anyway. Berkeley is top in most of those things. But there's another flavor here, which is to make a difference in the world, and Berkeley and UCMP cares about that and that also drew me very strongly here.

01-00:55:23
Burnett: Yeah. There's almost a civic element to it as the foremost public university, perhaps in the world. There's this interest in what can be done for the big questions of concern globally.

01-00:55:38
Marshall: Yeah, absolutely. And a lot of us feel that fairly intensely. But it means then that people are willing to put more than they're already doing into things that aren't maybe directly related to their immediate career success. And that's been incredibly rewarding here, where in other institutions that is not nearly the case.

01-00:55:57
Burnett: Right, right. It is a different climate. Absolutely.

01-00:56:02
Marshall: Yeah. It fits me well. Yeah.

01-00:56:03
Burnett: Right, right. Dr. Marshall, I want to thank you for taking the time to talk with us about UCMP and the incredible work that you've been doing over the last thirty-five years. [laughter]

01-00:56:15
Marshall: [laughter] Thanks.

01-00:56:16
Burnett: Thank you very much.

01-00:56:17
Marshall: Thank you.

[End of Interview]

Oral History Center
The Bancroft Library

University of California
Berkeley, California

The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology:

Nancy Simmons

Interviews conducted by
Paul Burnett
in 2015

Since 1954 the Oral History Center of the Bancroft Library, formerly the Regional Oral History Office, has been interviewing leading participants in or well-placed witnesses to major events in the development of Northern California, the West, and the nation. Oral History is a method of collecting historical information through tape-recorded interviews between a narrator with firsthand knowledge of historically significant events and a well-informed interviewer, with the goal of preserving substantive additions to the historical record. The tape recording is transcribed, lightly edited for continuity and clarity, and reviewed by the interviewee. The corrected manuscript is bound with photographs and illustrative materials and placed in The Bancroft Library at the University of California, Berkeley, and in other research collections for scholarly use. Because it is primary material, oral history is not intended to present the final, verified, or complete narrative of events. It is a spoken account, offered by the interviewee in response to questioning, and as such it is reflective, partisan, deeply involved, and irreplaceable.

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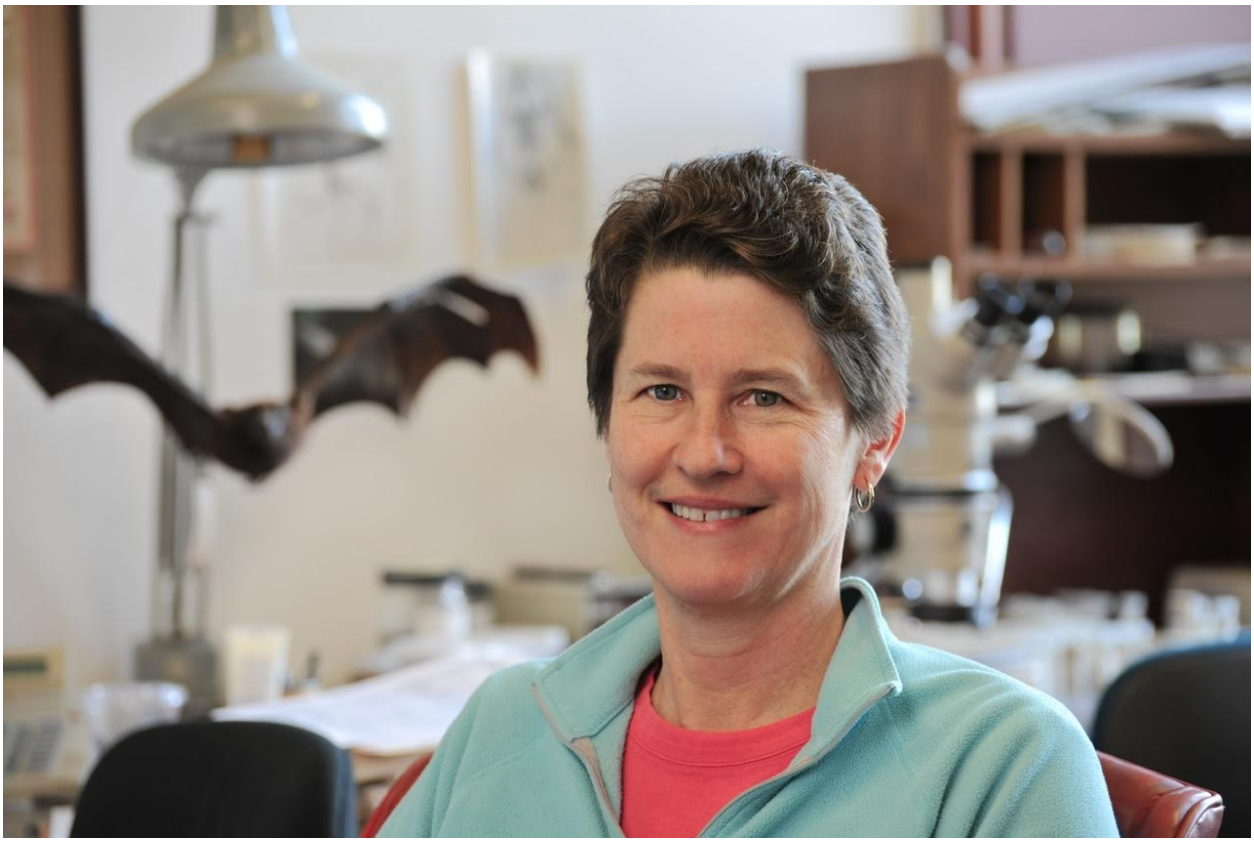
It is recommended that this oral history be cited as follows:

Nancy Simmons "Nancy Simmons: The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology" conducted by Paul Burnett interviewer in 2015, Oral History Center of the Bancroft Library, The Bancroft Library, University of California, Berkeley, 2017.



Nancy Simmons

Nancy Simmons is Curator-in-Charge, Department of Mammalogy, Division of Vertebrate Zoology, American Museum of Natural History (New York). She received her PhD from UC Berkeley in 1989.

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01-00:00:00
Burnett: This is Paul Burnett interviewing Dr. Nancy Simmons for the University Series, the Bill Clemens UCMP Oral History Project. And it's May 13, 2015 and we're here at the Bancroft Library. Dr. Simmons, welcome.

01-00:00:23
Simmons: Thank you.

01-00:00:24
Burnett: So let's start off if you could tell us a little bit about where you grew up and how you first got interested in science.

01-00:00:34
Simmons: Well, I was always interested in science. I grew up in Colorado and I was one of those people who was always interested in nature and animals, natural systems. I collected everything, skulls, rocks, bones, snakes, you name it. And so I think I always knew that I wanted to do something in natural life sciences or history, something like that.

01-00:01:02
Burnett: Was there an emphasis on that in Colorado in particular because of the great outdoors being just right there?

01-00:01:08
Simmons: Not so much. My mother was a biologist. She gave it up when I was born, as people often did back in those days. But she had a master's degree in biology. And so she always encouraged me to be interested in things like that. And so all the way through the time I lived at home I was always engaged in picking things up, reading about nature, reading about history, things like that.

01-00:01:31
Burnett: And you went away for school? Is that right?

01-00:01:33
Simmons: Yes. One of the things I wanted to do was go as far away from home as possible for college.

01-00:01:40
Burnett: Which is a natural feeling. [laughter]

01-00:01:40
Simmons: And so growing up in the center of the country, I was looking at schools on the East Coast and the West Coast and I ended up at Pomona College in Southern California. And, interestingly, I did not get a degree in biology or geology, but my undergraduate degree turned out to be in anthropology. And part of the reason for that was I discovered very early in college that if you wanted to be a biology major you didn't have time to take classes in geology

or anything else. I was also interested in archeology and anthropology. If you were a geology major they required so many classes that you didn't have time to take biology classes. But as an anthropology major they had very few requirements and so I was able to take lots of biology, geology, archeology, everything that interested me. And so that was what my undergraduate degree was in.

01-00:02:28
Burnett: Great. So that gave you the maximum flexibility and you could go—

01-00:02:33
Simmons: It turned out that way. I didn't really plan it that way. Obviously I was just a college student. I was just sort of following my interest and not worrying about the details. But I was always very interested in fieldwork and so starting out somewhere in the middle of college I started working on archeological digs in the summertime. And my last two years of college I worked for the Smithsonian digging up a mammoth kill site in Colorado in the summers, which was filled with Pleistocene mammals, things that are extinct today in North America, things like mammoths and camels, horses, things like that. And I found that I was fascinated with the animals and with the history of their being different faunas in the past than there are today in that area. And I wasn't so concerned with the human aspects, the anthropology aspects, which was theoretically what we were looking for, were the cultural artifacts. But I became fascinated with the animals. And so I decided that I wanted to go to graduate school in paleontology, not in anthropology.

01-00:03:40
Burnett: How difficult a shift was that?

01-00:03:45
Simmons: Not so much because the area of anthropology that I was interested in was prehistory of ancient human cultures and their interactions with each other and with the natural world. And so a lot of people working in those areas are studying the animals from the Pleistocene period and so on. And so it was actually a relatively reasonable shift and also when you're just entering graduate school most people don't come in with a perfect background anyway. And so the fact that I had a lot of geology and biology background, as well as the anthropology, meant that it wasn't that hard a transition. I was obviously missing some things but everybody is when they enter graduate school.

01-00:04:28
Burnett: And I suppose it's a great time in the history of paleontology, and many other disciplines, as well, where there is a kind of cross-pollination of disciplinary questions and even some disciplinary practices are sort of migrating around a little bit.

01-00:04:43
Simmons: Yes. I think that one of the things that was happening, and now we're talking about the late seventies, early eighties, was there was a recognition that

interdisciplinary studies were really important and that it was opening up new areas of research but also new discoveries, new insights by bringing together people in different fields. So rather than trying to keep everybody separate and in their own little cubbyhole there was an interest in fostering interdisciplinary research and work.

01-00:05:18
Burnett: And how did UCMP become the choice for you to pursue paleontology?

01-00:05:25
Simmons: Well, in looking for graduate schools I was not very sophisticated. I knew some of the names of people who were doing interested work that I had run across in my courses and in reading papers. And so I was looking for a program where I could study paleontology of particularly Pleistocene mammals. At that point I thought I was mostly interested in Pleistocene. And I just ran across the department of paleontology, University of California. And I looked into it and I thought, "Wow, they teach all these classes in paleontology and they have faculty who study extinct mammals and they have more than one faculty member who studies extinct mammals." And so I thought, "This looks like a good place." And so it was my top choice.

01-00:06:15
Burnett: This real concentration on mammalian and vertebrate paleontology was really important?

01-00:06:20
Simmons: Yeah. Well, for me because some people come out of undergraduate programs at big universities where they have large concentrations of people working in narrow fields, basically. But I came out of a small liberal arts college where there wasn't anybody who was a paleontologist. There were really talented faculty members but there are just not that many of them. And so the courses tend to be more general. And so I really wanted a focused program. I was very excited also about the idea of there being a museum associated with it with specimens, with material to work on. And so when I found out about the UCMP and the paleontology department I thought, "This is great. This is what I want to do."

01-00:07:04
Burnett: There seems to be a large number of the folks that—well, I wouldn't say a large number but many of them start being interested in humans and then they sort of radiate out and look towards other kinds of ecosystems and other kinds of organisms. What really attracted you to studying mammals?

01-00:07:33
Simmons: Well, I think people who come to vertebrate paleontology tend to come—that's one direction. The other direction is to come in obsessed with dinosaurs. Right?

01-00:07:41
Burnett: Right. Okay. Fair enough.

01-00:07:42
Simmons: So there are the kids who grew up and, "I want to study dinosaurs," from age five on up. And so people who study mammals do tend, I think, it come, often are interested in human evolution, as well. I found pretty quickly that physical anthropology, which is the subfield of anthropology involved in human evolution, was a very small, very political field which was largely controlled by men. And I found myself very quickly feeling like the opportunities in physical anthropology were extremely limited. Also I found quickly that the methodologies being used, I felt in physical anthropology, were basically a couple of decades behind what were being used by biologists interested in animal evolution. And I found the diversity of animals available to study in the rest of Mammalia to be really attractive.

01-00:08:48
Burnett: Right, right. And the research questions involved.

01-00:08:51
Simmons: The research questions. Yes, yeah.

01-00:08:53
Burnett: Yeah, yeah. So you I guess moved up to Berkeley to study. Can you talk about the nature of the training that you had? There's often a big emphasis on fieldwork. Were you out in the field?

01-00:09:10
Simmons: Yes. I was out in the field. So coming in with a degree in anthropology and I had reasonable basic coursework in geology, basic coursework in biology, I needed to fill in an understanding of basic paleontology. What is the history of life? And then start learning what are the areas that I might be interested in studying? What are the kinds of questions that I'm interested in? So I spent probably the first couple of years that I was here taking classes largely, as I think most people do. I remember Dave Wake's evolution class as being one of the big hurdles to jump through because I had never had a course in hardcore evolutionary biology, everything from population genetics on up. And so that was a real challenge for me and I found it very engaging. I also got interested, and this is a direction my career has taken ever since then, in phylogenetics, the study of relationships of animals, various species to each other, larger scale patterns of evolution, building evolutionary trees of groups and using those to interpret the biology of the organisms. So I took a lot of classes to fill in these various gaps. The thing I was most anxious to do when I first got here, I wanted to actually do research and I wanted to do fieldwork. So those two different components were things I just couldn't wait to get to.

01-00:10:53
Burnett: And when is the earliest time that you got out into the field? Was it—

01-00:10:59
Simmons: Frankly I don't remember but it was very early in my graduate career so it might have been the first summer or the second summer that I was here. That's a long time ago. I don't really remember the details.

01-00:11:14
Burnett: And so I imagine that you had mathematics before you came?

01-00:11:20
Simmons: I did.

01-00:11:20
Burnett: Yeah. So that's one of the things you took care of when you were at Pomona? You were able to—

01-00:11:23
Simmons: Yeah, yeah. I still don't have as much math as I wish that I had. But I came in with enough, I guess, to get going in my graduate career.

01-00:11:35
Burnett: Right, right. And I'm not entirely certain of this but I believe that in the early to mid-seventies, when Annalisa Berta was there, it wasn't done that women went into the field. So that's obviously a shift that has taken place between 1974—

01-00:11:56
Simmons: Yes. So I believe that I was the first female graduate student that Bill Clemens ever took into the field to do research. And one of the things I've always found amusing was that he didn't take me just to do research, he hired me as the cook for the field crew. So my first summer in Montana I was there to cook as well as to do fieldwork. Which actually turned out to be a great experience because one of the aspects of being the cook is you also have to do the shopping. And so Bill would take me out. Driving into town was a long way away from the field site, so it was a big deal to make these big treks. But we'd also stop and meet with the local ranchers and do the socializing that you do to keep up the connections and to explain to people what you're doing. And so if I had been there just to do the fieldwork part and I had gotten up every morning and put on my boots and gone directly out, I never would have learned really important things that I learned from Bill about how to manage your interactions with the local people on whose ranches you're working. You need their goodwill, you need their understanding, but also just their friendship. And Bill had been working for a number of years out in Hell Creek before I first went and so he had these personal relationships with many of the families out there. And so I got to take along because I was there to go do the grocery shopping basically at the end of a long drive. I got to tag along and see how this worked and that was a very important part of my learning about how you get fieldwork done.

01-00:13:43
Burnett: Absolutely. To say that that work is important is such an understatement. It's absolutely essential to pay attention to the larger social and political context of the field site.

01-00:13:55
Simmons: Exactly.

01-00:13:55
Burnett: It's in a place where people own land and where they live and do work, other kinds of work.

01-00:13:59
Simmons: Yeah. Exactly, exactly. So that was, as it turned out, an excellent experience, an excellent introduction to paleontological fieldwork for me, to get to see that side of it. So I perhaps got a few hours less actual fieldwork done but I learned these lessons basically from watching a master in action, just being friends with the local people, explaining what we were doing, explaining the findings, but asking after children and talking about each other's wives and families and things like that. So I was the fly on the wall that particular summer and learned a lot.

01-00:14:40
Burnett: That's a key piece of the field sciences. And you think of like cultural anthropology, too. The importance of trust. That you absolutely have to have the trust and the support of the local population.

01-00:14:55
Simmons: Well, especially when you're working on people's land and when you're dealing with ranchers where you're going out in places where they have their animals, their herds. And if you make a mistake and you leave a gate open in the wrong place you can cause serious financial loss to somebody. There's all sorts of things you can imagine going wrong. So it really is important that you behave well and that you don't violate that trust. So that was a really good summer for me seeing all the different parts of what happens in the field.

01-00:15:35
Burnett: Has that changed? Those types of relationships, has that gotten different in the intervening years?

01-00:15:43
Simmons: I can't really speak to that because I don't do fieldwork in the United States anymore. So I work overseas. But I tell you, what hasn't changed is the fact that it's so critical to be as little trouble as possible to the people who are bending over backwards to give you access to their land, their space, their resources and to develop personal relationships with them, to not treat them as somehow just a problem that has to be dealt with. But to build personal relationships with the people who are the local—whoever they happen to be in the region that you want to work.

01-00:16:19
Burnett: Do many of them become engaged and interested in the work that's being done?

01-00:16:23
Simmons: Yes. Absolutely. But I think you have to encourage that. You have to make people feel like they can understand what you're doing. Many people find scientists intimidating because they feel that they don't really understand what it is we're doing or why and it's possible to explain things in a way that makes them incomprehensible to normal people. I think it's just important today, as it was back then, to do as much as you can to explain what you're doing, to transfer the excitement. Why are we interested in this? Why is this cool? Why should you also be interested? And I've very rarely found that people don't respond to that. They find it interesting because what we do is interesting.

01-00:17:12
Burnett: Absolutely. So you've gotten all the catchup coursework that you needed to do and the training and the formation in paleontology and learned the fieldwork techniques and the larger social and political techniques for being successful in the field. How did you happen upon your research question for your dissertation?

01-00:17:35
Simmons: Well, totally by accident is the answer to that. And, in fact, today when I am counseling undergraduate students who aren't 100 percent certain what they want to do about entering graduate school—I went straight into a PhD program from my undergraduate and I probably shouldn't have done that. I probably should have gotten a master's degree first. And I tell people that all the time. And the reason is that what I ended up doing for my PhD was completely accidental in the sense that it wasn't a well-chosen topic that I looked far and wide for. Let me step back and explain. So I arrived at Berkeley and I wanted a project so desperately, any kind of research project. And Bill was always very open and helpful as an advisor. And he said, "Oh, I have these specimens here that are probably a new species of multituberculate from the Hell Creek and so it's just a handful of specimens." And he said, "If you want, why don't you work on those? You can describe them, write them up as a small paper and it'll give you a first research project." And so I thought, "Oh, this is wonderful." And so I dove into that. And I ended up doing my PhD on that group of mammals because it was what I got started on first. It was a great project. But looking back I probably would have chosen a different kind of project to do if I had known what I do now or what I learned during the course of that. But it was sort of the accidental PhD. And I'm very glad that I did it. But if I'd gotten a master's degree and done a project first as a master's and then said, "Okay, now what am I interested in? What's the best group to work in?" I probably would have made a different choice.

01-00:19:29
Burnett: Right, right. You worked at a number of institutions that we'll get to. Can you talk about the UCMP/Bill Clemens style, research style, orientation towards questions that shaped your research subsequently?

01-00:19:49
Simmons: Well, I was probably largely oblivious to all that when I was actually here because, of course, when you're in graduate school you're still pretty young and uninformed. If I would say that there was a Bill Clemens research style I would say that it's to be careful and thorough. And I think that that aspect of how I was trained and encouraged by Bill has stuck with me to this day. Not to rush through things, to take your time, and to be meticulous. And so the kind of research that I do still today is largely phylogenetics. And Bill didn't do that. And so he encouraged me because he knew the importance of cladistics methods in modern phylogenetics back in those days. But it wasn't what he actually did. And so I found my education and methods largely amongst the other graduate students who were working on similar problems because it was really a time of sea change, when people became really interested in phylogenetics and started to have computer algorithms where you could collect data and you could build phylogenetic trees using computer programs. And so Bill didn't do that kind of work but he was very forward thinking, encouraging all of us who wanted to do it to pursue it.

01-00:21:09
Burnett: Yeah, yeah. It sounds like there's a bunch of things happening in the eighties. Obviously the huge growth in computing power, processing power, which enables the kind of cladistics research that was theorized earlier.

01-00:21:25
Simmons: Right. To put things in perspective in that area. I graduated from college without having ever used a computer for anything. I never wrote a letter on a computer. I never logged in, I never turned one on. So I got all the way through my college career with zero computers. So I came to Berkeley and that was when the first Apple computer was—I think my first year of graduate school. So it's a little tan box about this big and then there's PCs which are sort of drabber gray colored boxes that are about three times the size.

01-00:22:02
Burnett: So this is 1982 to be precise, right?

01-00:22:04
Simmons: Yes, yes. And so they were all floppy-disk driven back then or mainframe computers that took up whole buildings and had less power than my iPhone does today. So it really was a time when computers, both for storing data, just basic database work, was just beginning. And word processing. I wrote my dissertation with floppy disks coming in and out and every time you needed to change something in the program you had to put a new disk in your drive and in and out and in and out for these. And then the computing power to build phylogenetic trees was just beginning. And so this was new to everybody. So

obviously it was something that Bill had not done before because nobody had done it before. And it was really the beginning of a whole new era, I think, for science.

01-00:23:01
Burnett: So let's get caught up a little bit. You complete your PhD. But you said there's a PhC.

01-00:23:12
Simmons: Oh, yes.

01-00:23:13
Burnett: Can you talk a little bit about that?

01-00:23:14
Simmons: Well, that's interesting because I didn't know that I had a PhC until I got my transcript after I graduated. It's a candidate of philosophy degree, which is some sort of strange thing that University of California Berkeley invented or uses for students who don't get a master's on their way to their PhDs.

01-00:23:36
Burnett: Fascinating.

01-00:23:36
Simmons: And so it allows you to have a graduate degree which then opens up certain doors for applying for grants. So I never used it. I didn't know I had it until at some point it turned up on my transcript.

01-00:23:50
Burnett: Okay. [laughter] And during this time you're doing work. You're a preparator at the museum in the eighties. And you're also lecturing and then you complete your PhD in 1989. Is that right?

01-00:24:09
Simmons: Yes.

01-00:24:09
Burnett: Okay. You changed coasts basically in the next phase of your research. Can you talk about your landing at new institutions on the East Coast?

01-00:24:21
Simmons: Yes. So when you're focused on finishing your PhD you're totally focused on the dissertation, getting the dissertation done. But then at some point when you realize you're actually going to finish it you start worrying about what am I going to do next. And frequently the answer to that is a postdoctoral position someplace. So I applied for postdoctoral positions I guess sometime during my last year of graduate school. And where I really wanted to go and where I landed was the American Museum of Natural History. In part because that's where Mike Novacek is still today. He was one of Bill's previous students. And Mike was doing a lot of work with phylogenetics of mammals which, of

course, was one of the areas I was interested in. And also had done some work on bats. And I thought bats were really cool. And as I mentioned before, I might not have done the dissertation that I did if more thought had gone into picking a dissertation topic. And let me explain a little bit about that. My PhD was on multituberculate mammals, which lived through the later stages of the Mesozoic era and then up to about the Oligocene, so early Tertiary. So across the Cretaceous/Tertiary boundary and the extinction of the dinosaurs. But they're a group of mammals that have no living descendants and very little was actually known about where they fit in the mammal family tree at that time. It was like they were a specially created group of strange sort of semi-rodent like animals.

Anyway, I grew frustrated during the time I was working on multi-tuberculates because of the lack of biology. We didn't have any modern analogues really or members of the group to work on it. So we didn't really know what they did in terms of behavior, what muscles they had, and where did they attach. We were guessing based on comparisons to groups that were just very distantly related. So I thought as I move forward in my career I want to work on something that has biology associated with it, that has living members that we can use for looking back into time and drawing conclusions about what the fossil extinct members were like by actually comparing to living animals. And so I thought, "Well, bats are just cool." And they're also a very diverse group in the modern age and so I thought, "Well, this is something I'd be interested in working on." And I never expected to spend the rest of my career working on bats but that's how it turned out.

But stepping back a bit, when I was applying for post-docs, Mike Novacek had done some very interesting work on the evolution of echolocation in bats. And I was interested in pursuing work along that direction. And so he encouraged me to apply to work at the American Museum as a post-doc and so I got a post-doctoral position and so I went. And when I arrived at the Museum, due to internal politics and people and offices and space and resources, I actually got placed in the department of mammalogy instead of in the vertebrate paleontology department. So that was a whole new area for me, basically, to be right in the middle of the living mammal department, which turned out to be very good for me, because that gave me a whole other aspect of education basically. And so that was where I landed with my postdoctoral research.

01-00:27:52
Burnett: And did that open up teaching opportunities, to have been situated in mammalogy?

01-00:28:00
Simmons: Well, no, because the American Museum is a big museum but at the time it was not a graduate education program or a university in its own right. We do now have a PhD program of our own there. And there was a lot of graduate

education going on through partnerships with other universities but as a postdoctoral fellow I had no teaching obligations. My job was to do research and so that was what I did full-time through my first few years there.

01-00:28:33
Burnett: And so the work that you would be doing, say, at CUNY and—

01-00:28:40
Simmons: Oh, I taught a couple of classes at CUNY, just teaching labs, biology labs, things like that. At the time I was still thinking that I was going to go into a university teaching position after I finished and so I was trying to prepare myself to teach. I'd done a lot of teaching at Berkeley before I left. But I wanted to be sure that I was prepared and eminently hirable as a university professor. So I did do a little extra teaching on the side for that experience.

01-00:29:09
Burnett: But you got wrapped up in the research questions?

01-00:29:11
Simmons: Yes, yes. I did.

01-00:29:12
Burnett: Let's all agree that bats are cool to start with. But there are specific evolutionary questions and phylogenetic questions.

01-00:29:25
Simmons: Yes, very much so. So bats today, there's over 1300 living species and there's probably at least another 300, I would guess, extinct species of bats known. So they're very diverse. They live worldwide and they have a fossil record that goes back into the Eocene and with some really spectacular fossils from the early stages of bat evolution. In terms of what makes them special amongst mammals? They're the only mammals that are capable of flapping flight. So how did flight evolve in this lineage is a big question. They're the only mammals that use—well, they're not the only mammals but they're the main group of animals that uses sophisticated echolocation to orient. Whales also use echolocation in the water but bats are the only ones that use it in the air. But not all bats echolocate. So one family of bats, the Old World fruit bats, don't have echolocation. So there's always been the question, well, why? Why do we see this pattern? Why don't all bats echolocate? When did echolocation evolve? Did it evolve once or twice?

And back in the early nineties there was also a big controversy in terms of evolutionary relationships in which a man named Jack Pettigrew, who was a neurobiologist in Australia, discovered that the neural pathways in the brain associated with the visual system in the one group of bats that don't echolocate, the Old World fruit bats, their visual system works just like primates. Whereas the echolocating bats are very different. And so he proposed that, well, bats had actually evolved twice. His hypothesis was that the Old World fruit bats were really more closely related to primates—

01-00:31:12
Burnett: Wow.

01-00:31:13
Simmons: —using these data from the brain and that the echolocating bats were related to something else, insectivores or something along those lines. This happened basically the year I started my post-doc.

01-00:31:26
Burnett: This is 1991?

01-00:31:28
Simmons: About 1990ish. And so I was encouraged by a number of people to go after this because the man, Jack Pettigrew, who had put this hypothesis forward had only looked at the brain. He hadn't like looked at the rest of the animal. And so I spent my post-doc, instead of working on what I had initially gone there to do, I was going to work on echolocation and ear ossicles in the skull, something that ended up never doing. I instead got sucked into the flying primate hypothesis and basically developing a dataset to refute that hypothesis. And as it turned out, that was the preponderance of data. If you add all the anatomical data in, not just the brain, it seems clear that bats just evolved once and that the visual system is simply a convergent feature seen in the Old World fruit bats and primates. But about that time was when the big transformation of the field of phylogenetics into using molecular data, DNA sequence data, was happening then, too. And as soon as the DNA sequence data started coming in it overwhelmingly supports the idea that bats are a single evolutionary lineage. And so the flying primate hypothesis was completely refuted, wiped out, in essence, through a combination of the morphological data that I provided and all this new molecular data which is unambiguous.

01-00:32:55
Burnett: There seems to be a common theme in the research from that time up until the present, and that is the question of developing models that fit molecular data with other phenomic or morphological data.

01-00:33:12
Simmons: Yes.

01-00:33:13
Burnett: And the debate seemed to circle around that. And you have questions of when the molecular data does not at all match and there can be these challenges. Can you talk a little bit about that work on developing models. Were you involved in that?

01-00:33:34
Simmons: Well, yes. Basically I'm a total evidence fan. So I believe that you need to bring all the data together and analyze it simultaneously to come up with your most robust best-guess hypothesis of relationships. So it's important to remember that phylogenetic trees and sets of statements about this is related to

this in this fashion are always hypotheses. So they're always things that require further testing and can always be refuted in the future. Today, however, we have so much molecular data that can be applied to many of these questions, that it's hard to imagine that these hypotheses will ever be refuted. But it's caused a rearrangement of our understanding of mammalian relationships.

01-00:34:25
Burnett: Can that also be helpful for the extinct species? For the multituberculates, for example?

01-00:34:28
Simmons: Oh, absolutely. Absolutely. So one of the recent research projects that I've been involved in was the Tree of Life for Mammals Project. So the National Science Foundation developed a program called the ATOL program, short for Assembling the Tree of Life about ten years ago. I think it's ten years ago. Anyway, started giving large grants for big projects aimed at building phylogenetic trees of large groups of organisms. And so I was part of a group that got one of these big grants for mammals. And so we as a team from the beginning sort of divided up half the money goes for molecular sequence work and molecular trees and the other half goes to the morphologists, that's the group that I was part of, for building a really big data set of morphological or phenomic data for mammals. And so the way it worked out, it turned out to be much, much harder than we thought to collect the phenomic data. We ended up with over 4500 phenomic characters, morphological characters for mammals scored—

01-00:35:37
Burnett: Goodness.

01-00:35:37
Simmons: —across nearly a hundred species of mammals, including a lot of fossils. We estimated at some point that it took about one person working for two months to gather one row of data for this data set, it's such a big set of characters. So the people working on the sequencing data were able to sample many more samples. And they gathered data from, I think, twenty-seven nuclear genes. So a lot of genetic data. And they published their results before we published ours because they were faster at getting their data together. In the end we combined all of that genetic data with all of the phenomic data to build one basically super data set and build a tree that included all of these living taxa for which there's all of this genetic data, as well as the fossil taxa. And then the fossils, of course, are only placed based on morphology because we don't have DNA from them. But their places are being constrained by the genetic data which is providing a lot of signal for the interrelationships of the living taxa.

01-00:36:49
Burnett: Right. So you can draw inferences based upon how the genetic space moves.

01-00:36:52
Simmons: Right, right. Not everybody agrees with me but I feel today that the role of morphological data in phylogenetics is really to place fossils, to allow us to place these large numbers of extinct lineages into trees. Because you can only use morphological data for that. And what it also does is it gives you calibration points for the molecular trees. Because everybody's interested in rates of evolution and patterns of change in rates of evolution, also knowing when various divergent times are for different lineages. When did these big splits occur in earth's history? What else was going on in the planet? Those sorts of questions. And while molecular data are hugely powerful for building trees of living organisms you can't put the fossils in unless you have the morphological data to go along with it. So when I first started building trees, I was building a lot of trees of living animals using morphology, and I would not do that today because I believe that the molecular data are much more powerful and easier to get. So if I'm interested in the relationships of a set of living species of bats I'm not going to go out and collect morphological data for that. But if I have a fossil that I think belongs to that group and that can help give me calibration points for knowing what the rates of evolution were, then I will gather the morphological data sets so I can put those fossils in and tie the whole thing together.

01-00:38:37
Burnett: From what you've shown me, showing me a couple of articles, it sounds that the morphological data can act as a kind of brake on perhaps overly ambitious genetic data.

01-00:38:51
Simmons: I don't think that's really true.

01-00:38:52
Burnett: No?

01-00:38:52
Simmons: I mean, we have so much genetic data today that it's largely going to pattern the tree. But morphological data can definitely play a role. And sometimes when you have branching points or sets of relationships which remain unclear even though you have a lot of molecular data, so maybe it could be this way or it could be that way, the morphological data may help you choose between those alternatives. So morphological data still has a huge role to play, I believe, but not as the primary data for building trees of relationships of living taxa. Frankly it's just too hard. It takes too much time and it's much more difficult than it is today to go out and sequence a couple of genes and plug it into a tree. Frankly that's much more time efficient for getting more data.

01-00:39:41
Burnett: Well, when you described the morphological data set that you put together, and I'm thinking about your earlier comparative anatomy research, 4500 characters?

01-00:39:53
Simmons: Yeah, yeah. So at one point I had to give a presentation to our board of trustees at the American Museum and we had a big projection screen up on the wall. So maybe the size of this wall for this presentation. And we put up a picture of the data set. So they're square. So it's a matrix style data set that has an image of a fossil or a living taxa on that particular trait that we're studying. So the species go down one side and the characters are across the top and there's all these squares. And I was preparing for this and I realize that the squares for the data set were really about one-foot square when projected on this screen. And so I thought, "Well, how big then is the data set?" And I calculated that from top to bottom, if each cell in this data set is one-foot on the side, that the data set was nine stories tall and half-a-mile long. [laughter] So it's a really huge, really, really huge set of data.

01-00:40:56
Burnett: That puts things in a little perspective, I think. But the basic story is that things become more complicated in assessing because you have different types of evidence and you have to fit them together. But the advantage is that you can use one to sort of test the other and arrive at a more robust portrait. So we are getting somewhere in terms of getting more robust reliable phylogenetic trees.

01-00:41:24
Simmons: Oh, absolutely. I think that's one of the very exciting things that has happened during my lifetime basically as a scientist is that we're now getting to the point where the big picture—most researchers who work on mammalian relationships pretty much agree on the big picture of how the major lineages are related. And there's arguments about certain parts of the tree and certain dates and so on. But the big picture, which was largely unknown or was just kind of guesswork back when I started graduate school, is now pretty much there. When I first started working on bats nobody knew how the families of bats were related to each other. So today we have about twenty living families and ten extinct families of bats. And nobody had any idea how those groups were related. Well, today, largely because of molecular data, pretty much everyone agrees how the living families are interrelated with each other. And so the questions now are, well, where do the fossil families go? And so that's where I'm working now and I have been working for a long time, sort of putting some of those pieces into place. And all the way down to the species level. We have species level phylogenies of many groups now which are really well supported and which people pretty much agree on. It gets us back, at least for me, to what I initially got interested in trees for, which was that they're powerful means of understanding biology and evolution. But we got so wound up in building them, because we didn't have them, that we sort of lost track of what some of these questions are that now we can go back and start addressing.

01-00:43:01
Burnett: Yeah. And it sounds like some of the big challenges, too, are distinguishing between genetic relatedness and convergent evolution.

01-00:43:13
Simmons: Well, yeah. Right. So there's two ways in which an animal can be similar to another animal. It can be similar because they shared a common ancestor that looked like both of them in a particular trait. So lizards have five fingers and I have five fingers and that's because we share a common ancestor a really long time ago that had five fingers. So noticing that something else has five fingers, it doesn't tell you much about its relationships except that it goes in this big group of five-fingered things. Another way you can be similar to another organism is because you have independently evolved a particular trait, which means that your common ancestor didn't have that feature. And so birds have wings and bats have wings but their common ancestor did not have wings. They've evolved their wings independently. In fact, you can tell that very rapidly by looking at the wings and seeing that they're built completely differently. But convergence can also be very sneaky, in which it can take the same part of an animal and modify it in the same way independently, often as a result of adaptation for a similar lifestyle. And so teasing apart the similarity which is due to inheritance and ancestry from the similarity that's due to convergence is a lot of what people who study evolution and phylogenetics, it's a lot of what we're doing at every stage of our research, is trying to distinguish the ancestry based similarity, which we call homology, from convergence.

01-00:44:48
Burnett: Right, right. And there are a host of techniques for doing that—

01-00:44:53
Simmons: Indeed.

01-00:44:54
Burnett: —that are far beyond the scope of this interview frankly.

01-00:44:56
Simmons: Yes. [laughter] And wisely so.

01-00:45:00
Burnett: [laughter] and I want to talk about your career a bit more, too, because, as you said, at one point you were thinking of becoming a professor in the academy and you were teaching to prepare for that. But you advance at the AMNH. You become an associate curator in 1998 in the mammalogy division and in 2000 you're chair of the Division of Vertebrate Zoology at the American Museum of Natural History. Can you talk about those responsibilities and how your work changes based on those moves?

01-00:45:36
Simmons: Yeah. Well, when I started as a postdoctoral fellow one of the most amazing experiences I had was the then chair of the department of mammalogy, Guy

Musser, walked into my office one day and said, “What can I do for you?” And as a graduate student I’m, “What do you mean? You’re the chair. What do you mean, what can you do for me?” And so one of the marvelous things was that I was more or less told, “Your job is to do research and we want to provide you with all the resources you need and help you need so that you can be as productive as possible.” And that was wonderful. I didn’t have to attend meetings, I didn’t have to write reports. I didn’t have to supervise staff. I didn’t have any of those responsibilities. Just research. And I didn’t realize how precious that was.

Subsequently at the museum, after my post-doc ended, I stayed on for a couple of years with NSF funding for a grant to continue work on bats, and then I got hired as a curator. Our curatorial system is setup the same way as an academic professor track. So instead of being an assistant professor you’re an assistant curator. And then you go through a review process and you get tenure and you become an associate curator, just like you become an associate professor. And then eventually get promoted to full curator versus full professor. So as I worked my way through that, because the duties to the institution at the museum are in collection management, collection building, outreach and things like that rather than teaching, those were my duties, was help look after the collections, help build the collections, help with public programs as needed and things like that. Which was really much simpler than most people face at universities where you’re torn between this teaching versus research aspect. Your main job may theoretically be to teach but you’re usually rewarded for your research and grantsmanship efforts much more than you are for your teaching efforts.

So as I moved up through the curatorial ranks, I guess, I did more and more management and at a certain point I was tapped to take over as the chair of the division of vertebrate zoology. And that position involves supervising four departments. So I supervised then the mammalogy department, the herpetology department, the ichthyology department, and the ornithology department, which stayed as separate entities when we became a division previously to that because they’re located in sort of separate parts of the building.

The first year or so that I was doing that work was very much overwhelming. It took me months to learn where all the rooms were. There are like sixty-five rooms or something that belonged to my division and it’s like I at least wanted to know where they were in this warren of a museum. So I spent a lot of time during those years doing management. And it was also when we were first building our electronic database. So I had to more or less develop all the plans for that, get funding for that, manage the process of how the data were going to get entered. Each department at that phase had their own internal database, electronic database, except ornithology which didn’t have any at all, and each database was different and they couldn’t talk to each other. Anyway, so it was a big trial and a big divisional effort with a lot of people involved. I was just

one of the players here. But helping make that become a reality, that we would have a single online database for our whole division.

01-00:49:13
Burnett: Wow. How long did that take to put together?

01-00:49:16
Simmons: Well, it's still ongoing. These things never end. But probably, I don't know, six or eight years maybe to get it to the stage where it was actually semi-functional—

01-00:49:29
Burnett: Usable, yeah.

01-00:49:30
Simmons: —and usable. And then lots of hiring and firing and moving staff from here to there and report writing and all of that sort of thing.

01-00:49:40
Burnett: And grants I guess continually, right?

01-00:49:42
Simmons: Yeah.

01-00:49:43
Burnett: Ongoing. There's a long list throughout this period.

01-00:49:45
Simmons: Yeah. The American Museum is a wonderful place in terms of support for research. So I've always had basic access to money to go to meetings and for whatever basic trips that I wanted to do and also seed money for getting started with field projects and things like that. But yes, I've applied for and been lucky enough to get a lot of National Science Foundation grants over the years to support my research. But also collections. So, replacing old cabinetry. The first one of those grants, I wrote a collection improvement grant to rehouse the whole bat collection. My husband is also a curator at the American Museum, also in the department of mammalogy. And he works on marsupials. So he wrote a grant, which I was involved in, to rehouse the whole marsupial collection and now we're trying to get another grant to do the primate. So there's a series of these institutional grants on top of the research grants.

01-00:50:44
Burnett: Right, right. It's a massive institution, isn't it?

01-00:50:48
Simmons: It is. It is.

01-00:50:49
Burnett: With gigantic collections.

01-00:50:50
Simmons: Yes.

01-00:50:51
Burnett: As is UCMP. So you finish the chairship of the division of vertebrate zoology in 2011.

01-00:51:02
Simmons: Yes.

01-00:51:03
Burnett: Since then have you been able to return more to research? Have things—

01-00:51:07
Simmons: Yes. I'm still the curator in charge of the department of mammalogy but after being division chair it seems like that's a piece of cake. [laughter]

01-00:51:17
Burnett: I bet.

01-00:51:18
Simmons: But I'm still managing staff, the collection staff, and so on. Just as I stepped down as division chair I was tapped to serve on the committee which oversees our graduate program, which is a huge amount of work.

01-00:51:34
Burnett: I bet.

01-00:51:35
Simmons: So this committee basically looks after—we do semiannual reviews of all the graduate students and their progress and how they're doing, meeting with the graduate students. We also handle the whole admissions process for our PhD program in comparative biology. We bring in usually eighteen people every year, students, potential PhD students and do long interviews with them and so on. Having all of that committee work, just in the time I stepped down as division chair, meant I didn't get nearly as much research time as I had hoped when I stepped down. But now I'm not doing that so now I do actually have virtually all of my time for research again, which is really fun.

01-00:52:21
Burnett: So what are the projects that are most engaging to you right now, over the last couple of years?

01-00:52:23
Simmons: Well, it's interesting. You asked me about the trajectory of my career. And after I left Berkeley and started working on bats, I worked almost exclusively on living bats for a long time. I've always dabbled in fossils along the way but what I've really been coming back to in recent years is a lot more paleontology now. So virtually all of my projects involve fossils at some level. And so right now I have a paper on my desk almost ready for publication describing a new family of fossil bats from the Eocene of Africa with Gregg

Gunnell and Erik Seiffert. Other projects with Gregg Gunnell, who works on fossil bats, as well. We have a project on bats of Olduvai we're working up. I've been working with one of my ex-students who's on the faculty at SUNY-Stony Brook on methods of integrating fossil data with data from living animals and building trees and also reconstructing skull size based on teeth. Have a paper on that. One of the things I continue to spend a lot of time on is just how many species of bats are there in the world. I sort of by accident ended up to be the person who sort of manages that for the bat community. That's a historical accident because the person who wrote the bat chapter for the *Mammal Species of the World* volume, several of their volumes, his name was Karl Koopman, and he was a curator at the American Museum before I was. And he passed away in 1998 and when he died I inherited his annotated copy of the book, where he'd gone through and scratched out species names and moved them to the next page and written in new ones. And so they asked me to take over for the subsequent edition of *Mammal Species of the World* and I agreed to do it before I had any idea what that meant. And between Karl's last edition, which was 1993 and when my edition was published in 2005 we went from 925 bat species to over 1100.

01-00:54:47
Burnett: Wow. And now there's 1300, right?

01-00:54:49
Simmons: And now it's up to 1354, I think, as of yesterday. And so maintaining that data set and being able to provide people with numbers. "How many species are there in this family and what are they?" Those sorts of things. I also spend a lot of time on that. It's a collaborative work with an old graduate student of mine who got her PhD at Columbia University back in 2006, Andrea Cirranello. So we're working together on that.

01-00:55:21
Burnett: Is it because bats were less accessible and it's just that more areas where bats live have been opened up?

01-00:55:31
Simmons: Combination of features. Sometimes you just open a mist net and catch a bat and it's one that no one's ever seen before. There's a lot of hidden diversity out there. It's not easy to catch bats and people still find totally new animals. Also molecular sequencing data have provided new insights into basically cryptic diversity. So things that look similar but are clearly genetically different turn up all the time. And so when people do revisionary studies of various groups they often find that there's more than one species in what was thought to be one. And so my work in that area is mostly just bookkeeping. It's just keeping up with the literature, reading the papers and going, "Oh, we used to think this is one species and now it's six." And so the numbers keep clicking up as a result of that. But it's important, particularly for conservation.

01-00:56:26
Burnett: Absolutely, of course. Of course.

01-00:56:29
Simmons: For understanding what are the limits of species. Sometimes if you take what seems to be a widespread species, maybe threatened in various parts of its range but you think, "Oh, it's widespread and it's okay in general over here." But then when it turns out that it's really five species and each one has a little tiny range, some of those may be under great threat of extinction because if their range coincides with an area that's being deforested or has been hit by a typhoon or what have you, can be natural disasters, it can be manmade disasters, it can be that they're just restricted to a really small area. And so we need this information for making informed conservation decisions. So it's interesting that my work ranges from things that are relevant on living species today all the way back to the Eocene and sort of everything else in between.

01-00:57:22
Burnett: Absolutely. Well, Dr. Simmons, I want to thank you very much for your time and sitting to talk with us today.

01-00:57:27
Simmons: My pleasure.

[End of Interview]

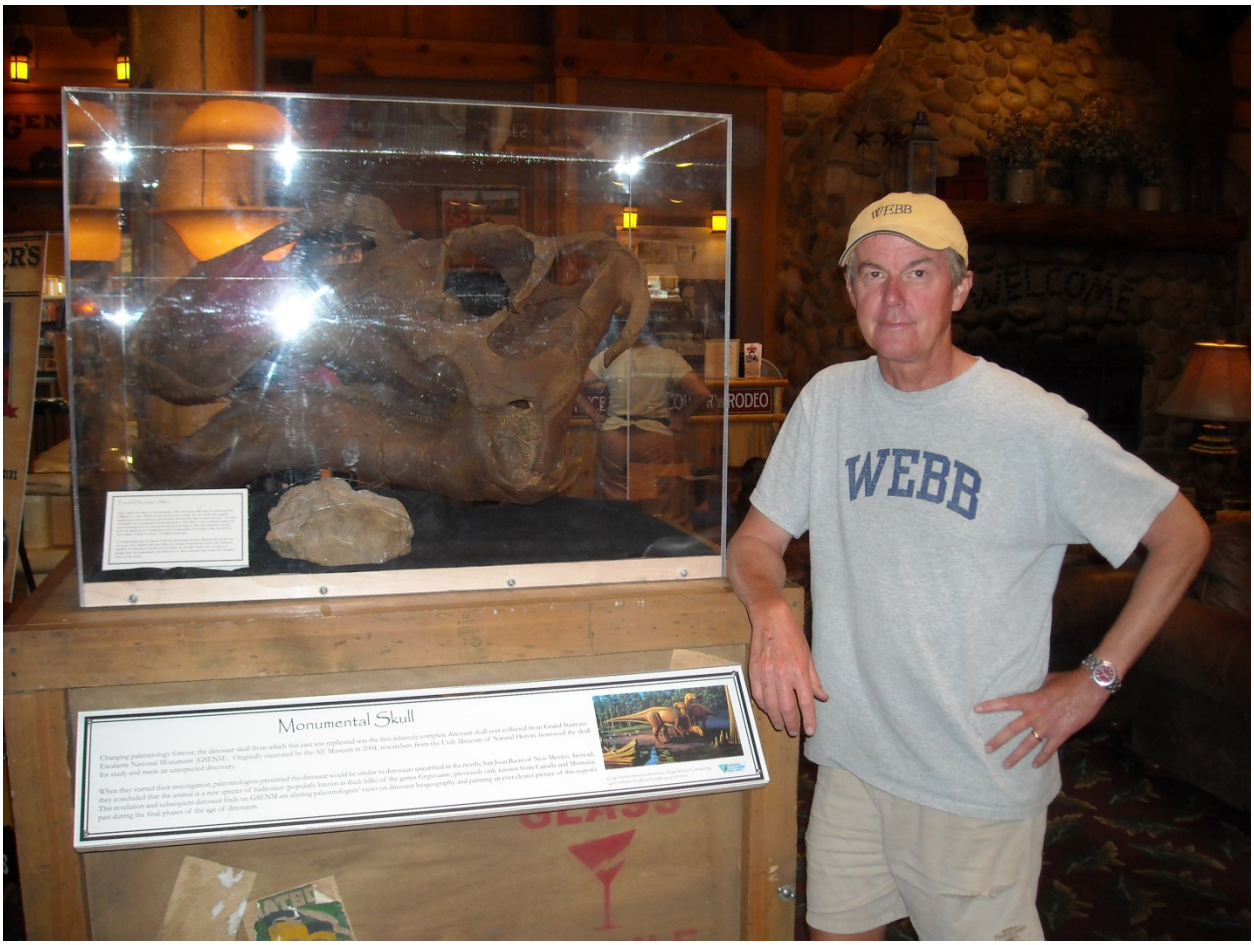
Oral History Center
The Bancroft Library

University of California
Berkeley, California

The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology:

Don Lofgren

Interviews conducted by
Paul Burnett
in 2015



Don Lofgren, 2010

Don Lofgren is Director, Raymond M. Alf Museum of Paleontology in Claremont, CA. He received his PhD from UC Berkeley in 1990.

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Audio File 1

Interview #1 March 10, 2015
Begin Audio File 1 lofgren_don_01_03-10-15_stereo.mp3

01-00:00:00
Burnett: This is Paul Burnett interviewing Dr. Don Lofgren for the University History Series, the Bill Clemens, UCMP Oral History Project. March 10, 2015 and here we are in the Bancroft Library. This is audio file one. Dr. Lofgren, welcome.

01-00:00:23
Lofgren: Thank you.

01-00:00:24
Burnett: Let's start a little bit at the beginning. Can you tell us where you're from and where you grew up?

01-00:00:30
Lofgren: I was born in Minnesota. When I was one-year-old we moved to Los Angeles and lived here for about twelve years. Then we moved back to Minnesota and then I graduated from high school in Minnesota, so I kind of consider myself a Minnesotan with a brief stint in LA.

01-00:00:47
Burnett: And can you tell me about how you first became interested in geology? Did you become interested in geology or was it a general interest in science to begin with?

01-00:01:02
Lofgren: I graduated high school in 1968. I went to college for one-year and dropped out. Then I worked ten years in a grocery store, different parts of it. But eventually I was into management, assistant management. But I didn't really like it. Wasn't very fulfilling. So I went back to school when I was twenty-eight, thinking about forestry as a major, science major in forestry because I liked the outdoors and the mountains and hiking. And my eventual goal was to transfer to the University of Minnesota but I was at a community college. And one of the classes that you could transfer for forestry was geology. So I took a geology class at this community college and I really liked it, so I immediately switched to geology. And then geology has paleo in it. So there was a couple of paleo classes I took at the community college level that I really liked. And so when I transferred to the University of Minnesota, geology was my major. I kept studying geology and I loved my field camp experience in Colorado and being outside. We looked for fossils sometimes in that and I took a couple of courses from Bob Sloan at the University of Minnesota. He was quite a renowned vertebrate paleontologist in those days. And more and more I liked the paleo part of geology.

01-00:02:21
Burnett: What did you like about it? What drew you to it?

01-00:02:24
Lofgren: I liked finding things. I really liked going out and looking for fossils. It's like looking for treasure. You find something, it's really a great feeling. Being outside just looking. I still like that. It's one of my favorite things to do. So when I graduated from Minnesota I was looking at graduate schools but there was another class that Bob Sloan taught, that you'd go out to Montana for a week and camp and look for fossils in eastern Montana. I said, "Wow, this would be great." So even though I had graduated, I took this class. And for one week we went out to eastern Montana to look around Bug Creek. Little did I know, that when I did my PhD work here at Berkeley I'd be working in the same area many years later. So it's kind of one of those moments like, "Hey, I've been here before."

01-00:03:15
Burnett: Right. Serendipity. And you did a master's in geology then at the University of Montana. So can you talk about that transition from Minnesota to Montana?

01-00:03:31
Lofgren: So when I left Minnesota I applied to a few places. And Montana gave me the best package, financial package. So I went there, kind of torn between what part of geology to study. I really liked paleo. I was thinking about mining geology. And then at Montana I was TA [teaching assistant] for George Stanley because I had a lot of experience in paleo already. But there was a semi-retired professor there named Bob Fields who was a Berkeley grad and he was a vertebrate paleontologist. So I immediately struck up a relationship with him and not long after I had started taking a couple of his classes I decided to switch to paleo. He told me that, "If you're going to be a paleontologist at the graduate level, you got to get a PhD, otherwise you're not going to get a job." So kind of right at this point, first year in University of Montana, I decided to become a vertebrate paleontologist working with Bob Fields. Did a field project in Montana, western Montana mapping a Tertiary base and doing the structure and all the geology. Mapped for about six weeks, collecting fossils, and then I remember applying the second year to grad schools. And Bob encouraged me to apply to Berkeley and some other places. And he said he's going to Berkeley for Christmas and he would talk to some of the professors there, Bill Clemens and Don Savage, try to help me get in. So I really appreciated that. But I had kind of set my sights on Berkeley because I had gone to the library and I saw a monograph just published, 1982, by David Archibald about his project in eastern Montana. This big monograph. And I saw that and I said, "Wow, that's what I'd like to do." I'd like to do some kind of big project for my PhD where you publish like a book. And I actually had talked to Bob Fields about it because he had some connections. He could buy me a copy at reduced costs. So he actually bought a copy for me. I paid him back. I never read it but just to have it. And so I really wanted to work with Bill Clemens and do some kind of big project like that, because that's what Dave Archibald did, even though I'd never met Dave Archibald or Bill. So when I applied, a few months later I got this phone call and it's from Bill Clemens telling me they want me to come to Berkeley. And I said,

"Nobody ever called me before from a college saying they wanted me to come there." I thought, "Wow."

01-00:06:06
Burnett: That's a good feeling.

01-00:06:07
Lofgren: Yeah. I thought, "This guy really wants me." So then not long after that I was accepted. I had some other choices but I definitely wanted to work with Bill at Berkeley.

01-00:06:20
Burnett: And he had obviously had a big name and had produced these multivolume sets. Were you interested in mammals at the time or were you interested in more the geology associated with paleo?

01-00:06:39
Lofgren: I was more interested in the geology associated with paleo and the mammals, too. But what the rocks are telling you, along with the fossils that are found there and what the environment was like, what age are the rocks, how do these rocks correlate to other rocks around the other sites in North America. That's called biostratigraphy. That kind of angle. So Dave Archibald's study was a biostratigraphic study of eastern Montana, where he worked by a place by Flat Creek in the Hell Creek Basin area. He studied the fossils, too. So he did both. That's kind of what I wanted to do, too. I wanted to look at the rocks and also study the specimens that were found there, particularly the mammals.

01-00:07:25
Burnett: People have often talked about paleontology at Berkeley being kind of this interdisciplinary node. The geologists work with the paleontologists and there's sort of early connections with people working in other fields. Did it have that kind of reputation or is that just something you knew you wanted to do? It's something that you saw in David Archibald's work and you knew, "I could do that there."

01-00:07:59
Lofgren: Yeah, I think it was Dave Archibald's work that really—I'd seen that monograph from there, like, "Wow." And then the other thing is that you'd hear discoveries on the news. Somebody found the world's oldest dinosaur. It would be a Berkeley crew. It seemed like the Berkeley crews are in the media for finding out interesting things about the past life. So I remember hearing a couple of news stories about Berkeley crews on TV and so I thought, "Wow, this Berkeley's a pretty happening place. This is a top place." And so I was so excited to get started that when I graduated from the University of Montana, I had to drive back to Minnesota in the summer and then go back up to Berkeley and get started, figuring I was going to drive back through Montana. I could contact Bill and maybe I could meet up with his crew in eastern Montana. So he said yeah. So he sent me this detailed map and I drove out to

eastern Montana and I located his camp. All these dirt roads and actually met him before I actually enrolled at Berkeley.

01-00:09:09
Burnett: These are pretty remote sites.

01-00:09:10
Lofgren: Very remote.

01-00:09:12
Burnett: Would you describe it that way? You have to—

01-00:09:15
Lofgren: Well, the map is go to this town called Jordan and you go east or west five miles and it turns into a gravel road and you go three more miles and you'll see a big sign that has all these addresses of these ranches and you take a right there and then you go across this cattle grate and then you'll see a big rock up ahead. And then the road will move left then, so you turn left and then eventually, a hundred yards later, you'll see this—it looks like an abandoned cabin to the right and that's where I'm going to be.

01-00:09:48
Burnett: Yeah. So it's like landmarks. You're not using—this is not—

01-00:09:53
Lofgren: It'd be hard to find unless you had a very detailed—

01-00:09:56
Burnett: Instructions.

01-00:09:57
Lofgren: —instructions. So I met him and he showed me around a little bit and had me work with his crew slightly. It was really hot. I just remember it was so hot. I wanted to get out of there so quickly. But I survived a couple of days there and met everybody and then I was off. It was so hot out.

01-00:10:16
Burnett: How hot does it get in eastern Montana?

01-00:10:19
Lofgren: It can get like to about 105 and it's kind of humid. I just remember we had tents on the side of the cabin where we slept outside and they were on the east side of the cabin. So when the sun came over the hill and the sun hit your tent, you were up, man.

01-00:10:33
Burnett: You were up. Yeah.

01-00:10:34
Lofgren: Because it was like an oven in there and it got hotter all through the day. I'll tell you, it was awful.

01-00:10:42
Burnett: It's the Badlands.

01-00:10:43
Lofgren: It was bad.

01-00:10:43
Burnett: It's a kind of inhospitable climate. That's kind of—

01-00:10:49
Lofgren: Certainly it was that those couple of days I was there.

01-00:10:50
Burnett: Right, right. And so you learned about what they were doing and the kind of work that they were doing and you got a sense of what you had gotten yourself into, I suppose.

01-00:11:00
Lofgren: Pretty much, yeah.

01-00:11:02
Burnett: So then you headed back to Minnesota and spent the summer there and then came back out to Berkeley to get started. So can you talk a little bit about how your research project at Berkeley came together?

01-00:11:19
Lofgren: Okay. So I came to Berkeley and I got all set up and I went into the department and announced my arrival and gave my address. And I ran into a couple of professors. I ran into Don Savage. He was a colleague of Bill's. Quite a renowned paleontologist. I explained to him what I wanted to do. I wanted to work on late Cretaceous or early Paleocene mammals and wanted to do some biostratigraphical study on them. He goes, "Oh, Bill's going to love you." And I said, "Well, that's kind of why I applied. I wanted to work with Bill." So once I met up with Bill again, we talked about some specific ideas and he had this long-term project studying the K-T boundary in eastern Montana. So he was wanting me to work on that. So I did some coursework the first year and I did okay. I remember this one class called animal evolution. And I'm a geologist and I'm taking this animal evolution class which is geared toward people from biological sciences. It was really a challenge. I still was able to get an A- in it, which worked my ass off. And Bill was pretty impressed by that, I could survive that class.

So what he would do is he would take people out in the field in the summer to get him started. So 1986 we went out in the field and he was there in western Montana. I had what I would call the Bill Clemens tour, which is quite extensive. He would bring you to all the major localities, introduce you to all the ranchers. Sit on the steps and have tea with the ranchers and collect a little bit. And he'd explain all this stuff, where the oldest, the best T-Rex is found by so and so. And then we'd go over to the other county where I was going to work, McCone County to the east, where they hadn't done that much work

and Howard Hutchison and Laurie Bryant had been working around the McGuire Creek area and so he thought that area could be a good place to study. So he showed me that area, showed me some other areas and kind of let me kind of settle in on what I wanted to do. And I ended up settling in in the McGuire Creek area that had already been partially explored and developed.

So there was some collections that had been made over the previous couple of years. And it was just south of that place, Bug Creek, I had gone with Bob Sloan. And Bob Sloan and Keith Rigby had worked together studying that area and they had decided that in this area there were records of Paleocene dinosaurs. The dinosaurs actually survived into the Paleocene era or epoch, which nobody really thought had happened before. It was a very controversial idea. And so working just a couple of miles south of there, McGuire Creek area is basically the same geological study. Bill wanted me to work there to get at this problem. And the problem being, you've got these massive channel deposits where the rivers cut down, cut a channel, and the removal of a lot of existing rock and some of that rock is Cretaceous with dinosaurs in it. And so the channels are Paleocene but the rock they've cut partly out is Cretaceous. So are the fossils in there Paleocene fossils or Cretaceous fossils? That was the question. Rigby and Sloan were saying that some of these fossils in these Paleocene channels are proof that dinosaurs lived in the Paleocene. That was a question. What are the ages of these channels? What are they trying to tell us? How do they form? As the river erodes, its bank, the heavy stuff settles to the bottom, including bones and it becomes what they call a lag deposit, of kind of gravelly rock with bones in it. And so these lag deposits were what basically we were arguing about. Do they have Cretaceous dinosaurs in them just because they're reworked or did dinosaurs actually live into the Paleocene?

01-00:15:26
Burnett: Right, right. So you can't use the law of super-position that you would if you had—

01-00:15:30
Lofgren: No.

01-00:15:31
Burnett: —stable strata. You can say definitely this is later than this earlier period, which is deeper. That's been cut through and washed out. When it was washed out, you know it's later than all of those rocks but you don't know which fossils have been washed out of which strata and that's the problem that you face.

01-00:15:55
Lofgren: You don't know if the animal died and was washed into the stream or actually was long extinct and washed out of the bank. See, that's the problem. So to get at that what you have to do is a very detailed mapping of all the strata. Took about two summers, about three miles square. Measuring sections of rock and thicknesses of rocks and using a coal bed, which I called the

McGuire Creek Z-Bed, as the common bed that's found in every one of the sections.

01-00:16:29
Burnett: Right. That's your baseline.

01-00:16:30
Lofgren: Baseline. And then mapped out these channels and mapped out where are they interfingered with the flood-plain sediments on the edge of the channel system, which was located there. And it turns out the channels themselves interfingered with Paleocene sediments. And so we thought they were Paleocene. And then the fossils in them were probably Cretaceous but we didn't know. We found this one channel where there were big chunks of dinosaur bone with still mud around it from the cut bank. So pollen was a way to look at that because of the pollen K-T boundary. So we sampled these bones. We actually jacketed out one of them and we scraped some clay off the side of the bone and then we took some clay sample below the deposit and some pollen and a little carbon-rich deposit a little bit above and it turns out that the bones were encased by Cretaceous mud, even though they were in a Paleocene channel. And the Paleocene channel was denoted by pollen found a little higher. So here you had a smoking gun of a Paleocene channel with Cretaceous dinosaur bones dated by pollen, independently dated from the bones themselves. We published that in *Geology* in 1990.

01-00:17:52
Burnett: But you were showing that the bone—

01-00:17:55
Lofgren: Deposits.

01-00:17:57
Burnett: The bone was, or the fossil was actually from the Cretaceous. It was not from the Paleocene.

01-00:18:02
Lofgren: They had been reworked. So the geological story suggested that they had been reworked and then this pollen study, where you had this unusual occurrence where you had the mud still around the bone, showed it was reworked, too. So my dissertation was called the K-T Transition at McGuire Creek, Montana and the Bug Creek problem.

01-00:18:22
Burnett: And that is the Bug Creek. I wanted to ask you what the Bug Creek problem was.

01-00:18:27
Lofgren: Bug Creek problem. Whether these fossils are reworked or not. Frankly, I think it was the Bug Creek problem and the Cretaceous-Tertiary transition at McGuire Creek. I want to have a little mystery in the title. People would go, "What is this Bug Creek problem?"

01-00:18:37
Burnett: Well, exactly. You succeeded.

01-00:18:40
Lofgren: There's a problem out there.

01-00:18:40
Burnett: You succeeded.

01-00:18:41
Lofgren: So Bill had steered me toward this problem. I had one idea of working on Cretaceous rock in Nevada but there wasn't very much and he was saying, "That's not big enough. You need a bigger problem." So he kept steering me to the McGuire Creek section, which I think was a very good idea. But what bothered me at the time is that Bob Sloan and Keith Rigby had been working in that area north of me for years and they had started publishing these papers. And I'm just thinking what if I do all this work and my research agrees with them. It's like it's already been done. And he said, "Don't worry about that."

01-00:19:21
Burnett: Right. Oh, yeah.

01-00:19:22
Lofgren: Very good advice.

01-00:19:23
Burnett: Right. Absolutely.

01-00:19:24
Lofgren: Which has turned out to be good advice. Bill Clemens never criticizes other scientists, almost never.

01-00:19:45
Burnett: Immediately when you told me about their project, the sort of elephant in the room, or fossil in the room, is the impact hypothesis, right, and this question of whether or not dinosaurs survived the transition across the K-T boundary. It's almost too good to be true, right? And so you're wondering, were they theory laden? Were they really looking for that problem? And you're approach was to take this as a question to test and so you were trying to figure out the nature of the evidence because you have to rely on other sorts of evidence. So there's palynology. You can check pollen. And in that area dating is not as easily done. That's the sort of rock dating. Can you talk about that as a kind of evidence? Have you ever done that kind of work or is that not possible in the areas that you were doing research?

01-00:20:58
Lofgren: Well, it was hard to find any volcanic ashes in the sediments there. The interval I'm talking about is only about sixty feet and so it's a very small amount of time geologically when this channel system was active there and it's really hard to put any dates on it. You can find an ash bed. There's coal

below it, this interval called a null coal, and there was an ash bed in there but we couldn't get any stuff to date out of it. Now it's been dated by other people but at that time the technology was such that we couldn't find enough material to date. And then the coal above it was clearly Paleocene anyway, so nobody had really taken the time to date it. But they have now. They've been dating all the rocks a lot more thoroughly now because the techniques are better. So it was a small piece of time. And it was a transition for sure. Just a question of how do the channels fit into this transition and the fossils that were contained in the channels. And so that was the Bug Creek problem. And so when Rigby and Sloan started publishing these papers about Paleocene dinosaurs it got a lot of attention because it's good evidence that maybe the asteroid impact didn't affect the dinosaurs that much because they lived into the Paleocene. So a lot of people were interested in this issue besides just paleontologists. And so the impact theory first was published in 1980. So this is like the late 1980s. So it's been going on for a while. And so I wasn't privy to all that earlier stuff but Bill was at the very beginning of it all. And so this is another big piece of it. Are these Paleocene dinosaur records really that robust or are we talking about pretty fuzzy data? And it turned out to be very fuzzy. Like in the case I had with that one channel, I could show for sure that these dinosaur bones had been reworked.

01-00:22:59
Burnett: Yeah. Can you talk a little bit about how the impact hypotheses impacted the work at Berkeley? In your view, did it stimulate research or did it cause some blind alleys? Did people get overly exercised about the impact hypothesis, in your view?

01-00:23:27
Lofgren: Well, I wasn't there at the time but what happened is that they published a paper about the impact hypothesis based on marine rocks in Italy. And so Bill had been working in eastern Montana where the transition was from essentially the Hell Creek Formation to the Tullock Formation, the Cretaceous-Tertiary boundary section there. Not thinking about the boundary that much. And so here was a chance to take that extinction hypothesis based on rocks deposited in the ocean and look at the same sequence and see if we can see the same effects on terrestrial rocks, rocks deposited on land, where Bill was working. So Bill was basically right there at the ground floor of testing this hypothesis on rocks in a terrestrial environment. Archibald was already working there. So there was a lot of work that had already been done, a lot of data collected that they could use to test this. And then they went to try to find iridium in that area and they found it. And so, whoa, it just kind of took off from that. So Bill was on the ground floor. Where I worked in eastern Montana they never found iridium because it's not the same coal. People used to say that that Z coal was this Z coal and, no. There are localized coal beds and the same one that occurs in one county might not be the same one in another place. So yeah, Bill was on the ground floor with that. But I came

later looking at the specific Paleocene dinosaur question. There's a lot of very hostile talk between groups of people back early nineteen—

01-00:25:06
Burnett: Yeah, it was a dramatic—

01-00:25:07
Lofgren: Very dramatic.

01-00:25:08
Burnett: —period that's since sort of simmered down a little bit.

01-00:25:13
Lofgren: Yeah. Yeah. You know who would know a lot about it is Lowell Dingus. I don't know if he's on your list.

01-00:25:18
Burnett: Yeah, yeah. Definitely. So you solved the Bug Creek problem or presented it.

01-00:25:29
Lofgren: We never used the word proof. The evidence I presented was robust enough that I think people would not think that there was Paleocene dinosaurs anymore in eastern Montana. And nobody's ever published anything since about it. Guess they believed me.

01-00:25:46
Burnett: I said solve because—

01-00:25:47
Lofgren: For now.

01-00:25:48
Burnett: —I was thinking of your analogy. It's like detective work. It's this sort of puzzle-solving activity that is—

01-00:25:58
Lofgren: Case is never closed.

01-00:25:58
Burnett: It's the ultimate cold case.

01-00:26:02
Lofgren: New data can come up and show you you're wrong. It's possible.

01-00:26:07
Burnett: So you did this project and published it in 1990.

01-00:26:19
Lofgren: Ninety-five.

01-00:26:20
Burnett: Oh, '95. Okay. In 1995.

01-00:26:24
Lofgren: The geology paper on the reworking, specific to that one site, was in 1990.

01-00:26:32
Burnett: So the next step usually for folks is to go on the job market. Did you want to become a professor? What was your plan at the time?

01-00:26:44
Lofgren: The plan was just to find a job, hopefully in a university or a museum where I could do the biostratigraphic work that I was interested in. So I actually had a two-year position, temporary position, at Berkeley. By the time I graduated I got this two-year position to get the collections ready to move them to the new building where they're in now. So it was a tremendous amount of curation to do, which I thought I was pretty good at, pretty conscientious. And so I was preparing collections for about a year to move them to the other building somewhere down the road, applying for jobs, anything that would come up, and getting no response. Because I really only had one major publication at that point. And a monograph like mine, dissertation, if you want it published as a book by UC Press, which Archibald did, you have to submit to UC Press within a year after you basically graduate. So I had submitted it there and so it was kind of in that black hole that it was not going to be published for a while. And so I was looking for jobs and I didn't get much action.

And then finally one day I went to the board and there was a job posted, new one, said Raymond Alf Museum. They needed a new director. I had never heard of it. It was in southern California. So I asked around. Howard Hutchison told me it was a museum in a high school founded by Ray Alf, who he knew vaguely, I guess. Mike Woodbourne was the chair of the search, who I knew a little bit, so I called him up and he told me, "Oh, yeah, you should come down, take a look at it." So I said, "Okay." So I drove down like the next week and met Mike and then we drove over and looked at it. And I was shocked. It's on a high school campus, beautiful high school campus on the edge of the LA Basin in Claremont, California. But it's much bigger than I expected. I expected a couple rooms. It's a high school. And I remember opening the door. It's a round building, too. Part of it was round and that's where the museum—I opened the door and walked into the upstairs exhibit hall and there's an Allosaurus skeleton right there. I go, "Wow, they got an Allosaurus skeleton. We don't have one at Berkeley." And it was big. But you could tell it was pretty run down. Didn't seem like it's being well taken care of and the collections are a mess, a real mess.

01-00:29:28
Burnett: How old is the collection? Where does this come from?

01-00:29:31
Lofgren: It goes back to 1936. A teacher, Ray Alf, who founded the museum, came to the Webb School in Claremont in 1929 and he got interested in geology around 1930s and he went out on a trip to Barstow, one of his very first trips,

and one of his students found a new species, a fossil peccary and he brought it down to Chester Stock at CalTech, who published it that year as a new genus even, fossil peccary. So that got Ray really excited about paleo. He did a summer trip with a couple of students, ran into John Clark from the University of Colorado, another paleontologist, who inspired him to go back to school for a year and study with John Clark, get his master's degree in one year, which is quite a feat. And then he came back to Webb and stayed for the rest of his life collecting fossils with students. Super ambitious guy. Very charismatic. Inspired some of his students to become famous paleontologists, like Malcolm McKenna. When he graduated from Webb he was going to be an electrical engineer and decided to be a paleontologist. Went to CalTech to study with Stock. Stock died, heart attack, and he transferred here to UCMP, Berkeley, got his PhD here, stayed on to teach. This guy's brilliant, Malcolm McKenna. And eventually he was recruited by the American Museum [of Natural History] and spent the rest of his career there. So anyway, so Alf had created this museum in a high school that nobody ever thought was possible.

01-00:30:39
Burnett: It's fascinating. Did he have field expeditions with his students?

01-00:31:11
Lofgren: All the time. Weekends to Barstow. Month-end trips, like five, six, eight trips to the Grand Canyon, to Salt Lake in Arizona, to Kanab, Utah. He had kind of a circuit. Summer trips he'd go way out for six weeks all the way to South Dakota, places out in Wyoming, Nebraska, six to eight trips he'd go to the Grand Canyon, Kanab, Utah, southern Utah, northern Arizona. Weekend trips Barstow, which was close. So he did so many trips. So many are undocumented but he amassed this huge collection of fossils.

01-00:31:45
Burnett: And did he do fundraising to get money to help the kids do these kinds of expeditions?

01-00:31:54
Lofgren: He got donations from parents or alumni. Like field vehicles. And eventually he had a little museum in the basement of the old library and it was completely full of fossils and they had to store fossils all over the place, in a stable, garages, Quonset huts. So they built a museum in 1968, named it after him, and all these alumni, or his former student were alumni, basically gave back by funding this thing. Like George Getty, class of '42, was in his dorm. So everybody's heard of the Gettys, right?

01-00:32:31
Burnett: Yeah, sure.

01-00:32:31
Lofgren: This is one of the sons of J. Paul. So he gave 100,000 for the project and other alums gave 50,000. That's a lot of money in the late sixties.

01-00:32:39
Burnett: Yeah, for sure.

01-00:32:41
Lofgren: So they built this museum dedicated to him and then he had to create all the exhibits and he had to deal with the collections. So he just was overwhelmed. He couldn't do all that. That's why the collections were in a mess. And then they were trying to fix it but it was a long ways from being professionally organized. So when I first got there, that was my job. So I didn't publish any research the first ten years I was there, except for my dissertation, which had come back from review a couple of years later. Took me about a year to revise it and send it back in. So that's why it didn't publish until 1995. When I send it out for review the first time I didn't see it for two years. Then I had it for about a year, then it went for review again, came back as accepted and took me about a year to have it all formatted and printed. So that was my only research publication. The first ten years that was it, the new job because there was just too many other things to do.

01-00:33:43
Burnett: Yeah, yeah. And some kind of teaching role, as well?

01-00:33:48
Lofgren: Lots of teaching.

01-00:33:49
Burnett: Lots of teaching. Right. So that's part of the gig.

01-00:33:53
Lofgren: A lot of teaching. A lot of different jobs. So yeah. It was tough.

01-00:33:57
Burnett: And so the collection. Roughly how many specimens are we talking about here?

01-00:34:02
Lofgren: About 150,000.

01-00:34:03
Burnett: A hundred and fifty thousand.

01-00:34:05
Lofgren: But both exhibit halls needed renovation. The collections needed to be completely reorganized. The classes that were taught needed to be revised and modernized and it was a long, long struggle. Slow progress. I almost left there about 1995, would have been about four years. There was actually a job at Berkeley here, kind of a collections manager job, and I was thinking about applying for it. And a couple of my friends, Mark Goodwin, suggested I apply for it but I decided not to. At the end I needed to—

01-00:34:45
Burnett: To stick it out.

01-00:34:47
Lofgren: Stick it out and see it through.

01-00:34:50
Burnett: It sounds like a pretty unique institution. It's a private high school that has an extraordinary collection and a connection to a kind of university, a very specific kind of university, kind of graduate research. Right? This is the kind of research that you would do at the graduate level or at least at the senior undergraduate level and yet it's being done in high school. Could you talk a little bit about how students respond to this kind of exposure? Does it—

01-00:35:32
Lofgren: Well, Ray Alf, when he would take students out and Grant Meyer, too, his successor before I came there, they would collect a lot of fossils. And a lot of fossils need to be prepared before they can be put on the collections. The kids would help prepare them and they'd put on exhibits. Kids would help build exhibits. But that's it. They didn't really study the fossils that they found. Ray Alf had very little time to study fossils. He did some publications. Grant Meyer did almost no publications. So the collection hadn't really been studied very well. So the idea was to try to take it to the next step. Students had been through all these parts. Let's take it one more step further and have them study the fossils that they find and then we can publish a paper with them, because it's too technical for them to write the papers but they can do a lot of the thinking and data gathering and problem solving. And they're pretty smart kids at a private high school that are all college bound. And so it's just a question of time. So I had a little bit of time here and there and I had a couple of students and we put out a paper or two. We started to get a little more financial base and then in 2008 we hired Andy Farke as our curator paleo and then it kind of took off, kind of fell together. We started a real research class and now we have like twenty students involved in projects and we've published, since about the last twelve years, about twenty-eight papers with student co-authors.

01-00:37:02
Burnett: Really? That's amazing.

01-00:37:03
Lofgren: Yeah. Beyond the papers that we do without student co-authors.

01-00:37:06
Burnett: And so it functions as a kind of honors class that students—

01-00:37:08
Lofgren: Yeah, it's very selective. They do a class called honors paleo, which is basically like a museum studies course focusing on paleo and then they get to the second half, which is focusing on the research part of museum studies. And then the top kids from that are selected to go in and do advance research and they take these classes at tenth grade. And then if they're selected, in eleventh and twelfth grade, for two years they're working on projects. And

they don't work like graduate students, where you can work ten hours straight on a project, which is the way you should do it. They work eighty minutes here, eighty minutes there. So it's kind of hard to keep the momentum going.

01-00:37:46
Burnett: That is. That is. And they have all of their other classes that they have to succeed in.

01-00:37:49
Lofgren: Yeah, right.

01-00:37:50
Burnett: So there's—

01-00:37050
Lofgren: And sports. Yeah.

01-00:37:53
Burnett: And some of them go on to paleontology? Do some continue on?

01-00:37:58
Lofgren: Some, yeah. Some are interested in sciences. All of them pretty much are interested in sciences. But yeah. There's one kid that's a senior at Montana State University, wants to be a paleontologist. But, yeah, it's a small field, paleo. They get just the experience of critical thinking, working with specimens, scientific rigor.

01-00:38:20
Burnett: Well, I think if you had to choose a science to get young people excited about science, you'd think that would be it, right?

01-00:38:30
Lofgren: It is. It's a thing that Malcolm McKenna told me a long time—I just never forgot. He says, "Paleo is a science that a kid can get to the edge of knowledge of pretty fast compared to chemistry or physics." You can find a fossil of some organism and study it and find out that really in a lot of cases, it's really never been studied very well and you can actually—

01-00:39:59
Burnett: Advance knowledge.

01-00:39:00
Lofgren: —advance knowledge by your study of this thing. And there's so many examples of that and that's exactly what they're doing.

01-00:39:06
Burnett: Not only that. It's kind of like a gateway drug for the sciences, right, because you start working in that area and then you can go into biology and you can go further into geology. And now with the various techniques of dating there's also physics and chemistry, right? All of these sciences at some point intersect with paleontology and so you can see how that would be—that's a great

model for education, for science. I know that you've written a couple of papers about the pedagogical side of your career. I can't remember the phrase you had. It was something about like a student-scientist. And have you kind of gone out to talk to education communities about this kind of work?

01-00:40:10
Lofgren: Somewhat. If you go to a high school conference and talk about—nobody else can do this because we're the only ones that have a big museum so they're kind of limited. I've given a couple of talks, yeah, but we're in a unique spot where we have the facility, we have the professional staff, and we have these really gifted students. All those three things kind of come together. And it's really unique because people don't have that facility which is comparable to anything you'd see at a college or university.

01-00:40:47
Burnett: I wanted to ask you a little bit to kind of go back to talking about Bill Clemens and UCMP. You continued to work with Bill Clemens after you started working at the Webb School. Can you talk about the NSF-funded expeditions in Montana in the nineties? Can you talk about that work?

01-00:41:17
Lofgren: Yeah. Bill asked me to help with the project he was working on, kind of a continuation of what I was doing, looking at some of the fossil localities in the lower part of the Tullock Formation, which is just above the K-T boundary. And he had a couple of guys that were working with him to date the rocks and to sample sediment for paleo magnetic polarity, either to get a reversal or a normal, and you can use these two things, ash dating and paleo mag to try to more precisely date the rocks beyond just the fossils themselves. So I was helping him map the rocks a little bit more. I basically extended my mapping a little bit up higher to map some of the marker beds and help collect some of the samples, fossil samples that he wanted to develop more. That was the extent of my part of that. I really wasn't that much involved in it. But Carl Swisher was involved and Lowell Dingus and Barry Albright and now they're continuing that work right now. So Bill has worked in this eastern Montana area since the seventies. So he knows everything about it and he's still out there pushing it forward.

01-00:42:43
Burnett: Right, right. One of the things that's striking about paleontology is that it's field research and there's a whole social and cultural aspect to that. You talked about how there's a romance for you when you were first starting out. It was either forestry or something outside and there's something about being outside that's important. And then the other feature of it, too, is what appears to me to be a kind of meticulous, right. That you have to keep pretty strict records. You were talking about the work that you had done. The location is so important and making sure everything is—could you talk a little bit about work practice that you were doing both in the field and how it relates to the collection at the

Alf Museum and how that fits into your pedagogy as well when you're teaching students about paleontology.

01-00:43:45
Lofgren: Well, when you're out doing field work and you're collecting things, you have to record exactly where it's from because fossils without exact locality information aren't that valuable for scientific study because you have to be able to go back to the rock and say it was found right here because you start mapping the rocks and stuff. You have to know the relationship among the rock layers. And if you don't know exactly what layer it's from it's fuzzy. And you want very precise data. So I learned very early on, especially from Howard Hutchison at Berkeley, who was one of the museum scientists, to be very meticulous about where you collect fossils and how you record it because it's so critical for the future. I worked on the collections, too, at Berkeley as a part-time job and you'd see the sloppy work with the notes. It was very frustrating because it brings into question some of the fossils and where they're from and that's not what you want. You want it to be solid. Take pictures of the site, label exactly where they are. Meticulous field notes. Measure a section, noting all the layers and point out particularly this is finding this layer or this spot and that's the kind of rigor you need to have to be a professional museum collection, which Berkeley has. And so that tradition of just super-tight data is really important. It's the thing I stress to my students now when we go out and collect fossils for their first time. I say, "You must record the stuff in your notes immediately. You must label everything right there. If you don't, you're asking for trouble," because you end up with mistakes.

01-00:45:41
Burnett: You're losing things. You're losing information right there.

01-00:45:43
Lofgren: You're losing information that you could have easily had and you look at your notes ten years later and you go, "Oh, this is kind of uncertain." You can't just run back. You should be able to look at it and say, "This is it." This is one thing Bill taught me, is that he worked slowly and carefully and everything he does is very meticulous. That's good science. People can rush and throw these papers together and then you read them later and they're full of mistakes. You can find them. You don't find that in Bill's work. He's very meticulous and so everything is very laid out, especially in the explanations. It's very laid out so you can follow it. I've had some people tell me they've read my dissertation, the monograph, and not many people read it, I'll tell you that right now. But they say it's really clear, really clearly written and you can follow along. That's what you want. You want it to be easily absorbed so they can get the message.

01-00:46:55
Burnett: I imagine some of the practice is standardized across paleontological work worldwide, I imagine. I don't know if I'm right about that.

01-00:47:07
Lofgren: Somewhat.

01-00:47:08
Burnett: But is it more program-to-program or even scientist to scientist? It's almost craft knowledge, right? No?

01-00:47:20
Lofgren: It's not taught.

01-00:47:29
Burnett: It's not taught.

01-00:47:24
Lofgren: It's just basically you pick it up.

01-00:47:25
Burnett: You pick it up.

01-00:47:26
Lofgren: And everybody has a little bit of different styles. Some people are a little more worried than others. When I took my first draft of my dissertation into my three committee members, one was a palynologist and he didn't want to read it. And Don Savage didn't want to read it. He read the whole thing and didn't even have a comment on it. But Bill Clemens, he really dissected it and he made a lot of good suggestions and clarifications. Where things weren't clear. You've got to be very clear but very succinct. That's research writing. And journals don't want a lot of fluffy—because good data, they want it written really clearly but short. And it's hard to do that. It takes a lot of experience and a lot of rewriting it to get it to that publishable level. And that's not taught. You learn.

01-00:48:31
Burnett: By emulation to some degree?

01-00:48:30
Lofgren: Yeah. You emulate Bill pretty much.

01-00:48:36
Burnett: That's it. Yeah.

01-00:48:36
Lofgren: And you want to be very thorough and very careful and make sure you explain everything really well. So when you get it back from review people aren't going, "Well, you missed this or what about that?" And you have all that covered. They can still have comments about your interpretation of stuff but they're not saying, "There's a mistake in this chart or you didn't reference this work." It's all there.

01-00:49:06
Burnett: I imagine that's the big challenge, too, with teaching when it comes to your students. As you said, that's not taught in paleontology but it has to somehow sink in with the students that you have to, right?

01-00:49:27
Lofgren: You got to be meticulous. If you're a scatterbrained person you're not going to make a very good scientist. You may have some great ideas but you may have a hard time writing them down where they're publishable. It takes a lot. It's not fun to write papers. I don't like it. It's hard.

01-00:49:46
Burnett: It's fun to finish them.

01-00:49:49
Lofgren: It's fun to find the fossils and study them maybe. But writing them up is hard. The figures and all the tables. It's a real grind.

01-00:50:01
Burnett: Because your work has been up in Montana but you've also done a lot of work in California. Can you talk about the Goler Formation and what makes California interesting from a geological or a biostratigraphic perspective?

01-00:50:21
Lofgren: Well, California has a lot of different-aged rocks. There's a lot of what we call Tertiary rocks and part of the Tertiary is the Miocene. And this is a time when you had a lot of camels and horses and peccaries and elephants running around what is now deserts. It was a more lush area. And that area's been studied a lot and we do a lot of work in that, especially a place called Barstow. But there's one unique area in the El Paso Mountains and there's a formation there called the Goler Formation. The Goler Formation is Paleocene in age and there's no other area in the West Coast in North America that has a very thick section of Paleocene rock that was deposited on land, except for this one spot in the El Paso Mountains. And Malcolm McKenna had first worked in there back in the fifties. He found the first mammal fossil from there. But the fossils are so hard to find that he searched on and off for thirty-eight years and found about twelve specimens.

01-00:51:29
Burnett: Wow.

01-00:51:30
Lofgren: I know. It's just mind-blowing.

01-00:51:33
Burnett: That is. Yeah.

01-00:51:33
Lofgren: So when I first came from Berkeley down to the Alf Museum, Malcolm McKenna was on the board there, board of trustees. And he wasn't

particularly that nice to me when I first met him, couple of times. He was a very famous paleontologist and I'm just some grad student from Berkeley. But when I mentioned I was interested in the Goler Formation, I had no idea he was obsessed with it. Here's a guy that could work anywhere in the world and he worked in the Goler Formation. So it meant a lot to him. He wanted to figure out the Paleocene fauna of California and this is the only place you could do it. And he'd been working on it for years and years and years and having little success. So we started working together on it, without students at first, and had a little bit of success at the beginning and then we totally zeroed out and I was about ready to quit but he wouldn't let me quit. So then he brought in some guy, Steve Walsh, to help start screen washing sediment, sieving, and we found a couple of good localities. A guy named Bob Baum, one of the alums of the school, would come out with us. His great-grandfather was L. Frank Baum, who wrote the *Wizard of Oz*. And Bob found out this locality that turned out to be the best locality we ever found in the Goler and we now call it Oz, the mystical Oz locality.

01-00:52:49
Burnett: [laughter] That's great.

01-00:52:52
Lofgren: Yeah. We found some great stuff there. So kind of took off around 1999 and we collected all these fossils over the next twenty years and then we've been publishing papers on it and basically describing the vertebrate paleontology of Paleocene of California based on this one area, which nobody really thought was going to be possible because you couldn't find anything there, but it's damn difficult. But yeah, we've had a lot of success. Malcolm died in 2008 but we're still publishing papers with him as co-author because he was the heart and soul of it, gathering all this data, and studying it, and Webb students are involved, too.

01-00:53:30
Burnett: That's wonderful.

01-00:53:33
Lofgren: So yeah. It's probably our most important project, at least for me right now since the K-T Boundary stuff.

01-00:53:39
Burnett: Yeah. Is it remote from Claremont?

01-00:53:45
Lofgren: It's about two and a half hours north. There's dirt roads that run through it. It's pretty accessible. Part of it's wilderness so you can't drive so you have to do a lot of walking. Part of it's not wilderness. But the fossils are really hard to find. You have to be very diligent to find anything. But when you find something it's usually pretty good.

01-00:54:04
Burnett: And these are mostly the—

01-00:54:06
Lofgren: Little mammal jaws.

01-00:54:08
Burnett: Smaller. Yeah, like smaller, like the kind of rodent-type—

01-00:54:10
Lofgren: A primate jaw might be that long [showing a length of about an inch]. When it's covered up with rock in a concretion, these hard nodules that have fossils in the middle, and you can just see a little tiny tooth sticking up. You have to look at a thousand of these concretions, sometimes with a little hand lens, and all of a sudden you see a little bump of a tooth sticking out. "Hey, there's— "Then you prepare it and here's an old jaw. New species. But it's really tough. I understand how Malcolm got obsessed with it because it kind of grabs you, this project, because you know there's more to get.

01-00:54:58
Burnett: Yeah, it's there.

01-00:54:59
Lofgren: There's more there and you just need to work a little bit harder to get that story, find the truth. Nothing he liked better than a geologic mystery. And so this was a big mystery and so he just could never let it go and I feel the same way. I can't wait to get back up there, run around looking for one fossil per day.

01-00:55:12
Burnett: That sounds like a good place to stop for now. Thank you very much for your time. I really appreciate your talking to us.

01-00:55:19
Lofgren: Sure. Okay.

[End Audio File 1]

Oral History Center
The Bancroft Library

University of California
Berkeley, California

The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology:

David Polly

Interviews conducted by
Paul Burnett
in 2015

Since 1954 the Oral History Center of the Bancroft Library, formerly the Regional Oral History Office, has been interviewing leading participants in or well-placed witnesses to major events in the development of Northern California, the West, and the nation. Oral History is a method of collecting historical information through tape-recorded interviews between a narrator with firsthand knowledge of historically significant events and a well-informed interviewer, with the goal of preserving substantive additions to the historical record. The tape recording is transcribed, lightly edited for continuity and clarity, and reviewed by the interviewee. The corrected manuscript is bound with photographs and illustrative materials and placed in The Bancroft Library at the University of California, Berkeley, and in other research collections for scholarly use. Because it is primary material, oral history is not intended to present the final, verified, or complete narrative of events. It is a spoken account, offered by the interviewee in response to questioning, and as such it is reflective, partisan, deeply involved, and irreplaceable.

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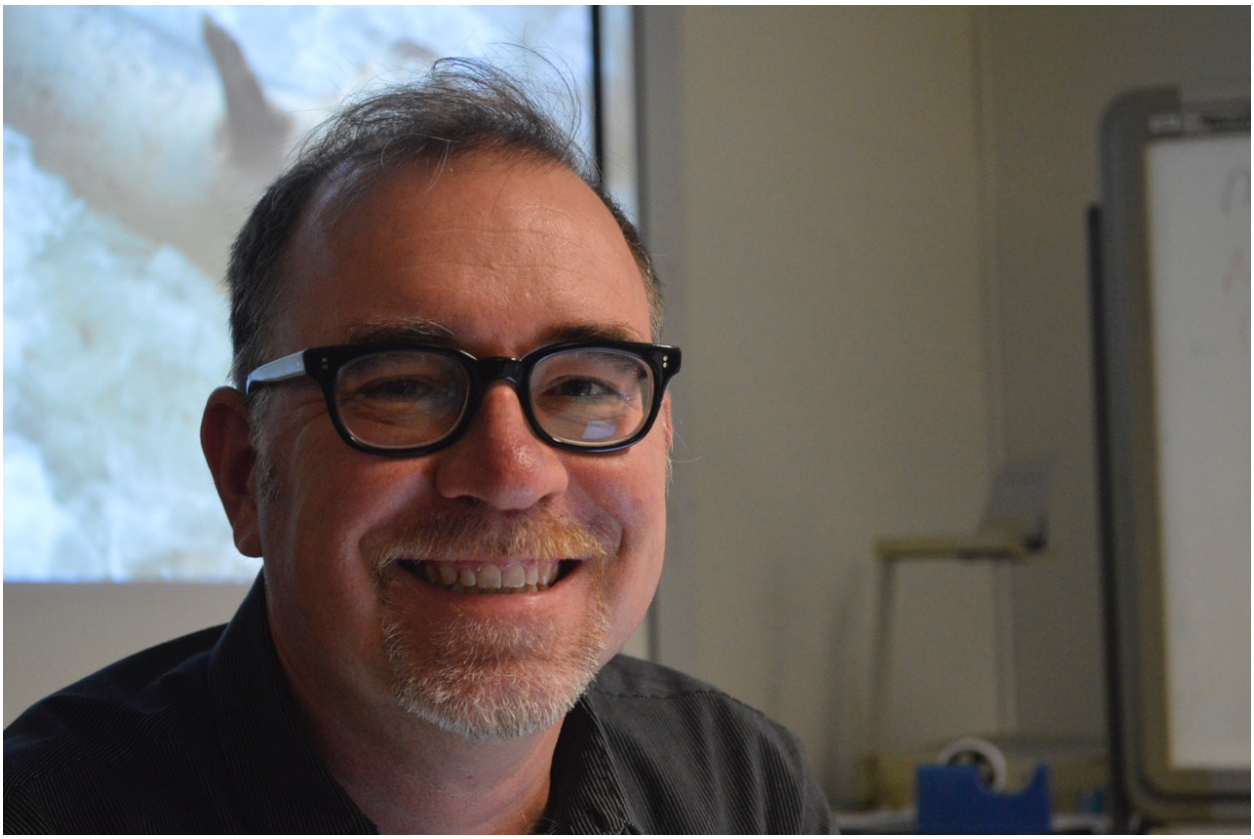
It is recommended that this oral history be cited as follows:

David Polly "David Polly: The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology" conducted by Paul Burnett interviewer in 2015, Oral History Center of the Bancroft Library, The Bancroft Library, University of California, Berkeley, 2017.



David Polly

David Polly is Professor of Geological Sciences and Curator of the Indiana University Bloomington Collection of Paleontology. He received his PhD in Paleontology from UC Berkeley in 1993.

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01-00:00:00
Burnett: This is Paul Burnett interviewing Dr. David Polly for the Bill Clemens/ UCMP Oral History Project. It's April 10, 2015 and we're here in the Bancroft Library. Welcome, Dr. Polly.

01-00:00:17
Polly: Thank you.

01-00:00:19
Burnett: So I'd like to ask you where you were born and where you grew up.

01-00:00:25
Polly: I was born in Jefferson City, Missouri and grew up on a farm six miles south of Boonville, Missouri.

01-00:00:34
Burnett: How big was Boonville?

01-00:00:34
Polly: Not very big, as the name sounds. About 6,000 people.

01-00:00:40
Burnett: Okay. And how did you become interested in science?

01-00:00:47
Polly: I don't know if I have a good answer to that. When I was a kid I was interested in paleontology but by the time I graduated from high school I wasn't, or certainly it wasn't my main interest. And, in fact, when I started university I had decided, after having lots of science in high school, that I did not want to do science. But, first from taking an anthropology course which had some paleoanthropology and then taking my first paleo course from somebody who was also a graduate of Berkeley, got interested in it and then suddenly switched everything I was doing and went—

01-00:01:28
Burnett: So that's when you were in your undergraduate?

01-00:01:30
Polly: That's when I was in my undergraduate, yes.

01-00:01:32
Burnett: Okay. So you went to the University of Texas?

01-00:01:34
Polly: Texas, yes.

01-00:01:37
Burnett: So you started off in the liberal arts?

01-00:01:40
Polly: I was in an honors program that was both arts and sciences and one of its main features was that it waived all prerequisites and you could take courses across whatever you and your adviser thought was the track you wanted to go.

01-00:01:59
Burnett: By your second year you were gravitating more and more towards the sciences?

01-00:02:06
Polly: Yeah. Probably even getting close to third year. Sort of went through history and then a little bit of anthropology and then zoology and geology.

01-00:02:17
Burnett: And at what point did you decide that you wanted to be a paleontologist?

01-00:02:23
Polly: Certainly by the time I applied to grad school but I'm not sure exactly. After that first course, which was with Tim Rowe, who was a student of Kevin Padian's and also worked with Bill, I did my senior thesis with him on phylogenetics of carnivores. So when would that have been? The thesis was the summer between my junior and senior year, so it probably would have been in the year before that. So my junior year, more or less.

01-00:02:55
Burnett: Did you go into the field?

01-00:02:57
Polly: I did some, not so much as an undergraduate but in the early years of being a graduate student I went with Tim, Big Ben National Park in Texas, went out with Bill and Mark Goodwin and the crews from UCMP to Montana. Went out with Dave Archibald, one of Bill's former students, to western Colorado. So I did quite a bit of fieldwork there. But I hadn't really done much other than either courses where we went out in the field or when I was a kid growing up collecting fossils.

01-00:03:33
Burnett: And when you decided to go to graduate school was it clear to you that you wanted to go to Berkeley or did you apply to a number of places?

01-00:03:47
Polly: The two places that I vied back and forth with were Michigan and Berkeley. I did apply to one or two more, mostly because when I went to ask people for references they said, "Well, these are two of the best schools. Where else are you applying?" And if I had not gotten into one of those two I don't know whether I would have gone.

01-00:04:17
Burnett: So you applied to the two top schools and a couple of others and Berkeley said yes. And did you know at that time what you were going to do or was it—

01-00:04:32
Polly: I was interested in phylogeny in mammals, which is more or less what I did. So in that sense yes. And was also interested in carnivores, having done an undergraduate project on them, although I had kind of chosen them in a very specific moment, when I was asked what I was going to do my project on. But beyond that general feel, no.

01-00:05:01
Burnett: Right, right. And so you mentioned going into the field. That wasn't a major part of your work it seems?

01-00:05:09
Polly: No. So yes. I've done quite a bit of work in the field but I have seldom published things that are based on the work that I've done in the field.

01-00:05:18
Burnett: And so in your graduate training that was much more formation. Like you were going out to learn about being in the field and do some work and participating—

01-00:05:28
Polly: Well, yes. Enjoying the field work aspects of it.

01-00:05:30
Burnett: Yeah, yeah. So that's not really your approach?

01-00:05:35
Polly: My research has always been, as you probably know—if you're doing a field-based project there are certain sorts of things that you can focus on with that, addressing a particular interval of time or a fauna or something like that. But since I was doing phylogenetics, where the different animals are from all over the world there wasn't much way that I could actually collect what I needed for those data. So most of my data have come from research collections, which they still do.

01-00:06:09
Burnett: Right, right. It's been mentioned that while you were at Berkeley you were helping out with the UCMP's website. Is that correct?

01-00:06:22
Polly: Right, yes.

01-00:06:23
Burnett: Yeah. Can you tell me a little bit about the origins of that project?

01-00:06:27
Polly: Well, Rob Guralnick and I started it. Rob, when I first met him, was an undergraduate who was working for Tony Barnosky. Tony was using an early ARC GIS system, which ran on a Linux server, and Rob ran the server for him. Rob was an undergraduate, had been in psychology but after being in the UCMP he decided he wanted to switch focus and come and do paleo and was originally going to be Tony's student. So he had been doing that job but he was going to start graduate school and he went out in the field with Tony and Liz, Liz Hadly, and they then ended up hiring me in his place. And before he left he said, "There's this new thing I've heard about, the web, and we should start a webserver when I come back. At least look into it, see if it's feasible." And so while he was gone, out of mischief I got into it and started it. So he came back and said, "Well, we should start thinking about this now." And I said, "Well, Rob, why don't you type in ucmp1.berkeley.edu and see what you find. So we just kind of started it. And it wasn't a project per se. In fact, we got yelled at a little bit for doing this other thing instead of what we were really supposed to be doing. And over the course of that semester Tony Fiorillo, I guess he was a research scientist at the time, started helping us, and Al Collins, who was another graduate student. And by December essentially the four of us had built the prototype of what is today the UCMP site. And it was one of the first hundred sites in the world. There were certainly no other webservers on the Berkeley campus at all.

01-00:08:22
Burnett: And this was what year?

01-00:08:24
Polly: This was August '93 was when it first went online, and by December of '93 we'd actually redesigned it about three times by then.

01-00:08:34
Burnett: Wow. Yeah.

01-00:08:37
Polly: In fact, when Rob was gone and I was installing the software, that's when you had to FTP it down and you would compile it and so on. And so if you needed help, if something wasn't working, you would usually write to the person who wrote the software, who was a guy in Switzerland, Tim Berners-Lee, who I emailed back and he'd tell me how to configure things.

01-00:08:58
Burnett: [laughter] That's just hard to fathom, that it was that kind of intimate time.

01-00:09:06
Polly: Yes, it was a small world.

01-00:09:09
Burnett: The World Wide Web was a little community.

01-00:09:12
Polly: We did a site redesign two or three times, and Rob and I, each time, we would just sit down and look at everything that was on the web. And you could look at literally everything and it would take you about an hour and say what we did or did not like about other sites.

01-00:09:26
Burnett: Right, right. And then it grew to this amazing resource right now. It's one of the largest, I understand, of paleontological collections.

01-00:09:33
Polly: Yeah, it is still a big and important site. And at the time it ended up by accident being really popular because, for various reasons, the media started getting really interested and we were one of the few sites with very many pictures at all and one of the few that had more resonance with the general public, I think.

01-00:09:56
Burnett: And education outreach potential. This is something that teachers could use in teaching classes.

01-00:10:02
Polly: Yeah. In fact, our original philosophy was really to, on the Web, meld the rather clinical and technical specimen catalogues with some interpretative information that anyone could approach and also with some history of science components. So it kind of had three. A geological component, a history of life component, and a history of science component. And as you dig down into it, eventually you get to collection catalogue with real specimens that were related to whatever research that was.

01-00:10:35
Burnett: And they seem to have preserved that model. There's a lot of history of UCMP there and there's a lot of different resources that are useful to different constituents. So can you talk a little bit about Bill and how you met him and how he helped work out what became your dissertation project?

01-00:10:58
Polly: I didn't really know him before starting -- I knew of him. He had been recommended and I had read some of his work. What I was interested in was not primarily what he was interested in but he was one of the main people working on fossil mammals. And I was originally mostly interested in the radiation of mammals at the end of the Cretaceous and the early Cenozoic, which he'd done a lot of work on. My dissertation ended up being much more specific than that but that's where it grew out of. And I learned a lot of functional morphology from him, a lot of fossil mammal diversity.

The two classes I remember best for him were a functional morphology, and I don't really remember what his title was, but it was looking at skeletons and interpreting them from these living organisms. And the other one was his

mammals class, which was a very deep and very broad crash course into the technical literature of fossil mammals, which was just extraordinarily important. But then also his other students and the connections with them and introducing me to other people who could help with specific areas.

01-00:12:20
Burnett: Were there people in your cohort, a few years ahead of you and behind you that you developed relationships with early on that continue?

01-00:12:31
Polly: Yes. I guess I still know most of the people who were there. Zhe-Xi Luo was just in his last year when I started and really took me under his wing when I started. At that time he was working on actually quite detailed things on mammal teeth and he taught me a lot of things about them. Nancy Simmons, who now works on bats but at the time was working with Bill on multituberculates, was very influential on me in the early days. Ken Warheit, who was working on fossil birds, a student of Kevin Padian's, but also very interested in phylogeny and morphometrics. Yes, and I could probably just keep going on and on and on.

01-00:13:20
Burnett: Right. It seems to be a really strong bond among the UCMP folks. They collaborate on projects together even though they're dispersed across the globe. It's a really important community.

01-00:13:32
Polly: Those who came in after me were Jessica Theodor and Anne Weil. Overlapped with quite a long time. And actually somebody who wasn't a student but was here visiting Bill, C.B. Wood, Craig Wood. I learned an awful lot from him. He was working with Bill on Mesozoic teeth.

01-00:13:54
Burnett: Right. Can you talk a little bit about the development of the dissertation project, your interest in phylogeny and some of the things that were in the air at the time in phylogeny that were developing as you were doing your dissertation?

01-00:14:11
Polly: I guess I was introduced to it while I was an undergraduate by Tim Rowe, who had just, not so many years before, finished his PhD at Berkeley. And he and Jacques Gauthier and Nancy Simmons, who was then Nancy Greenwald, had been among the first cohort of students really doing computerized phylogenetic analysis using cladistics. And so my first paleo course was taught with that and I was just absolutely fascinated by it and can still kind of remember being in an undergraduate class with Tim Rowe saying, "And so then you code all these characters and you put them in the computer and you get phylogeny." And I did not understand how that worked. And so my first interest was because of that.

And at that time the field was still in the heat of some of the debates about phylogenetic methodology and so on. So the places where I applied to go to graduate school, like Michigan, were the main areas in phylogenetic methodology and that's why I was interested in them. Berkeley didn't exactly have the same reputation, but this huge cohort of students who had been working on those problems had been here. So phylogeny was very much in the air, as was major relationships among mammal groups, which is still in the air. That hasn't gone away. So I was interested in those two questions, especially links between Cenozoic mammals and Cretaceous mammals and applying phylogenetic methodology to that. And I'd been interested in Carnivora, which were one of the early placental branches. But in order to do the phylogenetics on them you need to have outgroups to know what the ancestral states were. And there was this group called the Creodonts that no one had really done a whole lot of phylogenetic work with for quite a long time and certainly not with any of the new methods. And so I kind of got sidetracked into working on them, which is what ended up being my dissertation.

01-00:16:20
Burnett: And briefly, the new methods, how is this distinguished from the old way of classifying organisms by descent?

01-00:16:31
Polly: I can almost answer that in two ways, what I understood at the time I was doing the dissertation and what I understand now.

01-00:16:38
Burnett: We could be historical about it.

01-00:16:39
Polly: I think the way I would describe it now is a lot of it was the formalization of some of the methods so that they could be computerized and that introduced some changes that were often portrayed as being quite radical but in some ways, especially in retrospect, they don't seem as radical as they were said to be. But they were much more codified in order to literally code things for computational analysis. And then there were also arguments about how to classify those two things, how to study the phylogeny or relationships and how to classify. And I was, I guess, interested in both, but more in the phylogenetics, how to do that.

01-00:17:28
Burnett: And in your dissertation you had mentioned that it was a very specific type of research. You were studying something very, very specific. Can you talk about how you did the work and what the basic conclusion was or conclusions?

01-00:17:44
Polly: I guess I have to throw out something else in the development, which is when I entered, I entered into the Department of Paleontology, which was a standalone department up in McCone Hall. And while I was a student, in the

first two or three years, we merged into Integrative Biology. And that was quite transformative in some ways because, among other things, I started teaching as a teaching assistant in biology classes. And I'd always had a lot of biology and zoology but it really fused things. So Dave Wake and Marvalee Wake were major influences with their interest in how development channels variation and how that relates to evolution. Jim Patton, who was working on mammals. People working on some of the early molecular phylogenies. And Jim was doing what was being called phylogeography of speciation and its geographic components. And so having gotten interested in phylogeny, I was then also interested in things like how do evolutionary transformations happen and how might that be relevant to reconstructing phylogenies. So the components of my dissertation ended up being partly phylogenetic, partly functional morphology and partly trying to tie those together.

01-00:19:07
Burnett: It sounds like that's been a bit of a lifelong research project, too. There's phylogeny and there are adaptive functional changes, right, and how do you know which is which and what the relationships are?

01-00:19:23
Polly: Yeah. In fact, I think when I applied to come here I wrote something about that. I was never all that satisfied with my dissertation, how I put it together, and then really only in the last ten years have I felt that I was finally doing what I had put into my application to come to Berkeley. Therefore, yes, you're exactly right. It has been lifelong—

01-00:19:46
Burnett: And you can let me know at what point this happened or didn't happen—in your undergraduate period or in your graduate school, are there one or two works that really epistemologically united some of the questions for you?

01-00:20:06
Polly: Yes. Probably actually a whole series of them because I sort of have added new components as I've gone along. So the first would have been the phylogenetic things. Then Tim Rowe as an undergraduate just gave me lots of reading. He'd always said in class, as people do, "If you're interested in this, come by and I'll put you onto things," which, of course, most students don't ever do but I did. He put me on to Willi Hennig's book on phylogenetic systematics and other things. So that was sort of the first level of it.

But then I think the next level was really learning more about some of the development in evolution. And there was actually a very obscure set of volumes that were published in Czechoslovakia when it was still Czechoslovakia, behind the iron curtain that was about evolution and development that I randomly ran across in the library here which was quite influential on me. And some of the people who had previously worked with Dave Wake, like Pere Alberch who died quite young. He was from Spain and came to work here but he was very interested in development, how it

channeled variation, and I was very interested in development and how it controlled morphology of teeth and how the functional aspects were related to the morphology and how those transformed phylogenetically.

01-00:21:35
Burnett: And so you completed your dissertation. Well, it was no longer the paleo department, I suppose. It was Integrative Biology—

01-00:21:46
Polly: Yeah.

01-00:21:47
Burnett: —in 1993. And the next step was actually to Michigan, right, after that? Can you talk a little bit—

01-00:21:59
Polly: More or less. There was actually two small steps in between which may be of interest. In spring of ’94 I ended up teaching comparative and developmental anatomy here for Marvalee Wake. I can’t remember exactly what it was but she had some administrative duties which took her away from teaching, so she needed somebody to teach for her. So I taught that nominally for her, though Marvalee is a very energetic person, so she actually still taught half the class. So we shared it.

01-00:22:30
Burnett: But you learned in that process I imagine.

01-00:22:32
Polly: Yes, I learned a tremendous amount in that process. And I had TA’d it before that. And that was a very influential thing. And then in the summer I actually worked as a consultant for Genentech across the Bay doing web stuff because the Web had just erupted and built for them what is now known as an intranet but at the time had no name. And they wanted to use this for internal communications. So I did a lot of that. But yes. Then I went to Michigan, back as a post-doc in the Michigan Society of Fellows.

01-00:23:04
Burnett: Others have described their post-docs as really transformative. What impact did that have on you?

01-00:23:12
Polly: Having worked on early mammals, which are rare in the fossil record and doing the phylogenetic methodology that I had been doing, where you coded characters and said, well, this species has this character and this species has that character. What happened at Michigan is they had a major research program on a place called the Big Horn Basin, which is an extraordinarily rich sequence of geology that is full of fossils spanning the late Paleocene through early Eocene. And it’s so rich that there were suddenly hundreds of specimens for any particular species. And I was working on some of them and realized that that approach didn’t work because there was so much variation and they

graded into one another and I couldn't really tell which species was which and that's really when I started doing quantitative work. My work before then there had certainly been a computational part of it but none of it had actually been quantitative in a real sense. And that really did change everything I do because since then what I have done is barely recognized—it's hard to recognize what I do now as related to what I did in my dissertation, though I think of it as quite related.

01-00:24:26
Burnett: Right. Is there something about the early mammal work that kind of drives the quantification a little bit? Because it seems to cry out for subtle analysis. Because you're getting these disembodied teeth, and lots of them.

01-00:24:43
Polly: It didn't at the time. It should have and I think that's been one of my connections, because most quantitative analysis was done on things that are living today, where you actually have a big sample, because most quantitative analysis requires a pretty big sample to do statistics and have a significant effect. Early mammals tend not to have many specimens so it was very late in coming. And I think part of what has driven me since then is finding different ways of approaching earlier—essentially the fossil record quantitatively when you're dealing with things that are not amenable to ordinary statistical tests. And, indeed, the questions aren't even ordinary statistical ones. Quantifying those in a phylogenetic framework. And so still interested in phylogeny but in a quite different methodological way.

01-00:25:36
Burnett: Right, right. So you were a visiting research scientist at the Museum of Paleontology at the University of Michigan Ann Arbor from '94 until 1996 and then you got a job. Can you talk about that transition?

01-00:25:53
Polly: Yes. Actually what I did was I met somebody who was in the Michigan Society of Fellows post-doc program, who had been in it and was just finishing and had gotten a job in London and I had just moved to Michigan and she was moving to London and so I helped her pack and gave her my boxes and things like that and six months later we were engaged and so I followed her to London where I did not have a job.

And so I went there and first worked volunteer helping teach human anatomy at University College where several people, notably a paleontologist named Susan Evans and a paleoanthropologist named Paul O'Higgins sort of took me in and allowed me to do that and trained me in human anatomy for a bit. And I also worked for the natural history museum there as their web person and eventually their network manager for the first year and then got a job at Queen Mary, which is one of the University of London campuses, teaching human anatomy. So going back to Marvalee's course.

01-00:26:59
Burnett: Yeah, that helped.

01-00:27:00
Polly: That was the foundation that gave me a bit of qualification for doing that.

01-00:27:06
Burnett: Others have talked about, around this time, the folding, not just at Berkeley but at many institutions, the folding of paleo or paleo work into the life sciences. I guess it had happened a lot earlier in other universities. And human anatomy is something that paleontologists can teach and it provides them a bit of a supper to—

01-00:27:30
Polly: Historically vertebrate paleo grew out of human anatomy, people trained in that. And the thing that I did not really realize until I spent a while in a medical school teaching human anatomy is people who are trained in medicine don't want to teach. They're there to be doctors. And so most of the people who teach basic science in medical schools are not medically trained at all and vertebrate paleontologists and paleoanthropologists are two people who have to know anatomy pretty well and therefore are logical people to end up teaching it.

01-00:28:11
Burnett: Were there unanticipated benefits at an intellectual level that pushed you to ask different questions by having that kind of exposure?

01-00:28:26
Polly: Yes, no doubt. Teaching anatomy I learned anatomy much better than I ever had up to that point. But also being, first in the medical school, but then later I managed to get transferred to biology where nobody knows anything about paleo, just in order to keep your job and to get a job and to communicate with your colleagues you start reaching out. I got very interested in quantitative genetics while I was there and how that could apply to the analysis of morphology, especially with a quantitative framework. So the anatomy sort of continued to feed my interest in functional morphology and development in being in a department that had quantitative geneticists in it. Taught me to think about the evolution of morphology in a different way.

01-00:29:27
Burnett: Perhaps we could dive into a couple of your papers to get a sense of the evolution of your research. In 1996 you published, and I want to make sure I pronounce this right, on the phylogeny of the *Hyaenodontidae. This was a longstanding group. Over a century ago had been identified and classified. And you were reopening some of these questions, it seems, and looking at it cladistically. Can you talk about that research and what was significant about it for you and for the community?*

01-00:30:16
Polly: It probably wasn't as significant for the community as one ought to have done during a PhD, I'll have to say in retrospect, but it has been significant to at least some. And, in fact, I guess its significance has grown over the years. I think I said before that I was originally interested in them because in order to work on Carnivora, which has been my primary interest, they were the outgroup and it turned out that they were not very well understood phylogenetically and to some extent still aren't with how they're exactly related to Carnivora, which was my original question. But no, no one had worked on them very synthetically and so using collection from North America and Europe I made the first attempt at doing a phylogeny of them. And, as I guess usually is with research, when I was doing it I didn't necessarily feel all that confident about it. Look at the data and this is what the data said, that it was slightly to the extent that anything in Creodonts can be called radical, it was quite a different picture than had been there in the classification before. But I am pleased to say, in retrospect, now, for whatever reason, there are quite a lot of people working on Creodonts and phylogeny. But the basic framework has kind of held up, even with new discoveries. And so in the small world of Creodont phylogeny it is apparently a seminal paper.

01-00:31:53
Burnett: Right. Great.

01-00:31:53
Polly: But it is a very small world.

01-00:31:54
Burnett: [laughter] Right. Although the world of paleontology appears to—

01-00:31:58
Polly: And I don't think I realized that until I got to London, when no one really cared.

01-00:32:07
Burnett: Perhaps we can group that as a period, in talking about from 1997 until 2006 you were at the University of London in the department of anatomy and vertebrate biology. And you were doing research and you were continuing your interest in further developing quantitative analysis of these specimens. And there is a tremendous increase in computer processing power, which is a big theme throughout these periods.

01-00:32:39
Polly: Yes.

01-00:32:39
Burnett: Can you talk a little bit about how the technology shaped your research and how your research questions kind of drove you to seek out new methods and new techniques?

01-00:32:50
Polly: Yeah. I guess I should say about the ’96 paper. One of the themes in it is something about functional transitions and how those play into the phylogenetic understanding, all of which was done not quantitatively at all. While I was in London, partly delayed, that influence emerged— I’d been interested in morphometrics even before I started my PhD but I didn’t really end up doing much of it. And it really wasn’t until about that time that I did start doing it. And certainly one of the things that came along was digital cameras, because before digital cameras it was not all that easy to capture morphometric data. But they really changed that because you can take a digital camera to collections and just photograph hundreds of specimens.

Also having gotten interested in quantitative genetics, I was interested in variation and evolution of things like teeth and bones but using modern groups where you know something about the genetics. So I started working with geneticists and doing a lot of morphometrics on populations and genetic subdivisions and phylogenetic context.

And, yes, through those years it just became much more easy to capture data, to process data. New kinds of languages to program in, better ways of doing graphics, ways to capture 3D data instead of just two-dimensional data and so on.

01-00:34:36
Burnett: And a lot of complex mathematics and statistics, too, because you’re photographing them but you’re using those high-resolution photographs to kind of tag different data points and then those are rendered into numbers that can then be manipulated. Is that basically—

01-00:34:57
Polly: Yes. Yeah. So there’s a lot of geometry and algebra and matrix math and so on. And I guess one of the things that I have done—there’s a couple of ironies about my career, partly because of the undergraduate program I had, which didn’t have any prerequisites. I had been naturally pretty good at math, tested out of what was required, and then just simply skipped all the ordinary—

01-00:35:24
Burnett: All the higher math, right?

01-00:35:26
Polly: Well, all the lower math anyway, because you could skip prerequisites, so you didn’t have to do the things that people consider weed-out courses. And I also learned to program when I was in junior high or high school, as my father had been a computer programmer. He’d gotten us a computer at home, one of the very first IBM PCs that came out. And so having that through high school, I’d actually learned to do all of my math, like algebra and calculus and everything else, by programming it so that I could graph it out and see it. And a lot of what I do now has come out of that. And so my approaches have often been

quite different than standard ones, partly because I come from a standard training and so translating morphology that you can see into numbers and then analyses is kind of what I had done since a kid. So happily for me, a lot of that has come pretty naturally, learning it through implementing it that way.

01-00:36:40
Burnett: Can we open up a parenthesis on the nature of education? And some folks have talked about that period when people were learning to work with computers, were much closer to the machine, as they say. There was not a lot of prepackaged software that you would just plug into a problem. You had to really think as close as possible to the origins of the problem and its solution. Do you have any thoughts on that when you're thinking about the nature of how people are trained and how we train scientists?

01-00:37:21
Polly: I don't have any good recommendations for how to train them. But yes, I have had lots of thoughts because, yes, that's exactly true. So those first computers that I had, they had manuals but they were mostly about the technicalities of the processors and things that you never see in computer documentation anymore and you basically had a command line screen which you could program graphics, and you almost did it pixel by pixel and drawing lines with algebra and so on. If you wanted to do anything of interest at all you had to program it yourself and really kind of figure out what was going on. And it is quite clear to me that gave me a way of thinking about computers that people don't necessarily have.

Going to Britain also kind of reaffirmed that, because by the time I got to Britain, a lot of people who were university-age in the US had had computers and had them for a while. Whereas in the UK, people at university-age still hadn't. And so we had students in London coming in who'd never touched a computer before. But then through the years I was there, went through the phase where everyone had it and those kids were pretty computer-literate. And then you come to the present day, where the user is pretty far away from the programming. And so now it seems like university students are much worse at computers again. They know how to run the interface but not—

01-00:38:51
Burnett: Right, right. Exactly. So there's a shaping of the user to the point where it being user-friendly has become the enemy in some ways of inculcating an innovative problem-solving approach to using computers.

01-00:39:12
Polly: I do teach two graduate courses, regular graduate courses that involve quantitative analysis and programming and I never feel particularly satisfied with how I've taught it, though the students say it's good. And since I never formally learned it from anyone, it's not like I have a model of how to teach it. So I tend to teach it kind of like I learned it, which is I basically kind of very quickly and holding hands throw them off the deep end to get them through an

analysis as quick as possible so they can see the outcome and see what's interesting about it and then sort of back-up and go more slowly through everything again, both teaching the programming and teaching the quantitative analysis and how to interpret it. And, again, I don't have any breathtaking insights other than some people learn it quite well that way, some people don't.

01-00:40:09
Burnett: Right. But it's a somewhat Socratic approach—yeah.

01-00:40:12
Polly: Yeah.

01-00:40:13
Burnett: And so in 2005 you came out with a paper with the *Journal of Mammalian Evolution*, "On the Occlusal Fit of Tribosphenic Molars: Are We Underestimating Species Diversity in the Mesozoic?" And this paper describes an elaborate process that we won't have time to go into now, but it involves establishing quantitative relationships among these specimens of teeth and how the teeth fit together. Can you talk a little bit about the process of doing that research and, again, the techniques that you used?

01-00:41:06
Polly: Yes. First I'll note that that was in 2005 and I was a graduate student about twelve or thirteen or fourteen years before that. And that is essentially a quantitative version of problems that I was interested in when I was a student. And I mentioned Craig Wood before, who was visiting Bill. Craig really taught me a lot about thinking about teeth functionally and showed me that you could do cool things. Like if you had this very complicated tooth, teeth, they function, they come together. And there's actually only one orientation where they can come together and you can find that — sort of like sighting down a gun barrel or something like that — where all the fossa line up, and he had been working with Mesozoic mammals with Bill, which are rare, and you often find them as isolated teeth. And so he'd been working with putting those in that orientation so you could then see how well they fit together to see whether one upper tooth matched a lower tooth and therefore was the same species or whether they didn't match. And so he'd taught me that when I was a graduate student. And so that particular paper was for Bill's retirement celebration, a group of papers that we got people to put together, and this was my contribution to it. And so having been doing morphometrics and phylogenetics and having a lot of morphometric data sets at that point, had a set of data where we had pictures of uppers and lowers from the same population and the same species and closely related species and more distantly related species and, using morphometrics, you could then actually, instead of just looking to see how well they fit, you can measure how well they fit. So it was sort of an experiment where you put together teeth that literally belong to the same individual, same population but different individuals, same species but different populations, different species, and so on. And you can measure

how badly they fit as you went more distantly and so could then go back to look at some of those same questions and find out ones that looked like they fit reasonably well actually fit as poorly as some fairly distantly related species, which would suggest that, yes, there may be more species there than was thought.

01-00:43:37
Burnett: Right, right. It's just a really, really complex process. There's this, I love this phrase, this "Procrustes distance," right, so that you're reducing, essentially, all the dimensions of a tooth down to a single number. Is that right?

01-00:44:07
Polly: Not a single number but you are making them comparable. So if you sort of imagine it as you take photographs of things that are in slightly different orientations and, depending on whether you're close to it or far away, it's bigger or smaller on the photograph. And if you're really comparing them, if they're like objects, you need them to be in the same orientation, et cetera. And geometric morphometrics, which is what I work with most, its variables are the X and Y coordinates. So the X and Y coordinates of something are different if it's in this orientation versus this orientation. So Procrustes is a fitting technique that goes along with that. And being from the world when there were photocopiers, which I guess there still are—I guess now it's more computer screen—but you can enlarge or reduce something. And so what Procrustes does is it rotates them around and enlarges and reduces until they're the same size and it fits them together that way. So it essentially finds the best fit which makes them as alike as possible so then you can statistically measure the distances that you can't remove that way. And it is indeed named after Procrustes in Greek mythology, who either lopped off parts of people to make them fit into the bed or stretched them out to fit the bed for exactly that reason.

01-00:45:32
Burnett: Yeah. It's a great term. And just a few years later there's a paper on adaptive zones in the pinniped ankle. And this is using a new technique, right?

01-00:45:56
Polly: Yes.

01-00:45:56
Burnett: With a new morphometric technique. Can you talk about that and why that's important?

01-00:46:01
Polly: Yeah. Two aspects of that. Let's start with the sort of technical one. Going back to technology. By that point it had become possible to get three-dimensional representations, scans of the entire three-dimensional surface of something. And before that you could, in laborious ways, get 3D objects, but usually if you were doing morphometrics you were either taking caliper measurements or taking digital photographs and doing some of the analyses.

And people had done points on objects and people had done outlines around curves. But with this new three-dimensional digital object, it was really seductive to want to be able to use the whole three-dimensional surface. And I'd been doing a lot of morphometrics at that point, including programming the methods and was working pretty closely with Norman MacLeod, who was in London, also a morphometrician who'd done a lot of the outline kind of analyses.

There's also not only a scientific side but also society of science, so to speak. Among the people that I was introduced to via Bill were people of his cohort, one whom was Malcolm McKenna, who was a student here and who was at the American Museum and big into phylogeny. And there was another person who wasn't a Berkeley student but had been a McKenna student named Frederick Szalay who was very interested in functional transitions in phylogeny. And, in fact, Szalay and McKenna, probably because they had close roots, argued a lot. And Szalay was very much against computerized because it didn't take into account any of the functional transitions. He argued they would put things as though they were closely related where you couldn't possibly get a functional transition between them. And so he was very much against basically any quantitative analysis.

And I really got along with him and he was very good-natured about arguing and we had lots of scientific arguments. I learned an awful lot from him either directly or indirectly through his papers. And I, of course, had been interested in the functional transition question because it seemed like a very important one. And, plus, he always worked with ankle bones which have a very tight interlocking functional relationship but one that's impossible to measure. And so that paper was for his retirement symposium. And having done all of this morphometrics I kept thinking, "You could take this three-dimensional stuff and you could actually capture all the things in that ankle that are important about how it fits together and you could then quantitatively map that onto a phylogeny and measure all of the things that he has"—the contributions he made I really admire. But because we liked to argue all the time, it became sort of a challenge in his honor to be able to quantify that and do quantitatively what he was doing in a way that he would appreciate but which was completely quantitative. So yes. Norm and I kind of hashed out that algorithm, mostly in the pub on Friday nights.

01-00:49:40
Burnett: This is Norm—

01-00:49:42
Polly: Norman MacLeod.

01-00:49:42
Burnett: Norman MacLeod.

01-00:49:44
Polly: There's now two or three different methods for really analyzing that three-dimensional surface kind of data and that's one of them.

01-00:49:56
Burnett: What was the reception from Szalay?

01-00:50:05
Polly: It was quieter than I had hoped for. He basically said he liked it but he is somebody who has retired and he's really retired. He was kind of leaving the scene by that point.

01-00:50:21
Burnett: Yeah. But it's an effort to say, "Hey, you can quantify what you're describing in a really fascinating way." And some of what I was reading in these papers, a big theme, and we've talked about this a little bit already, is that there's a phrase or a passage that you write in the adaptive zones paper. You're exploring to what extent adaptation masks phylogenetic history, how the convergences can be recognized, whether adaptation impedes phylogeny reconstruction and how the interplay between form, function, and phylogeny can be better understood. And so for the layperson one of the basic questions and problems is if there are two specimens that are alike, are they alike because they're descended from a common ancestor or are they alike because they're not descended from the common ancestor but they adapted to the same kind of ecological niche. Is that one of the problems?

01-00:51:24
Polly: Yes. That is very closely related to what I said there. The other aspects of that, another big theme through the time I was a graduate student, was the idea of constraints on evolution and that development might constrain evolution because it doesn't necessarily allow the variants that would be fit to be produced and therefore evolution can't go in certain directions. And also in quantitative phylogenetics, as it has emerged, there's different processes of evolution. We talk about Brownian motion, which is sort of random evolution or directional selection which is carrying you off in a certain way or what is frequently called the Orenstein-Uhlenbeck process but it's where everything is evolving around kind of an optimum.

So if it goes too far away ,it gets brought back. And so if a structure like an ankle is adaptive in the sense that it supports the weight of an animal, allows it to locomote, it can certainly evolve but it can't evolve in a way that the animal can't support its weight or can't get around. So there's a certain constraint on it. And so that particular paper kind of put all three of those concepts together in a phylogenetic context because terrestrial mammals, they evolve. You being arboreal or terrestrial and sometimes they go back and forth and so they get convergent evolution. But then in Carnivora you've also got seals and sea lions which are not supporting their weight on their ankles anymore and their ankles are actually quite different. And so quantitatively showed in that paper

phylogenetically the terrestrial clades are all sort of weaving in and out in a fairly constrained space but then when the aquatic, the clade that goes into the water, they change quite radically and they do so by just going in a completely different direction. They don't go faster than the others but they're no longer in that adaptive zone. They've gone into a new one and it takes them in a new direction.

01-00:53:39
Burnett: Oh, that's fascinating. I want to thank you very much for your time. But before we end off, is there anything else you'd like to add about your time at Berkeley, at UCMP, or working with Bill?

01-00:53:56
Polly: I can tell you a funny story.

01-00:53:58
Burnett: Sure. We have time for a funny story.

01-00:54:00
Polly: Which is mostly just simply that. When I started, Bill, one he was busy and as you can tell from the interviews you've been doing, he had a lot of students. He probably had ten graduate students while I was—

01-00:54:20
Burnett: At the time. Concurrently.

01-00:54:20
Polly: —at the time. And he was director of the UCMP and somewhere in there he was president of the Society of Vertebrate Paleontology. And I've gotten to know him actually a lot better when I was in London because he and Dot would visit us in London. But certainly when I started as a graduate student I only barely knew him and, because he was so busy, had really only met with him a few times in that first year or been in a class with him where he was teaching but not really interacting. And he took me out into Montana. And not only did he take me out to Montana but the whole UCMP group was going. But most of them were going to one area and Bill was wanting to do some mapping and he took me with him to do mapping. So it was just me, the new graduate student, and Bill in a cabin, both of us with a bunch of sheep. And the sheep would come in and stay around the cabin in the evening and then they would go out in the morning. Bill is not exactly the most talkative person and I'm not normally the most talkative person either, so it was really a rather silent time. And the sheep would come in and there were hundreds of them. Baa, baa, baa, baa, baa, baa, baa, baa, baa for hours and hours. And Bill and I would talk a little bit. I sort of remember the first day we were there, he said something like, "All right, Polly, you make the breakfast, I'll make the dinner." [laughter] I said, "Okay." It was a really great experience but it was a rather solitary and quiet one. And when we got off to the rest of the group they had been imagining this whole scenario and Don Lofgren, who I think you've also interviewed, had already named me Lonesome Dave Polly in anticipation of

the story of what that had been like. But that was a really great time because there was a huge amount that I learned from Bill just in that week or so that we were out there by ourselves about the paleontology of the Hell Creek and just field paleontology in general.

01-00:56:39
Burnett: With a running commentary from the sheep, as well.

01-00:56:41
Polly: Yes.

01-00:56:42
Burnett: Yeah, absolutely. Well, thanks very much for taking the time to sit with us.

01-00:56:46
Polly: Thank you.

[End of Interview]

Oral History Center
The Bancroft Library

University of California
Berkeley, California

The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology:

Jessica Theodor

Interviews conducted by
Paul Burnett
in 2015

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It is recommended that this oral history be cited as follows:

Jessica Theodor. “Jessica Theodor: The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology” conducted by Paul Burnett interviewer in 2015, Oral History Center of the Bancroft Library, The Bancroft Library, University of California, Berkeley, 2017.

Jessica Theodor, 2015

Jessica Theodor is Associate Professor, Ecology and Evolutionary Biology at the University of Calgary. She received her PhD from UC Berkeley in 1996.

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Audio File 1

Interview #1 March 9, 2015
Begin Audio File 1 theodor_jessica_01_04012015_stereo.mp3

01-00:00:00
Burnett: This is Paul Burnett interviewing Dr. Jessica Theodor for the Bill Clemens UCMP [University of California Museum of Paleontology] Oral History Project of the University History Series. It is April 1, 2015. We're here at the Bancroft Library, and this is audio file one. So Dr. Theodor, welcome. Customarily we'd begin at the beginning in a short oral history, so can you tell us a little bit about where you're from and where you grew up?

01-00:00:31
Theodor: Okay. I am originally from Toronto, Ontario, in Canada, and I came out to the University of California for my graduate work in '89. Before that I was an undergraduate at the University of Toronto. In 1988 I went and attended the Society of Vertebrate Paleontology annual meeting in Drumheller, Alberta, which is where I first met Bill [Clemens], when I was looking for graduate advisers.

01-00:01:00
Burnett: To back up a little bit to your undergraduate days, how did you become interested in paleontology?

01-00:01:08
Theodor: I found a fossil in my backyard when I was six.

01-00:01:10
Burnett: Really?

01-00:01:12
Theodor: Yeah, I did. I found a Paleozoic invertebrate. It's called a Crinoid, so it's an echinoderm. What it looked like is like a little series of carbonate disks. Toronto is on Ordovician-age limestone, and so those are not uncommon fossils around there. I was very excited and wanted to know what it was, and my godfather was a geologist, and he suggested taking it to the museum on one of their own identification day, and I did. They told me what it was, and my eyes were the size of dinner places. [laughter] Then subsequently they had some classes for kids at the museum that I took. My parents were very, very good about encouraging my interests in that way, and after the classes were over you could wander wherever you wanted in the museum. And I wandered into the fossil galleries, and I was transfixed. So it's a very old interest of mine.

01-00:02:11
Burnett: I'd say that story probably happens for most kids. I think they go to the museum and they see the fossils. I think probably 90 percent of kids in developed countries go somewhere to a museum and see dinosaurs and are transfixed. But unlike most kids who grew up, you continued. What drove you to really pursue it as a profession and as a calling?

01-00:02:39
Theodor: Well, for one, I never lost interest in it. It was always—I mean, I came home after the museum and said to my parents, "I want to be a paleontologist when I grow up." Unlike most parents, they didn't say, "Don't be silly. You can't do that." It helps that my dad was a professor in psychology, and my mom was in graduate school at the time, or very close to starting graduate school. I'm forgetting at this point. But both of them academically oriented. My godfather was a geologist, so they knew it was something you could do, and so their attitude towards it was, well, if you're interested, pursue it. As long as you can support yourself, that's fine. So I never lost interest in it. I think I kind of lucked into something in terms of interest that supported things I'm good at. I'm a very visually oriented person. I pay a lot of visual attention to things, and I have always been interested in history, so my best subject in high school wasn't science, it was history, by far. In fact, I learned more from my history teachers than I learned from my science teachers in high school.

01-00:03:49
Burnett: So it was kind of the natural history side of paleontology that you were drawn to?

01-00:03:54
Theodor: In large part, yeah. It's understanding the animals as animals in the past. It's understanding the past in a real context that I can relate to what we know from living animals. That really grabs my attention.

01-00:04:09
Burnett: And so you pursued a degree in paleontology. The BSc is in paleontology—

01-00:04:16
Theodor: Yeah.

01-00:04:16
Burnett: —from the University of Toronto. So is there a Paleontology Department or is that subsumed under a larger—

01-00:04:22
Theodor: No. At the University of Toronto when that program existed, and I don't know if it still does, it was essentially a double major in zoology and geology or botany and geology, any of the biological disciplines plus geology. So mine was very slanted to zoology. There was a list of courses you had to choose from, and out of twenty course units you had to take sixteen in the concentration, so it was pretty intensive.

01-00:04:52
Burnett: Then you applied for graduate school?

01-00:04:59
Theodor: Yeah.

01-00:05:00
Burnett: How did the UCMP connection happen for you?

01-00:05:06
Theodor: When I was applying there was still a Department of Paleontology on campus, and it was heavily affiliated with UCMP. It was really essentially almost one and the same. There were some administrative differences, but for the most part the museum and the department were one and the same. Then after I applied the decision was made to move paleo into integrative biology. So when I arrived there was a lot of pessimism actually. When I remember talking to other grad students at conferences and professionals who are like, "Why are you going there? That program is dying and disappearing, and it's not going to go anywhere." I was like, I don't think so. That just doesn't make sense to me, and Bill's a known entity, and so I stuck with it. I'm actually the last graduate of the Department of Paleontology. I don't have a degree in integrative biology.

01-00:0:00
Burnett: Did you go there to work with Bill?

01-00:06:05
Theodor: Yes, explicitly. Yeah. Unlike many people who do fall for paleontology, I wasn't actually that interested in dinosaurs per se, by that point. While I was in high school Lucy was found, well, before I started high school anyway. I was in elementary school. That really captured my imagination. For a long time I thought I wanted to study hominids, and then when I hit undergrad and took some anthropology classes I started to realize that there were very few fossils relative to the number of people who cared about them and thought about them, and I really didn't like that aspect of sort of backbiting, and I stared to get interested in other mammals.

So when I was looking for graduate programs, I did undergraduate research on some Cretaceous reptiles with Rufus Churcher, and some work on Miocene armadillos at the ROM, at the Royal Ontario Museum, and decided I wanted to work on mammals. So when I was looking at graduate programs I applied to work with Bill, I applied to work with Tim Rowe at Austin, and I applied to work with Phil Gingerich in Michigan. It was very much oriented toward mammals right from the start. Having been a faculty brat, I knew I wasn't just applying to a program, I was applying to work with specific people that were very carefully picked.

01-00:07:29
Burnett: Right. So you made some very careful choices.

01-00:07:33
Theodor: Yeah. Being a faculty brat kind of changes how you look at graduate school. [laughs]

01-00:07:36
Burnett: For the better I imagine. Your eyes are wide open in terms of—

01-00:07:40
Theodor: Yeah. You're not going into it naïve, right? You don't go into it and think, oh, it's all going to be perfect, not when you've grown up with it.

01-00:07:51
Burnett: Right. So you arrived. I imagine you just got involved in a research project fairly early on because you were certain of what you wanted to do?

01-00:08:06
Theodor: It actually took me a while to settle on a dissertation project. One of the things that when I applied to work with Bill, Bill was like, "I'm between grants, I don't have a specific project to put you on. So if you can find funding, then you can pick your project." I got myself an NSF [National Science Foundation] fellowship, because I was in the unusual position of being a dual citizen. And Bill's like, "Okay, what do you want to do?" I was like deer in the headlights for a little while. The first thing I wanted to do, I had a long talk with Howard Hutchison, who used to work Mark [Goodwin]'s job essentially, well, Pat [Holroyd]'s job, collection management, right, and originally was hoping to work on some material that Pat subsequently worked up on Bitter Creek, which at the time Don Savage was still interested in and wasn't ready to let anybody else work on.

So that kind of went by the wayside, and I was looking for other things to do, and I got focused on Artiodactyls. They're a really understudied group, and Bill agreed to let me work on them. What I was interested in doing with them was everybody was trying to figure out how they're all interrelated. It's a big group of mammals, and everybody had been using teeth, which is the classic mammalian paleontology thing to do. The problem is that Artiodactyls make the same patterns in their teeth over and over and over again. There's a huge amount of convergent evolution. So my thought was, well, nobody's really taking into account the rest of the skeleton and what's going on outside the head. Maybe that will help. As it turned out, it didn't help as much as I was hoping it would, but it's a complicated group. But that was what my thesis really focused on, was looking at, well, what's the variation in the rest of the skeleton?

01-00:09:57
Burnett: So this is kind of morphological research? You're looking at the structure of the fossils and trying to understand the relationships among the groups. As I understand it, the artiodactyls, their origins are ambiguous even to this day. It's quite a bit of a mystery, and, there's a small number of them. They range so widely.

01-00:10:27
Theodor: Yeah, there's actually a tremendous diversity even today. I mean, if you count up among the groups of mammals, the orders, it's the fifth most diverse, and

it's the only—when you look at mammals, usually things that are small are diverse. Artiodactyls are big, I mean, you're talking about cattle and moose and giraffe, they're big, they're hard to miss, and they're incredibly diverse. That, to me, was interesting. So we don't know that much about who they're descended from. There's a huge amount of diversity today. There's a huge amount of diversity in the fossil record. If you start to count up at the genus level, so like instead of species this would be the slightly bigger group, 89 percent of the diversity is extinct, of the whole group. That's a tremendous amount of paleo-biodiversity to account for. And there's still some really big questions, and one of the big questions that came up as I was working on my dissertation is where do whales fit in, which we hadn't previously thought was an issue. But it turns out that whales are very closely related to hippos, and so where they fit in turns out to be a really interesting question.

01-00:11:40
Burnett: So you finished your dissertation in 1996, is that right?

01-00:11:47
Theodor: December, 1996. I squeaked into the deadline. It was pretty tight. [laughs]

01-00:11:56
Burnett: What happened next for you? Did you decide on post docs? What was the next step for you?

01-00:12:02
Theodor: I had run up against the end of my time and kind of had to finish in a hurry.

01-00:12:09
Burnett: You had a F1 visa, is that right?

01-00:12:12
Theodor: No, I'm a citizen. It was normative time. I ran up against the limits of if you don't finish now you're going to have to retake your candidacy exam. Right? Because, you know, like many graduate students, I just kind of was noodling along and then went, oh, crap, there's a deadline. Okay, put the headphones on, write, write, write, write, write. That's basically what happened.

01-00:12:32
Burnett: That is your preferred way, with headphones on and music?

01-00:12:36
Theodor: By that point we were all over in VLSB [Valley Life Sciences Building], over in a big shared lab, and the only way to really concentrate was to have the headphones on, because otherwise people were talking to you, right?

01-00:12:46
Burnett: Right. Nice strategy.

01-00:12:50
Theodor: So I finished, and I didn't have anything lined up immediately. I finished in '96, but in order to get everything written on time I had been working for the museum as a curatorial assistance at that point, and Pat was already there at that point. So I had made arrangements with Pat to sort of get paid for work that I was going to do as soon as I filed.

01-00:13:13
Burnett: And this is Pat, for the record?

01-00:13:15
Theodor: Holroyd, Patricia Holroyd, yeah, the collections manager. So essentially I ended up spending the next term sort of working off my debt, and I was doing other work as well and applying for post docs. I kept applying. The one I got was the one at Brown [University], which was a teaching post doc, so teaching human gross anatomy at Brown in the medical school, which is cadaver-based dissection.

01-00:13:43
Burnett: That's a very different kind of thing.

01-00:13:46
Theodor: It sounds like it, but if you're a morphologist, dissection is really pretty much part of your training. When I was an undergraduate at Toronto I had two semesters of comparative anatomy, dissection based, and when I was at Berkeley one of my TAships was in Marian Diamond's human anatomy class, which was not dissection based, it was pro-section based, so we had stuff that had already been dissection that we were demonstrating to the students. But if you've already dissected a lot in comparative anatomy, and you've TA'd that kind of lab anatomy of any kind, teaching dissection anatomy of humans isn't hard, right. It's not like you're dealing with twelve different species, they're all the same species. You're just doing it in more detail, and human anatomy is pretty well understood, so it's a pretty straightforward thing to teach, except when you start dealing with the occasional groups of students who've had no sex ed and need remedial sex ed while you're doing dissection. [laughs]

01-00:14:43
Burnett: Interesting. Teaching presents its challenges.

01-00:14:48
Theodor: It does. And anatomy lab particularly, there's an awful lot of strange sense of humor that goes on in the anatomy lab, just because it's kind of one of those situations. And I don't mean that to mean that there's any disrespect to the cadavers, because there's not. But you just find yourself in really awkward situations.

01-00:15:05
Burnett: There must be something surreal about it.

01-00:15:08
Theodor: Oh, very. I have been in a room where there are seventeen cadavers in various stages of dissection with arms tied to IV poles and things, just to keep them out of the way so they can do the dissection they need to do, and looked around and thought, "if I didn't have a PhD, I'd be arrested for this."

01-00:15:24
Burnett: That must have been an experience. But you did that, and it was a teaching post doc, which can be intensive and can block you from doing research.

01-00:15:35
Theodor: Yeah, I mean, luckily the way it worked was we had one term of teaching human gross anatomy, and the other term was free for research. So during anatomy term, things were a little intense, but the rest of the time it wasn't too bad.

01-00:15:50
Burnett: So were you able then to focus on publishing or working with the dissertation project and turning it into—

01-00:15:57
Theodor: I did work on publishing parts of the dissertation. I did not publish all of it or even close. But I immediately started a new project with Christine Janis, which led to the browser grazer paper, the 2000 paper with Janis and [J.] Damuth. That one plus another paper on camels were the big products of my post doc, where we were working with those data. So Christine Janis had compiled a big database, as part of a book project, of North American mammal data. What we did was go in and really look at—there was this idea at the time that what was going on with the evolution of grazing mammals was that grazing spread at the same time as the spread of the C4 grasses, that the evolution of C4 grasses promoted this, like grazing took over.

What we showed was that's not actually true. Grazing mammals actually come in earlier, and browsing mammals actually underwent a big decline. It wasn't like there was this huge explosive radiation of grazing mammals, it was there was this tremendous decline of browsing mammals in the Miocene, and we got that out of the data. So that was a really different project for me. I mean, it was still morphology based, but it was really more of a statistical project than anything else.

01-00:17:21
Burnett: I wanted to ask about that as a historical question about computing power and the changes in what was possible in terms of paleontological practice from say when you started, in the mid '80s until let's take it up to 2000. How did computing power enable different kinds of questions to be asked?

01-00:17:53
Theodor: It certainly enabled a lot more big picture statistical kinds of questions to be asked. I would say from my perspective that's actually how my career got

established, because I got a reputation for being good with computers and databases and numbers. Probably undeserved, but I could use them. At the same time, there was the beginnings of, and I think there still is, a certain level of suspicion among older-guard workers that there's an awful lot of bad work done that way that ignores problems with the datasets. That if you pull information out of the literature without having seen the fossils or knowing the field notes, that you may miss things that were hidden sources of bias in your data. That's identified as this, but everybody knows that identification is wrong, and if you don't go back to the drawer you don't see the list of notes from a hundred years of workers who've made comments about it, right. That's, I'd say, a big criticism that I hear a lot about a lot of the younger workers today is that they're not spending enough time in the drawers.

Now there's a certain amount of get-off-my-lawn quality to those complaints, right? I mean, there's always the past complaining about those young whippersnappers who don't know anything. But there is a grain of truth to it as well. There are some really egregious things that get out there, and so it's a bit of a balance. I think one of the big, from the perspective of my post doc adviser, Christine Janis, one of the big reasons she was interested in me was she knew I came out of the museum, and I knew those things and that I was a competent morphologist. She trusted my judgment. If she asks me, you know, "What do you think this animal was doing?" and I say, "I think it was eating this," or "I think it moved like that," that there's a reason I think that, and that I can elucidate what it is based on the morphology I'm seeing, that I'm not just pulling it out of my butt.

01-00:19:57
Burnett: That has to do, I guess, with the reputation of UCMP a little bit.

01-00:20:02
Theodor: In part, yeah.

01-00:20:03
Burnett: It has a reputation for meticulous, careful work.

01-00:20:06
Theodor: Well, it's in part UCMP's reputation, it's in part the reputation of just museum-based programs. But I think UCMP particularly, there is definitely a reputation associated with having been one of Bill's students. You know. There's no denying that. I mean, you look at where we're all employed, and the fact that we all are employed I think makes a point right there.

01-00:20:33
Burnett: Yeah. That's right. So it's not even a kind of divide between, or people thinking that there could be a certain naiveté of I guess let's call it in silico modeling, for example, and field work. It's between in silico modeling and collections-based research, so it's not even—

01-00:20:57
Theodor: Yeah, I mean, there are certainly naive users of collections who also make mistakes, and the big difference in our training was that we got the whole package. We got the field training, we got the curation training, we spent time in the collections just noticing things. When Bill taught his graduate mammals class, that was an entire semester of Bill pulling out specimens and putting them in front of us and asking us to talk to him about them and reading papers about them and really think, be able to identify them. When I took Jim Patton's mammalogy class, as a grad student because I didn't get mammalogy as an undergrad, because of scheduling issues, I can remember Jim putting out a few fragmentary things. Normally when Jim does a mammalogy exam, he puts out a skin and a skull, and you have to be able to identify it. He put out, in the year I took it, a beach-rolled maxilla, so this part of the face, of a walrus with no teeth in it. So it wasn't a full skull, there was no skin, it was just this maxilla, and there were no teeth, there were just some pebbles jammed in the tooth sockets. I was the only one in the class who could identify it, and he put it on there partly to see who else could, because the paleontologists can do that, and the mammalogists have no idea. The undergrads were like, what the hell was that thing? I'm like looking at it and going, well, it has a bunch of little tooth socket holes here and one big one. That's a walrus.

01-00:22:28
Burnett: So you're being trained how to see fragmentary evidence. You have to be able to infer from a small—wasn't that [Georges] Cuvier's claim to fame, is he could take a single bone—I don't know if this is apocryphal or not.

01-00:22:43
Theodor: Exactly. That's the story. Yeah. It's visual pattern recognition and also prediction and sort of being able to visualize what's missing. It's a very particular skill, and the strength I think for UCMP students was that we didn't just learn how to do the statistics. In fact, I mean, Bill certainly didn't teach us how to do the statistics. He taught us how to think about the problems, but he didn't do that kind of analysis. But we all ended up going in the field, where it was with our advisers or not. We went on field trips. The grad students used to organize the spring field trip. We, all of us, not only TA'd, but I can't think of anybody who TA'd and didn't end up working in the museum doing some collections management at some point. Some of us did a lot more than others. Certainly Anne Weil and I did a lot in the collections, because we were heavily involved in the collections move, and I did a lot with collections databases because I got into database programming at one point.

01-00:23:46
Burnett: It sounds to me that what's important about UCMP and the people who've come out of there and the people who work there was you have to understand the full set of relationships between the different kinds of practice, scientific practice. There's field research, then it's brought into the collections and organized and relationships are established and reestablished and broken up again. Then there's this third order of analysis that is taking large numbers of

data points and doing statistical analyses and modeling. Paleontology has become so big now that it has diversified itself, differentiated rather into these potentially narrow domains. Do you see that as a danger?

01-00:24:39
Theodor: I think that is one of the dangers. The other is, I mean, the problem in the past was that when paleontology was seen as its own discipline and sort of isolated, it was regarded as irrelevant by a lot of people. So geologists and biologists would say, "Oh, yeah, whatever. That's for kids." Except it's important. What really happened, and UCMP was in some ways a big part of it and not the only one, was bringing it back out to the disciplines and saying, "No, you have to pay attention to the fossils if you're going to be a biologist." Actually coming into [the Department of] Integrative Biology turned out to be really good for that. It's not like Bill and the other paleontologists associated with UCMP didn't interact with the folks from MVZ [Museum of Vertebrate Zoology], but being in the same department meant they really had to get to know one another a lot better. So that was actually really good, because it forced the biologists to pay more attention to the fossil record and to cross train their students a little bit.

So Jim Patton's students would take Bill's graduate mammals class, and that's really good training and not something a lot of places get, because a lot of places don't have paleontologists at all. That's been my role at [University of] Calgary since I've been there, is to really bring this time depth to biology, to a department that really had never had it—well, they'd had an anatomist who had some students doing paleo but the integration wasn't great. Because in his case it was a bit more of a sideline, and in my case it's my primary research, there's been this sort of forcing—I hope anyway— them to come to grips with more of it.

01-00:26:16
Burnett: And ecology, right? I think your position is ecology and evolutionary biology, so it's bringing in—

01-00:26:23
Theodor: In my work, I do a lot of paleoecology these days, so I do a lot of trying to reconstruct diet and understand changes and responses of the animals to changes in the environment and thus to their diet, so like an adaptation to grasslands and things like that.

01-00:26:40
Burnett: Yeah. Bringing ecology into paleontology seems to be beginning in the late fifties or early sixties, but it grows and grows, of course. So returning to the question of morphology and what you can see, as a paleontologist, can you talk about the development over time of what paleontologists and other researchers can see or learn about the fossils, in terms of not just—because initially I suppose its biomechanics, you know, they stood on their hind legs,

but on top of that new things can be inferred from them? Can you talk a little bit about that?

01-00:27:33
Theodor: Yeah. Yeah. When you look at what Cuvier described or original stuff, they were trying to figure out things like how did these animals hold their bodies? What's their posture? How did they move? Our ideas on that have certainly changed a lot over time. We can do things like look at evidence of scars from muscles, so where muscles attach to bone they usually have little fibers that go into the muscle to anchor the muscles, and so there's a rough spot on the bone. So we can reconstruct some of the muscle structure from the scars left on the bone. The problem is that not all muscles leave scars on bones, and interpretation can be a little squidgy. But that's the starting point for an awful lot.

Then people started wondering about things. So there's always some groups that have always been controversial about how you reconstruct them, so dinosaurs certainly were one. In mammals there's a few where we're going, "I don't know." There's a group of animals that I have done some work on called oreodonts, and there's one member of that group that doesn't have hooves, it has claws on the hands. So there's all these arguments about whether it climbed trees and what it was doing and things like that. But there's the additional layer though, and people started really trying to reconstruct ecosystems, and "what's this locality like?" They started looking at things like, well, how many species of different body sizes are there in an ecosystem and what does that tell us about was there a lot of woods or not? It turns out that it does, lots of small mammals tells you something about how wet an environment is and how much tree cover there is, as opposed to how many big mammals, things like that.

People started looking at the teeth specifically. I mean, we've always been obsessed with teeth because they have a really good record, right? The bulk of the fossil mammal record is teeth. We can identify teeth to species in a lot of cases so it's really useful, and that's where we have a big advantage over what you can do with dinosaurs, because we don't need a whole skeleton to tell a lot. So people started looking at what the teeth could actually tell you. So starting in the eighties, I would say, people started looking at wear on teeth, how are the teeth worn at the microscopic level, and what does that tell you about what those animals were eating? At first it was really expensive and difficult to do. You needed a scanning electron microscope. Gradually people have evolved different techniques that involved light microscopes and other ways of doing it that are much cheaper, and we're doing it at a much broader scale on a lot more species.

01-00:30:09
Burnett: And that seems to be why the interdisciplinary stuff becomes so crucial, because now that you're doing this kind of research you bring in paleobotany

as well, so if someone working on mammals is also going to have to know something about what they're eating.

01-00:30:22
Theodor: Yeah, so you need to learn a little bit about plants and the microstructures of plants that affect tooth wear. In some cases, we're talking right now about potentially setting up some experimental work on teeth wear, and to do that we're going to have to coordinate with our vet school, right? So these networks get built, and one of the things that's changed a lot is that a lot of paleontologists used to be employed in geology, and now, certainly in vertebrate paleontology it's almost all biology and an awful lot, because we're qualified to teach anatomy and many of us do teach anatomy now, because most MDs would rather teach clinical practice than anatomy.

So a lot of the anatomy taught in North America in vet schools and med schools now is being taught more and more by paleontologists, and that gives us new toys to play with and new people to interact with and new questions to answer. So it's really interesting. Because at my school we actually have, what, six paleontologists actually on the faculty, but you wouldn't know it, because Jason Anderson teachers in the veterinary medicine faculty, he teaches anatomy. One of them teaches anthropology; one of them teaches sedimentology and stratigraphy in the geosciences department. They're just all over the place, and we're not all in one department. So we're interacting with a whole bunch of different people. I interact regularly with archaeologists, anthropologists, biologists, geologists, vets, doctors, and it changes what research questions you ask and what toys you have to play with to do it.

01-00:32:02
Burnett: Yeah. Well, that sounds really exciting, and I imagine also that paleontology has always had to sing for its supper I suppose. It's had to appeal to the romantic side of learning about the origins of all life. Recently it's you have quantitative skills more and more. More and more the paleontologists have skills with computers, as you said. Is that also a factor in that you can teach different kinds of things, or was it that paleontologists had always had that skill with anatomy, and they could, when they got into those niches, they flourished?

01-00:32:42
Theodor: I don't know. I think the quantitative stuff is all of science really, and that we stopped being able to publish stuff without the numbers attached. So if you talk to some of the older paleontologists, some of them are pretty phobic about statistics, and very few of the young people are. What the older people will tell you is that they are worried that it's garbage in, garbage out, which is where somebody like me, who has museum experience, gets more credibility than somebody who hasn't done all this work in a collection. So I've seen graduate students publishing who have done these great analyses on a database, and they've never seen the fossils, and they don't know all the problems in it. I can understand where the older guard are coming from.

Given how hard it is to get a job and how the field is, it's hard for these students to do otherwise, some of them, especially if their advisers are not pushing them into collections. But it's challenging.

01-00:33:42
Burnett: Right. No, absolutely. Well, perhaps we could dive a little deeper into some of the modeling and hypotheses that you tested in some of the papers that you've done subsequently. I don't know too, too much about this, but there was this edited volume that David Archibald and Ken Rose put together, and you wrote a paper on that. It was about the Artiodactyl.

01-00:34:15
Theodor: Yeah. That paper basically was really a review paper of all the work that had been done on the systematics of Artiodactyls. So I was going through and writing about the whole group, where it comes from, what we know about its origin, what we know about its radiation, what do we know about how the groups are related to each other. That was more of a review paper than a data paper. But it was a pretty good summary of the state of the state in 2005.

01-00:34:49
Burnett: Yeah. The whole volume seemed to be about trying to put together what the new molecular evidence was saying.

01-00:34:59
Theodor: And how we integrate it with the fossil record, which is still a challenging area. One of the strengths of the molecular data is that you can get all of the data for every living species. The problem is that you can get none for the extinct species, and that's a problem, because Artiodactyls are not unusual in being 89 percent extinct. There's a lot of lineages where we know very little. Giraffes today. There is the giraffe, which is a couple of species, and the okapi. You look back in the fossil record, there's sometimes over ten different forms, most of them short neck, some of them with incredible headgear that looks like moose antlers out of their ossicones, and sivatheres, which are these ones from Pakistan, which look like triceratops. I mean, if you see the skull, you would think triceratops until you turned and realized it had giraffe teeth. It's very odd. Well, maybe not triceratops, but it looks like a horned dinosaur. I mean, seriously, I have a picture of the skull. It's really funny. It looks like a horned dinosaur, and you realize, no, that's a giraffe.

People don't think about that, and the fact is that it's a problem for phylogenetic analysis, because if you only have a few species and they're related to a group that has a lot of species, that causes problems for the analysis actually. It's called long-branch attraction. There's some questions that we have about the origin of giraffes that we can't really answer because we don't have any molecular data for any of the extinct giraffes. We have the morphology, and the problem is we've got enough holes in the morphology that it's really a problem. This happens over and over in Artiodactyls, so we have camels, they're a similar problem. So today everybody thinks camels,

they're African and Asian. Well, no, actually they're a native group in North America for almost all of their evolutionary history. Lamas got down to South America and camels got into Eurasia, and then the rest of the group went extinct in North America, in the Ice Age.

So when you do the molecular phylogeny, you're talking about a relic. There's a little tiny bit of biodiversity left of this huge group. Horses are like that. There's a huge tremendous diversity of horses in the fossil record. There's hardly any alive today. Hardly any. So you're just trying to reconstruct. It's a problem, and we try and combine molecular data and morphological data. The problem is that some of the methods that you can use statistically on genetic data are much more accurate, and we can't use them on morphology. So when you combine the data you're stuck using the crappy methods, statistically speaking. We have a better model for how DNA changes over time, because it's a very simple code, right, and so we can predict the statistical property. We know that there are four bases, and there are two sets of two kinds of bases, and they have different shapes, so you're not going to get certain changes as often and so you can weight those differently. You can't do that with morphology yet. We're working on it. Eventually we're going to understand a lot more of the genetic basis of the morphology in terms of pattern regulation genes, but we don't know the rules yet.

01-00:38:28
Burnett: In your other papers that were data based, they were these really interesting studies over tens of millions of years that tried to evaluate changes in the size of mammals, for example, and trying to get a sense of the maximum rate of final evolution. Could you talk a little bit about how some of those studies were done and some of the new conclusions are about the evolution of mammals that were maybe a bit of a departure from what was known or supposed, say, decades ago?

01-00:39:14
Theodor: Those came about because we had a group of researchers who were all really interested in the evolution of body size in mammals. It was spearheaded by Felisa Smith, at the University of New Mexico, and Kate Lyons, who's at the Smithsonian, and an ecologist whose name is Morgan Ernest. They gathered together a group of people that they wanted to get to work on the evolution of size in mammals. When we all got together, it was a big group of paleontologists and macro-ecologists and conservation people and physiological ecologists, so people with varied backgrounds on mammals, not all paleontologists by any stretch of the imagination. When we all got to talking about what things about size that we could tackle that would involve the paleo people as well as all of these people, the most tractable thing we could think of to do was to say, "What are the limits on size over time?" Because in the fossil record it's easy to get at what's the biggest thing. That's always in the literature. You're very unlikely to miss a fossil of something big.

You're very likely to miss the smallest. And the paleontologists were all excited about the project, and we started compiling the data.

We went through, group by group, what's the biggest pig, what's the biggest elephant, what's—in every time interval. That's relatively easy for us to do from the literature, and because they'd selected enough experts from enough areas of mammals who knew enough literature, we were able to do that from the literature, what was in the Paleobiology Database, but we were able to ground truth it with specimens. So all of us were like, okay, we need to know the best estimate of the body mass of each of these. What we would do is we would go, okay, so who's the biggest that we can think of? Let's double check. Okay, we need a mass estimate. How do we do that for that group? So for some groups you use different measurements. For Artiodactyls, the group I work on, we can use dental measurements. They're very strongly correlated with body mass, very easy. For other groups like elephants, we had a graduate student who was working on them and who would go and measure whatever skeletal elements he could, usually the arm if he could get it, to estimate mass, because that's much more accurate for elephants. Tooth size is not as accurate for elephants. You have to know that about the groups to do it.

We compiled this data set and used it to look at what are the maximum limits on body size for mammals in the Cenozoic. We really didn't address the Mesozoic mammals. They're a different problem, because it's a much more sparse record. What we were able to see was something that had sort of been floating around in the literature. It was an idea, but it had never been really demonstrated, which is there is an absolute cap that mammals never exceed, and it's much lower than what you see in dinosaurs. What's interesting is that that's achieved very early on after dinosaurs go extinct. It takes about, basically, before the end of the Eocene, you have rhino-sized animals, so it's not that long. It's like twenty million years, twenty-five million years, something like that. Maximum size goes up really, really fast after dinosaurs go extinct, and then it hits this level, and it doesn't go down again really until the Ice Age. Basically you hit somewhere around twelve to seventeen tons is about as big as a mammal can get. That's really big, but it's nowhere near what dinosaurs can do.

01-00:42:54
Burnett: Right. Was that related to what they ate?

01-00:43:07
Theodor: What it's probably most closely related to is mammalian metabolism, so there are a couple of things that allow us to say that. One of which is that mammals, like birds, live fast, die young. We eat a lot, we burn a lot of heat. Dinosaurs, it had been argued for a long time, were really warm blooded like birds, except that the biggest dinosaurs maybe not. There's a few things about their biology that looked different to us, one of which is if you start to add up the number of really giant sauropods, right, the big Jurassic forms, if they all ate

at the level of mammals, I don't think you could have as many as we do and still have any plant matter left to be eaten.

01-00:43:58
Burnett: So what is that, ectotherms or whatever they're—?

01-00:44:03
Theodor: There's a lot of argument about this in the literature right now, but one of the most recent papers that I think is actually probably right argues that they're around the metabolic level of a Komodo dragon, so elevated compared to a lizard but not at bird level, the metabolism. There's also the fact that dinosaurs have some interesting things called pneumatic systems, so they basically have really light bones. They have air sacs in their bones as part of their breathing. That allows them to get a lot bigger. Mammals can't do that because our bones have to support the mass. We're heavier for the size we are, because we don't have air sacs in the bones.

The last constraint that mammals face, so mammals do some neat things. We don't have to change our ecology as we grow, because when you're an infant you're being fed by your mom, so you're essentially eating the adult diet. Dinosaurs have to change diet as they grew, right, like other reptiles do. When you're small you eat small prey, and when you're bigger you eat bigger prey, and you can switch. Mammals don't have to do that, which has advantages and disadvantages. But at the same time, because we carry the young, we're live-bearing, they can't be a lot smaller than us. If you look at the size difference between a baby dinosaur and an adult dinosaur, they can change much more than we can. So our size is constrained by our mother's size, to a certain degree, and we can't reproduce that fast. If a dinosaur had more food, they could just lay a whole bunch more eggs. We can't. So as mammals get bigger, they have fewer offspring, right. If you look at gestation times, an elephant's gestating two years. You can't have twenty elephant offspring in a cohort from an individual mother. You can from a dinosaur, no problem.

Our reproductive rate is tied to our size. Those things seem to be constraining mammals, the fact that we eat a lot, the fact that we constrain our reproduction with the physical system we're using for our offspring, means that we probably could never get that big.

01-00:46:18
Burnett: But we have advantages, I suppose, in terms of obviously, as mammals, we're wider ranging, we seem to occupy more different kinds of niches.

01-00:46:28
Theodor: We occupy a few niches that reptiles can't. Certainly if you start to look in really cold environments, we thrive. Mammal diversity is high in cold environments. Being fully aquatic is something dinosaurs really didn't do very much, and mammals do very, very well. Although marsupials never get fully aquatic, because they can't, again, because of their reproduction. There's

certainly some new niches that mammals occupy. But I think the biggest thing that mammals do really, really well is we take herbivory to an extreme. We really mess around with our whole feeding apparatus to allow us to eat a really wide variety of foods. So there are certainly dinosaurian herbivores, but if you look at the diversity of specializations in mammals for different kinds of herbivory, it's kind of astonishing.

01-00:47:30
Burnett: That's interesting. We don't have much time left, but could we talk a little bit about the state of the art in terms of research involved in paleontology? High-dynamic-range imaging is something that's popped up in your research lately. Can you talk a little bit about that?

01-00:47:49
Theodor: That came about because my now ex-husband was interested in it as a technique that's used in animation for backgrounds. Basically the idea is that when you take a photograph you only capture a certain range of light values. But if you take multiple photographs and combine them digitally, and that was actually a technique that was developed at Berkeley in I think architecture or physics or something, I don't even remember who, but if you look, HDR [high dynamic range] actually comes out of Berkeley originally. What you do is you take multiple photographs of different f-stops essentially, so you take an exposure bracket. When you combine those values, you can more accurately represent the light in a scene, and you can actually calculate the light in a scene. That allows you to expose areas of a photograph that might be blown out or way under exposed in the same image, when you're combining them digitally. My ex suggested that it might allow us to see more features of skeletons in dark museums and things like that. Then we thought to try it on photographs of tooth microwear, so these microscope images that we do of scratches on teeth for counting, to sort of figure out what the diet is, it turns out we can get a better signature on the microwear if we do a high dynamic range image, because we can see scratches that are a little blown out or kind of problematic under the microscope by doing an HDR bracket. That's how that came about.

01-00:49:21
Burnett: That's wonderful. So it seems that technology is enabling different kinds of analysis.

01-00:49:29
Theodor: Oh, huge. There's one researcher who's been experimenting with all sorts of UV filters and photography and starting to see preserved soft tissue in areas we hadn't realized it was preserved. So there's a lot more in a lot of fossils of things like pterosaurs and early birds and studies on the melanocytes, the pigment-bearing cells in squid turn out to have led us to the idea that we can actually see these in the fossil record in bird feathers and tell what color birds and dinosaurs were. Stuff I never thought we'd be able to get at when I was a kid.

01-00:50:10
Burnett: Yeah. So it's all kinds of ways to infer evidence from the past.

01-00:50:16
Theodor: Yes. I mean, the problem is that whenever one of these ideas comes out it catches everybody's imagination, and people try and use it everywhere without checking to make sure it's true. That's, I think, been a lot of what I find myself doing, especially with new ways of looking at diet, is saying, "Wait a minute. You've been using this method, but what is it capturing of your diet?" There's lots of ways to infer diet for extinct animals. What I try and do that I think is different from some of my colleagues is use as many as I can on the same teeth, to say, "What are they all telling me, and what's the intersection of what they're all telling me?" If somebody has proposed a new method, how does it compare with the ones that we've already used so that I can see how accurate it is? For example, one of my current students is really interested in using a method that one of our archaeologists is using where they look at what's trapped in dental tartar. Well, if you think about eating and what gets stuck in your teeth, is it a representative sample of everything you ate? Maybe not. So the question is how does what we're getting out of the dental tartar intersect with all the other ways we look at diet, right? The scratches on the teeth and the isotopes in the teeth, and does it fit or is it giving us a biased picture? What part of the diet is it telling us? That's what I want my student to try and figure out.

01-00:51:39
Burnett: It sounds to me like, based on your training and your experience, that you advise students and colleagues to be careful of that kind of technological progressivism. That just because it's new, a new technique, doesn't mean it's going to explain everything. You have to triangulate with other, older types of evidence, and you have to have, as a student, a really broad connection between the new techniques and technologies all the way back into the field.

01-00:52:08
Theodor: Yeah. One of the perils of getting really technology obsessed is losing track of the early literature and where the specimens came from and what you know about them. I guess fundamentally I think what UCMP gave me and what Bill gave me is the tie to the specimens always.

01-00:52:23
Burnett: The importance of place. Yeah. Absolutely. Well, on that note I think we'll stop for now, but thank you so much for taking the time to speak with us.

01-00:52:32
Theodor: Oh, you're very welcome. That was not what I was expecting to talk about, but it was fine. [laughs]

01-00:52:39
Burnett: Great.

[End of Interview]

Oral History Center
The Bancroft Library

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Berkeley, California

The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology:

Greg Wilson

Interviews conducted by
Paul Burnett
in 2015

It is recommended that this oral history be cited as follows:

Greg Wilson. “Greg Wilson: The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology” conducted by Paul Burnett interviewer in 2015, Oral History Center of the Bancroft Library, The Bancroft Library, University of California, Berkeley, 2017.

Greg Wilson, 2008

Greg Wilson is Associate Professor of Biology at the University of Washington. He received his PhD from UC Berkeley in 2004.

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01-00:00:06
Burnett: This is Paul Burnett interviewing Dr. Greg Wilson for the UCMP Bill Clemens Oral History Project. It's March 19, 2015, and we're here in the Bancroft Library. Welcome, Dr. Wilson.

01-00:00:20
Wilson: Thanks.

01-00:00:21
Burnett: Let's begin at the beginning. Can you tell me a little bit about where you're from, where you grew up?

01-00:00:26
Wilson: Sure. I grew up in Kalamazoo, Michigan, southwestern Michigan, a fairly small town, in 1973, and stayed in the same house my entire life. Went to a Catholic grade school and a public high school. My dad is a community college teacher. My mom is a nurse. She's of Colombian descent, and my father is from Indiana.

01-00:00:57
Burnett: Can you tell me a little bit about how you first became interested in science, generally speaking?

01-00:01:07
Wilson: I think I became interested in science probably through both my parents, but my mom as a nurse certainly encouraged me to pursue medicine, wanted me to become a doctor. I think that sort of gatewayed me into biology. As a kid, I would go outside in our backyard and the woods and so on and collect frogs and bugs and so on, and try to keep them in jars and whatnot. I think that was my initial entry point into biology and science. We also would collect rocks and attempt to find gold in the hills behind my house, which was unsuccessful. Anyway, that was my first entry point into geology.

01-00:01:56
Burnett: I think a lot of young people have found lots of iron pyrite.

01-00:01:59
Wilson: Yes, that's what I found. [laughter]

01-00:02:02
Burnett: "I've got gold!" How was the science education? Did you have any inspiring teachers, or were you drawn to it, or not inspiring teachers?

01-00:02:15
Wilson: I did. In high school, I went to a public high school, but for half the day, I went to a magnet school for our county, the Kalamazoo Math and Science Center. That drew on many different schools in the county and surrounding

area, and I had a number of very motivated high school teachers that really pushed me. I think up until that point, I had been having an easy time in school, and at the Math and Science Center, I encountered my first failures. I think that really sort of made me work hard at it and really start to enjoy the struggle, the challenges that come with pursuing academia. I would point to that Kalamazoo Area Math and Science Center as a pretty formative point in my career trajectory.

01-00:03:08
Burnett: A lot of young folks encounter the struggle. People struggle with math at one point or another. It's a question of when. A lot of people, their first time they encounter the struggle, they say, "I'm not a math person." What pushed you over the edge to say, "This is a challenge; I'm going to go for it"?

01-00:03:31
Wilson: I think the first math test I had at this Math and Science Center quickly found that people from these different school districts had a better background than I did, that I received at my Catholic grade school. I failed the first test that I took there. I think, from my athletic background, I've had to face challenges before and develop some sort of—I think they would refer to it as grittiness these days. My parents certainly saw grades as something to pursue. I think they wanted me to get as high grades as I possibly could. They didn't talk to me quite as much about learn the subject matter and really enjoy it. They wanted the grades so I could get on to a good college. I certainly remember struggling long nights in high school, when I think some other people speak of having a very easy time in high school. I struggled quite a bit with those math courses, but I built confidence in my ability to take on those sorts of challenges through some of these early failures.

01-00:04:47
Burnett: Did you like the math?

01-00:04:50
Wilson: I did. I've always liked the challenge of complex problems. I think math has a lot of these things that you need to struggle with them for a little bit to sort of bring in different aspects of your learning to sort of derive an answer. I enjoyed it as a puzzle.

01-00:05:19
Burnett: Obviously, you did well, because you end up—and your transition to college. Can you talk a little bit about that?

01-00:05:29
Wilson: You asked me the origins of my interest in biology. I think most of my childhood was spent pursuing a dream of playing professional and World Cup-level soccer, and getting the good grades was a way for me to be able to go to the school to play soccer that I wanted to go to. When I finished high school, I was the valedictorian and had good grades, et cetera. I was choosing among various schools, many of the Ivy League schools, and I went on visits

to places, and Stanford was the school that I went to, and I fell in love with California and the academics and the athletics that was there. I know I shouldn't be saying that here on this campus, but—[laughter]

01-00:06:18
Burnett: That's all right.

01-00:06:21
Wilson: That choice was made in my mind very quickly after going there. Financially, it was something that wasn't part of the plan for my parents, so I really had to struggle and plead to try to get them to help out with that, and then I had to search for a lot of scholarships and do the financial aid route, and work jobs through college. I certainly had my eyes set on going to Stanford. That was the first time I had been to California, and it was a different place. It was far from home. I had a very good education experience there as well.

01-00:07:09
Burnett: Very obviously world-class in terms of life sciences. When was the first indication that you might be interested in paleontology, apart from your early experiments with explorations with rocks?

01-00:07:24
Wilson: It's interesting. My brother is a paleontologist. He's four years ahead of me. Right as I was starting my first year in college, he was starting to apply to graduate schools, and one of the graduate schools that he was applying to was for paleontology. It sort of left a little seed in my head, and as I went through my college education at Stanford, I started to get into the human biology program that they had there, because of medicine as a possible future career, and got into genetics, and I got into using genetics to understand the origins of human populations. I linked up with a couple professors that were doing that. I started with human genetics, and then I moved to human genetics and human origins, and linked up with people that were looking at genetics to do that, and then people who were using fossils to look at the origins of humans. Luca Cavalli-Sforza at Stanford, and a postdoc of his, Joanna Mountain, were some people I linked up with on the human genetics side of things. Then Richard Klein is a paleoanthropologist at Stanford that I linked up with as well.

But all the while, I was playing soccer, and that was drawing a lot of my time. I don't think I was a serious student until probably my junior or senior year in college. Then I started to think about career paths, and I thought about medicine and got a couple experiences outside of college, trying on that possible career path, and it didn't suit me. I didn't feel that it gave me enough flexibility. I also thought about veterinarian medicine. But I ended up graduating from Stanford and then taking some experiences before I entered graduate school. One of those experiences was living with my brother and his grad school mates in Chicago and working at the University of Chicago lab that my brother was in. This is a dinosaur paleontologist by the name of Paul

Sereno. I spent about four or five months working there, and that was my first serious foray into what would end up being my career path in paleontology.

01-00:10:03
Burnett: Just the exposure to the work that was being done, once you saw the questions that they were working on—was it the intellectual questions or was it the kind of work process, the kind of work practice? What was the most attractive thing to you?

01-00:10:25
Wilson: I think I wasn't quite hooked yet by then. It was sort of romantic. There was a lot of adventure. There was a lot of thinking about deep time and the expansive evolutionary history and things like that that were attractive. They sat around and drank beer and talked about cool things about new publications and expeditions, and going to Africa, and who was doing what. It was attractive from a superficial level that I was able to interact with it at. But the questions and that excitement didn't quite sink in until later, when I started with Bill, actually. But I did, before starting graduate school, go on an expedition to Africa, to the Sahara Desert, Niger, in 1997. That was my first paleo field experience, and I think most people don't jump into the deep end of the pool like that. It was four months in the field. Maybe once a month, go into town to get a shower. The rest of the time, sleeping outdoors on a cot in the middle of the Sahara Desert. Warm drinks all the time and no refrigeration. Dried bags of food that we'd have to rehydrate and things like that. It was a pretty rough experience, but it was the only thing that I knew, so I didn't have any other sense to think, oh, this is harder than some other experiences.

01-00:12:24
Burnett: That was through Stanford that you went?

01-00:12:27
Wilson: No. I graduated from Stanford University in 1995. In the fall of 1995, I spent some time at the University of Chicago working with Paul Sereno in his lab, and my brother, Jeff Wilson. Then I ran out of money and just moved back home with my parents in Kalamazoo for a while. I worked in a hospital and so on. At some point, Paul asked me to go on their expedition that was to take place in the fall of 1997. In the fall of 1997, by that time I had already submitted applications to go to grad school, and I got this experience to go to Africa for four months, and I didn't blink. I jumped right on that opportunity.

01-00:13:24
Burnett: At that point, you'd say you were hooked on the practice side, maybe?

01-00:13:33
Wilson: I certainly loved the experience. It incorporated all kinds of different skill sets. It was very physical. There was loading heavy jackets, plaster jackets that had dinosaur fossils, onto the backs of trucks in the middle of the desert, with very little machinery to help out. Hiking long distances. Putting up with rugged terrain and temperatures. That adventure aspect was exhilarating and really

captured my interest. I was on the periphery of a twenty-person expedition, so I wasn't in the inner circle to discuss many of the scientific ideas behind what we were doing, so I didn't really get into that quite yet, but I was starting to get into the literature and poke around a bit about what I might want to study at that point. I was a little unsure of pursuing a career in paleontology, where it might leave me. I was headed towards graduate school, but I still wasn't certain about pursuing it long-term.

01-00:14:52
Burnett: And so you applied to graduate schools?

01-00:14:56
Wilson: I applied to graduate school. I only applied to one school. I had three applications that I was going to prepare, and I ended up only applying to Berkeley. I advise students now, and I would never advise them to only submit one application to one place. But I only applied to Berkeley, and I think I had some nostalgia for the Bay Area. I had read up a little bit about Berkeley and its history as a place where paleontology had been done for many years. But I was uncertain about what I wanted to do, so I didn't send out many applications. I sent out the one and I kind of put it in fate's hands.

01-00:15:40
Burnett: And they bit.

01-00:15:41
Wilson: They bit. They offered me admission and asked who I wanted to work with. I think Bill knows this. I didn't really have a good handle on what Bill did or who Bill was relative to other people. Kevin Padian actually called me up on the phone and offered me admission. He asked me who I wanted to work with. He said, "Do you want to work on dinosaurs or do you want to work on mammals?" I said, "What do you work on?" He said dinosaurs, and I sort of reacted and I said, "I want to work on mammals." [laughter] I offended Kevin right out of the gate. Then Bill called me up, and at that point—I think it was spring of 1997—he called me up and I told him that I would like to go on this expedition to Niger beforehand, and he permitted me to go away for the first quarter of my—when I should have started was in the fall of 1997, but I deferred until the winter, so beginning of 1998 is when I showed up on campus.

01-00:16:55
Burnett: So the Niger expedition was kind of part of the graduate school experience? Is prior to, but—

01-00:17:02
Wilson: It was just before. It was just before, and it wasn't associated with any of my graduate studies, really.

01-00:17:09
Burnett: The expedition, was that mammalian stuff that they were looking at? Was it dinosaurs?

01-00:17:13
Wilson: It was dinosaurs. I would go back with them in 2000, and I would become the mammal person in those expeditions. In 1997, it was a mainly dinosaur expedition. They had found in previous expeditions some dinosaur skeletons that they wanted excavated, and they wanted to prospect for more material.

01-00:17:39
Burnett: I'm fishing here, but is there a link, perhaps—you were interested in human origins initially. Was the mammalian stuff—you wanted to do the ultimate in human origins?

01-00:17:50
Wilson: I think there was some of that, and I was probably partitioning, or separating myself from my brother and his peers, who were all working on dinosaurs. I felt like maybe I should do my own thing to separate myself a little bit. I've been more interested in mammals, just in growing up, in terms of my interest in diversity of life. I'd been interested in mammals. It was more towards my liking. Didn't have anything to do with Kevin. [laughter]

01-00:18:23
Burnett: It seems to be a less visible field. On the one hand, everybody knows about dinosaur paleontology, and everyone knows about the search for human origins, and that handful of early hominins and so on. Then there's this middle area that people don't know as much about, which is the search for mammalian origins. I wonder if you could talk a little bit about what you encountered when you started to work with Bill. What the state of the field was, what the burning questions were, and where you thought you might fit into that field?

01-00:19:09
Wilson: I don't think I fully had a grasp of the field. I certainly thought, when I decided to work on mammals, that I'd probably be working on something like mammoths or saber-tooth tigers, or something more appealing to the public than what Bill works on, which are mammals from the age of dinosaurs, represented in the record by tiny teeth that we stick on the heads of pins. Once you look at them under the microscope, they're beautiful little jewels, but to the broader public, they're not the skeletons that are exhibited in museums. It took Bill some time to be able to convince me that this was a very interesting time period to be working in, and it was exciting, and that I could become passionate about it. But Bill has always been about questions, and I think he did a really compelling job setting up the questions that he was interested in, and convincing me that this was a very fertile area to be pursuing research, and asking interesting questions. I got hooked on it after a little bit of time. I really got hooked on it. I talked about that experience I had in the field in Niger, which was very exhilarating for much of the physicality of doing all

these things in tough conditions. With Bill, it was much more of a cerebral exhilaration that was going on out in the field with him, and I really got passionate about that. That's where I really think I latched on, and I knew that I was going to continue pursuing this.

01-00:21:27
Burnett: You did fieldwork. Well, you did this prior to starting graduate—you were in the field. You weren't doing fieldwork, necessarily. You were part of the field expedition. Then you did, presumably, your foundational coursework. Then you had begun, by working with Bill, to think about some of the bigger questions. When it came time for you to develop a thesis, what drew you to the question that you ended up focusing on?

01-00:22:03
Wilson: I will say that, before I had gone out to the field with Bill, when I really got hooked, I spent the first semester going through that crisis of confidence. I went into Bill's office towards the end of that first semester and I told him I was going to quit. I told him that I think it's not right for me. Bill said, "Just wait. I'll help you move on to whatever you want to move on to, but just come out to the field one summer." Then it was that summer that he really hooked me and got me interested in those questions about the extinction that killed off the dinosaurs and a lot of other vertebrates and other organisms, and what precipitated the rise of mammals that we know today. It was that sort of large-scale setup that Bill really laid out for me that got my wheels turning in a way that I could develop a project. Bill really opened up a number of the projects that he had going and sort of availed me to what's been done, what people are doing, and what hasn't been done. The one thing that I saw was that there was a large mountain of work that had been done looking at the extinction of dinosaurs and the rise of mammals, and there was a great fossil record that Bill had assembled, and Bill's teams had assembled, through the years, but they hadn't analyzed things with modern quantitative approaches that were now available. That was kind of where I found my niche, in the ability to address some old questions and some new questions.

01-00:24:03
Burnett: You had slogged through the math and excelled at the math, so you had that ready, and you were able to jump in and do that. They had amassed this incredible collection of fossils, I think something like 12,000 specimens. Just for the study that you ended up doing, you took a sample of that, but they had collected—and not just collected, but—collection just implies picking something up, but it's this whole work practice, or set of work practices, around cataloging and situating each of the finds so that you can be very careful about the stratigraphy and the dating of these specimens. An earlier question I have, I suppose, is that it was interesting that you said that Bill dealt with your doubt by proposing the field expedition. Is there something about being there that really allows Bill and others to fully explain to someone the importance of what they're doing, or the connection between the minutiae of the day-to-day work practice, and these large, ultimate questions?

01-00:25:40
Wilson: The analogy that I like to use now is that the area in northeastern Montana that we work in, this Hell Creek area, the study area, is sort of like the model organism—the fruit fly, or the lab rat—for studying this extinction and this rise of mammals. It's the model system. It's a classroom, because it's all there. It's all laid out. And so Bill held class for me. He is pretty good at developing a Socratic sort of dialogue about the sorts of questions he's thinking about and putting them towards you, and sort of working through the thought process right there on the outcrop, with fossils, with the evidence he knows from historical studies, and so on, and with the geology right there. You can test hypotheses right there in the field. I think that was a very illuminating experience for me, and one that sort of set the course for everything that I do now in the field. I think he was very wise in luring me out to the field. The other thing is that the experience out there does get into your blood, into your system, the community, the locals there, the routine, the joy of collecting and being around camp with people, et cetera. All of that was very enticing, and I grew to love it, and I still do.

01-00:27:41
Burnett: You used the word earlier, "romantic." In a sense, it's literally true. The fieldwork has elements of nineteenth-century fieldwork. You go out and you're against the elements, and you're with your team or your crew, and there's camaraderie. Other interviewees have talked about the importance of being outside. Some people said, "I wanted a job or career where I could be outside. It was really important to me." I think that's a feature for some people, an attraction.

01-00:28:21
Wilson: Certainly, I think, building and contributing to something together as a team, enduring challenges that occur out there, whether they're the local politics, or navigating to get onto private land, or slogging hundreds of pounds of sediment in burlap sacks on your back a couple miles, and then finding that one mammal jaw within that amount of sediment you carried. Just those sort of little rewards kept us going, and I think makes it so rewarding.

01-00:29:08
Burnett: Bill had talked about the way in which it was, in some ways, a lot easier, to—in terms of the legal procedures way back when, those were simpler times, and you could just ask permission to go on someone's land, and they were like, "Sure." Things have changed a little bit in the interim.

01-00:29:29
Wilson: Things have changed a lot. Things like the sale of the tyrannosaurus rex for $7 million really impacted what local people thought they now had on their land when they knew that they had fossils back there. Regardless of whether they had a complete T-rex sitting in their backyard or not, they thought all fossils must be worth a lot, and these paleontologists are—we might want to negotiate a little bit more about letting them on our land. Then the state and

the federal government also started to develop plans for how to manage these lands and stricter policies on who should be allowed to collect fossils, and how they collect fossils, and so on. This was a lot of navigating of the logistics that occurs now. Now I have to get three different permits to do the work out in the field that we do, and then private permission from the different landowners. It's a process that I don't think existed at the same degree back in the seventies and eighties.

01-00:30:41
Burnett: We were talking earlier about the new techniques that had become available to analyze large quantities of specimens in new ways. Could you talk a little bit about how the formulation of your thesis came into being and how you developed it further?

01-00:31:01
Wilson: Students before me, graduate students of Bill's before me, and Bill, had looked at change across this important boundary, the K-T Boundary, and looking at a coarser scale than I would eventually look at the change. So there was a matter of temporal resolution in the rocks that we were able to add through, as you were mentioning earlier, documenting where fossil localities fall relative to the rock record and the timeline that we've been able to apply to this rock record. There was that component, higher resolution in time, and then there were numerical approaches to data that computers allowed us to apply to large data sets. One, you needed a large data set, and that took twenty, thirty years to develop, and so people like Bill and Dave Archibald, Don Lofgren, people before me, were able to contribute to it. I contributed to it. The numerical approaches in the computer technology that was available to me at that time allowed me to analyze them in ways that hadn't been possible before, really. Then I also did some imaging of fossils and comparing shapes of mammal teeth in a way that was more sophisticated than the qualitative approaches that people were taking before.

01-00:32:51
Burnett: Three-dimensional imaging?

01-00:32:53
Wilson: I didn't do three-dimensional. It wasn't cost-effective or available on campus at that time. I do that now. But at that time, I did two-dimensional images of teeth, and with computers, I digitized different points on the teeth and then compared the configurations of those points across different species and across time. Essentially, the shape of the teeth is reflecting something about the diet of the animals, so by comparing the shapes of teeth across different mammals that lived at the same time, and across time, I was looking at the range of diets that these animals were feeding on during particular time intervals in and across the extinction event.

01-00:33:44
Burnett: That explains in a nutshell why teeth are so important. It's partly what's available in terms of, when you're doing the screen washings, this is what comes up, but what you can learn from this is obviously the dentition evolved to eat different kinds of foods.

01-00:34:06
Wilson: It's unfortunate that the record isn't typically more complete, but it's fortunate that the aspect that we get is such an informative component of the animal's body. So we can learn about body size from teeth, we can learn about taxonomy, so we can identify things to species, and we can learn about diet from teeth. So it was quite a bit of information in that aspect of the body.

01-00:34:31
Burnett: Getting a sense of the range, and perhaps the competition with other species, and the prevalence of that species in a particular area.

01-00:34:41
Wilson: Right. How they're dividing up the ecological niches that are available to them, and what diets they're expanding into, which diet space they're expanding to changed across the boundary. That's something important to learn from the fossil record.

01-00:35:06
Burnett: You had mentioned that the research that had been done earlier had given a fairly coarse impression of the survival of species across the K-T Boundary, and also the evolution of species after, which seems to be almost more important. The extinction is pretty well documented, but what the extinction meant—we know that dinosaur species went extinct—but what it meant for mammalian species is really a burning question, it seemed.

01-00:35:44
Wilson: I'll say that the counter-perception is that we know everything we need to know about the extinction, but again, I mentioned that Montana is the sort of model system, and there aren't a lot of alternative views. It's a very myopic view of this extinction event, restricted to northeastern Montana. But even in northeastern Montana, we had lumped, previously, about two-million-years' worth of time as what existed before, and then the next-million-years' worth of time of what survived. With the new resolution and—the collecting that I did was in the very base of the Hell Creek Formation, so the beginning of that two-million-year window. I did a lot of collecting down there, in that part of the stratigraphic section, to add to our expanse of time that we could look at. So we could look at changes within that two million years before the impact event. Why that's important is because an alternative hypothesis to the impact event—the asteroid impact event being the sole cause of the extinction—an alternative to that is that volcanism that was going on in India had an effect on the atmosphere.

01-00:36:58
Burnett: The Deccan Traps.

01-00:36:58
Wilson: That's right, the Deccan Traps. That volcanism was occurring within the last 500,000 years or so of the Cretaceous. So if you don't have that window of time during the last part of the Cretaceous finely resolved, you can't distinguish between the effects of the volcanism and the effects of just the impact. My work in resolving the record in the last two million years allowed us to start to test this idea that volcanism had an effect on the ecosystems before the impact happened.

01-00:37:38
Burnett: What were some of your conclusions, that that was in fact—?

01-00:37:43
Wilson: When I was a graduate student, I looked only at the mammals through this two-million-years' worth of time, and what I found was that, in the middle of the Hell Creek Formation, which seems to correlate with when the volcanism starts, you see the beginning of a decline in relative abundances of particular groups. You don't see extinctions, but you do see the beginning of what we might call ecological decline, so uneven communities, that today we use in looking at ecological disasters, like oil spills or deforestation. Some of those more subtle signals were evident only because we had this very large sample of mammal teeth and we had this fine temporal resolution in the Cretaceous now.

01-00:38:40
Burnett: This involved a lot of computer analysis, and statistical analysis, effectively, right, that you're—

01-00:38:53
Wilson: There was a lot of numerical, statistical analysis of the data, these large data sets that other people had accumulated and I had accumulated. I think it would be unfair to suggest that I just took the data and just analyzed it. I did quite a bit of fieldwork and slogging through bags of sediment to pull out those mammal teeth, and my undergraduates that worked in our lab did that, too. We did a lot of numerical treatment of the data, but contributed a lot of data as well.

01-00:39:33
Burnett: And so you finished your thesis and graduated in 2004. It was a quantitative analysis of mammalian change across the Cretaceous-Tertiary Boundary in Montana. Then the next step is that you transitioned to a museum in Denver. Can you talk a little bit about that experience?

01-00:40:00
Wilson: Before I did that, as I was graduating, I applied for an NSF postdoctoral fellowship, and I applied for the job at the Denver Museum as a curator there, and I got both of those, and I negotiated with my boss at the Denver Museum

to take part of my postdoc. My postdoc was from December 2004 through July of 2005, and spent in Helsinki, in Finland, working with Dr. Jukka Jernvall, who is an evolutionary biologist and an evo-devo, or evolution and development person, who works on mammal teeth. Was a shortened postdoc stint, but an important one, and one that I maintain connections with.

01-00:41:02
Burnett: Did it open new avenues of investigation for you in terms of thinking about the problems in different ways?

01-00:41:07
Wilson: Yeah. I had mentioned before that I had analyzed teeth in two dimensions to look at shape of teeth, and I used little digitized landmarks on teeth, and I digitized the same landmarks across teeth. The problem is you can only compare apples and apples. So if the teeth look too different from one another, you can't really compare them, because you can't find the same landmarks to digitize. In Helsinki, it was Jukka Jernvall and another postdoc, Alistair Evans, and myself who developed a new approach for analyzing teeth that allowed you to compare apples and oranges, and used three-dimensional laser scanning technology and geographic information systems analysis, or GIS analysis, which is most often used to look at watersheds or mountain ranges. But these little teeth have little watersheds and mountain ranges on them, effectively, because they have bumps and valleys. So we took that technology and applied it to three-dimensional models of teeth, and we could start to quantify the complexity of the surface of these teeth. We did that in modern mammals and found that there was a relationship between how complex the surface was and what sort of foods they were eating. We got around this problem of having teeth that were too different from each other that didn't have the same landmarks. We could start to put them in the same analysis, and really put information about diet to numbers like dental complexity, this quantification of the surface that we could get. So it was a really important time for me to learn and develop this technique.

01-00:42:54
Burnett: That's really exciting, and the importance of you had to test the hypothesis by looking at modern mammals, because you knew for sure what their diet was, and then you could develop some kind of thesis as to how the complexity affects diet or is related to diet. So that was an important moment for you. Can you talk about the next career transitions and how that enabled you to take the next step in your research?

01-00:43:28
Wilson: After I got back from Helsinki in July of 2005, I started as a curator at the Denver Museum of Nature and Science, and I was curator of mammals, essentially. It was a big step for me. Basically, I was professional at that time. I had a collection of fossils that I was in charge of, and I interacted with museum exhibit people, museum outreach people, other researchers. I was in a new area. There's a lot to learn, and I definitely took some time in the

beginning there to slowly grow into that position, because it's not often that people finish their PhD and start right in on a job. It was just fortunate that the timing was such that the perfect job for me at the Denver Museum showed up when I was ready after finishing my PhD I certainly had to grow into those shoes there, and I was still working on questions around the K-T Boundary, looking in some new areas in Colorado in the Denver Basin and so on, but still also going up to northeastern Montana. I had developed my own crew of volunteers there that came into the museum and sorted through sediment, pulled out fossils, and they came out to the field with me. It was fun. It was an exciting time for me to sort of venture out into my own area and develop my own program. But I still maintained a lot of connection with Bill and met with him in the field and collaborated with him on various projects.

01-00:45:21
Burnett: Could you talk a little bit about the importance of outreach in the discipline of paleontology? It seems to be perhaps more significant than in other scientific disciplines, or more important to the health of the field, perhaps.

01-00:45:40
Wilson: Dinosaurs, and deep time, sort of appeal to our sense of wonderment, I think, and particularly in kids. I think, for that reason, it's been a gateway to science for many people. I think paleontologists have recognized this and taken it upon themselves to really develop outreach programs. When I was at the Denver Museum, I started taking volunteers out into the field with me, and really building off my experiences with Bill, about how he used the field as a classroom. I started to use it as a classroom for interested volunteers, many of them retirees and so on, but a few teachers here and there. As I continued out in the field, I realized I wanted to give back to the local community in northeastern Montana, and I also wanted to capitalize on this outdoor classroom. So I started a program called the DIG Field School to really bring K-12 science educators out and show them geology, paleontology, and talk to them about evolution in the field in a way that was—the hope was to give them confidence to really speak with enthusiasm and confidence about these topics that I think are very engaging ways to reach their students, giving them real experience in what scientific research was about hypothesis testing. Next Generation Science Standards is a big thing nowadays in the past few years. It's been fortunate that we seem to have caught onto it before it became a standard across the school districts and so on. We were taking that approach of putting the teachers in the driver's seat, or putting the people that we brought out in the field in the driver's seat, of testing hypotheses and posing questions to them in a sort of Socratic Method that Bill kind of raised me academically. I first took out, I think, seven teachers from Montana, and now we take out thirty teachers a year, from not only Montana but Washington, Ohio, Massachusetts.

01-00:48:29
Burnett: It's expanding into other states as far away as Massachusetts.

01-00:48:32
Wilson: Yeah. We're accepting applications now for our DIG Field School, and we have fifty applicants right now, and I think the deadline is in April, so we'll certainly get up to about a hundred applicants from all over the country.

01-00:48:47
Burnett: That's fantastic. What jumped out at me from your resume was this story about the K-12 field school. You did crowd funding for it. So you're getting into a whole other domain of marshaling resources for this kind of activity.

01-00:49:10
Wilson: I initially did it through my own startup, and pro bono. I used my research funds to support it. Two thousand and ten, I think, was the first year we did it, and we've grown more sophisticated every year with curriculum that we've developed, and these boxes with fossils and objects and so on that teachers can take to their classrooms and so on. We've really built the program into a sophisticated operation that continues on throughout the year, not just the four days we take them into the field. That's required funding, external funding. One of the first things that we did to generate funds to support it was this crowd-sourcing on one of these websites that helps you develop this crowd-funding network. We raised more than $10,000 through that.

01-00:50:10
Burnett: So this outreach is an important part of the job that you've taken to heart from that time in Denver, right up to the present day. You make another transition in 2007. Can you talk about that?

01-00:50:30
Wilson: I have to step back a little bit, because in my graduate school years here at Berkeley, I met a paleobotanist that would become my wife, and two academics—it isn't easy to find jobs in the same spot.

01-00:50:49
Burnett: I agree, yes.

01-00:50:51
Wilson: So the two-body problem persisted for a couple of years. When I was in Denver, she was doing postdocs in Sweden—she's from Sweden—and then at the Smithsonian in Washington, D.C. We tried to find jobs in the same place in Colorado, but we couldn't manage it. But she was quite successful on the job market and had two job offers, one at the University of Illinois in Champaign-Urbana, and then one at the University of Washington in Seattle. Both places offered to interview me for positions, and the positions weren't determined as tenure-track at that point until I came to interview. So I went on interviews to Champaign-Urbana and to Seattle, at the University of Washington, and we were offered jobs at both the places. We had to choose between Seattle and Champaign-Urbana, and Seattle made a pretty darn good case. It had a very thriving biology department. A few paleontologists there, including one of the people that I went to Niger with was a curator there

already. That was attractive, as well as the fact that they had a museum, a paleontology museum there, and it was on the West Coast, and I had grown accustomed to the West Coast. We moved to the University of Washington in December of 2007 and started our jobs there in 2008 as assistant professors. She is curator of paleobotany as well as the Burke Museum, and I'm adjunct curator at the Burke Museum.

01-00:52:40
Burnett: I imagine this is also someone with whom you converse about your area, because you're interested in the things that your subjects eat, and she knows a little bit about that.

01-00:52:54
Wilson: That's right. My family includes my brother, who's a paleontologist, my wife, who's a paleobotanist, and my sister, who's a nurse, who hates going to Thanksgiving and listening to these conversations.

01-00:53:13
Burnett: You got settled in there, and you've now reached, recently, associate [professor]. Congratulations.

01-00:53:22
Wilson: Thank you.

01-00:53:24
Burnett: You've published a number of really interesting publications having to do with mammals across the K-T Boundary, and also, I guess, the evolutionary robustness of various mammalian species, and how you determine that. I'm fascinated by the number of conclusions that you were able to suggest from the research that you did. Could you talk briefly about some of the more significant findings in recent years that you've come across in the publications that you've brought out in the last several years since you became professor? You can pick and choose. There's no pressure.

01-00:54:15
Wilson: Two thousand twelve, we published some research that built on my postdoc at the University of Helsinki, where we developed this technique to analyze tooth typographies and quantify dental complexity and its relationship to diet. We did that initially in 2007 on modern mammals. The goal for me was always to apply it to the fossil record and use that technique to start understanding something about diet in long-extinct animals that we don't have modern analogs of. One particular group that was highly successful in the numbers of species and numbers of individuals on the landscape was a group of mammals called multituberculates. They don't exist today, but they were rodents before there were rodents. They're very similar to rodents today. I wanted to learn something about how they became so successful, and I wanted to learn something about their diet, so we applied this technique, analyzing the complexity of their teeth, and what we found was something counter to sort of the conventional wisdom about mammals during the age of dinosaurs.

Conventional wisdom is that mammals during the age of dinosaurs were ecologically constrained to the dark shadows. They were nocturnal, they were mostly terrestrial, they were mostly eating insects, and they were small in body size. When we did this complexity analysis on this group multituberculates, they show an expansion in numbers of species and their body size, and in the range of dental complexity values. Twenty million years before dinosaurs go extinct, so during the time of dinosaurs, these things were radiating. They weren't being hemmed in by dinosaurs occupying all the important niches. Their teeth, and this complexity data, showed us that what they were doing was starting to expand into eating plants. What we found was that, at the same time, if we look at the paleobotanical record, the record of plants, flowering plants were starting to become ecologically dominant at the same time that multituberculates were radiating into new forms of teeth that could take advantage of this new dietary resource. It was against conventional wisdom, and it was a nice quantitative study across an important group and a long expanse of time. That was a very fun paper. Difficult paper, but fun paper to put together and publish. It got a little bit of press. That was one contribution that I'm proud of.

Then, with a couple of other editors, Bill included in that editorial list, we just recently published an edited volume that really was the state of the study system in the Hell Creek area, this area that we'd been working in, for Bill, for more than forty years, and me for fifteen or sixteen years. We published the latest state of that study system in 2014, and that covered a variety of different topics from different vertebrate groups and how they fared across the K-T Boundary, to the geology and the time framework we have there, et cetera. My work focused on the mammals, and I published a sweeping paper across the Cretaceous through the early Paleocene, looking at this immense database of mammalian fossils, and made some conclusions about the extinction that killed off the dinosaurs, in which I suggest that the volcanism seems to have had some effect on ecosystems prior to the asteroid impact hitting at the K-T Boundary. So there seems to be some signs of deterioration in the mammal communities. Then, post-extinction, the analyses that I did reveal some new things about how mammals recover during the succeeding million years, and some interesting things about the rise of the ancestors of ungulates. So the ancestors to pigs and deer and horses, et cetera. And our ancestors, primate ancestors, and how they started to really become the dominant groups on the landscape in northeastern Montana within the 600,000 years after the extinction of dinosaurs. That was a large analysis that I was very proud of as well, and built upon work of people like Bill and Dave Archibald and Don Lofgren.

01-00:59:43
Burnett: It sounds like, in a way, it's a confirmation of what they were saying pretty early on. When the Alvarezes [Luis and Walter Alvarez] came out with the hypothesis of the impact, they were suggesting that, well, that doesn't seem to

square with what we know. There seemed to be a decline for a long time prior. The fine-grain analysis you did wasn't even possible at that time.

01-01:00:17
Wilson: Certainly the granular scale that we could apply because of all of the geology and the paleo collecting that happened in the intervening years, and the technology catching up, was essential.

01-01:00:35
Burnett: Your earlier paper on the evidence of a long period of radiation for the mammalian species, for twenty million years prior to the impact, was that some evidence that the mammals were ripe to occupy new niches because they were already doing so? Was there a path dependence of that, or is that too far to stretch it?

01-01:01:04
Wilson: The group that radiated these multituberculates seem to be somewhat unique in radiating before the extinction of dinosaurs. The group that you and I belong to, and that a common opossum belongs to, do not seem to have radiated until after the extinction of dinosaurs. There's just some complexity to the story that hadn't been appreciated before. In those groups, they didn't radiate until after the extinction of dinosaurs, really.

01-01:01:47
Burnett: I'll give you the last word on the impact that Bill's teaching, and the other teachers as well, the other scholars at UCMP, and your colleagues in your cohort as well, had on your research. Is there a UCMP style?

01-01:02:15
Wilson: You said the word "scholar," and I think Bill certainly is a very thorough researcher, very cautious and conservative, and takes his time to build a very airtight case. Doesn't step too far beyond the data at all. I think he left a very strong impression upon me with that, and I think, in terms of the UCMP, when I was here, there was a pride in collecting fossils and being close to the data. Knowing the field area, knowing the geology, knowing how the fossils came out of the ground and got into that museum drawer, not just walking into a museum and opening a drawer and looking at the specimen outside of that context, and being grounded in the data. There's a growing effort to increase the quantitative approach to paleontology. Large data sets that need to be assembled across many different museums and across great expanses of geologic time, which lead to some really fantastic conclusions, but oftentimes the researcher who's performing these analyses do not know the finer points of each individual datapoint and whether they're actually verified contents of the data. I think I certainly received the lesson that it's great to aspire to these big questions, but keep your feet on the ground as far as understanding where the data are from. Bill was a very strong proponent of that, and I think that UCMP has this long history of that, of collecting fossils and studying individual specimens, as well as thinking about big questions.

01-01:04:27
Burnett: Right, this marriage of theory and practice. Thank you very much for your time.

01-01:04:32
Wilson: Thanks.

[End of Interview]

Oral History Center
The Bancroft Library

University of California
Berkeley, California

The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology:

Mark Goodwin

Interviews conducted by
Paul Burnett
in 2015

Since 1954 the Oral History Center of the Bancroft Library, formerly the Regional Oral History Office, has been interviewing leading participants in or well-placed witnesses to major events in the development of Northern California, the West, and the nation. Oral History is a method of collecting historical information through tape-recorded interviews between a narrator with firsthand knowledge of historically significant events and a well-informed interviewer, with the goal of preserving substantive additions to the historical record. The tape recording is transcribed, lightly edited for continuity and clarity, and reviewed by the interviewee. The corrected manuscript is bound with photographs and illustrative materials and placed in The Bancroft Library at the University of California, Berkeley, and in other research collections for scholarly use. Because it is primary material, oral history is not intended to present the final, verified, or complete narrative of events. It is a spoken account, offered by the interviewee in response to questioning, and as such it is reflective, partisan, deeply involved, and irreplaceable.

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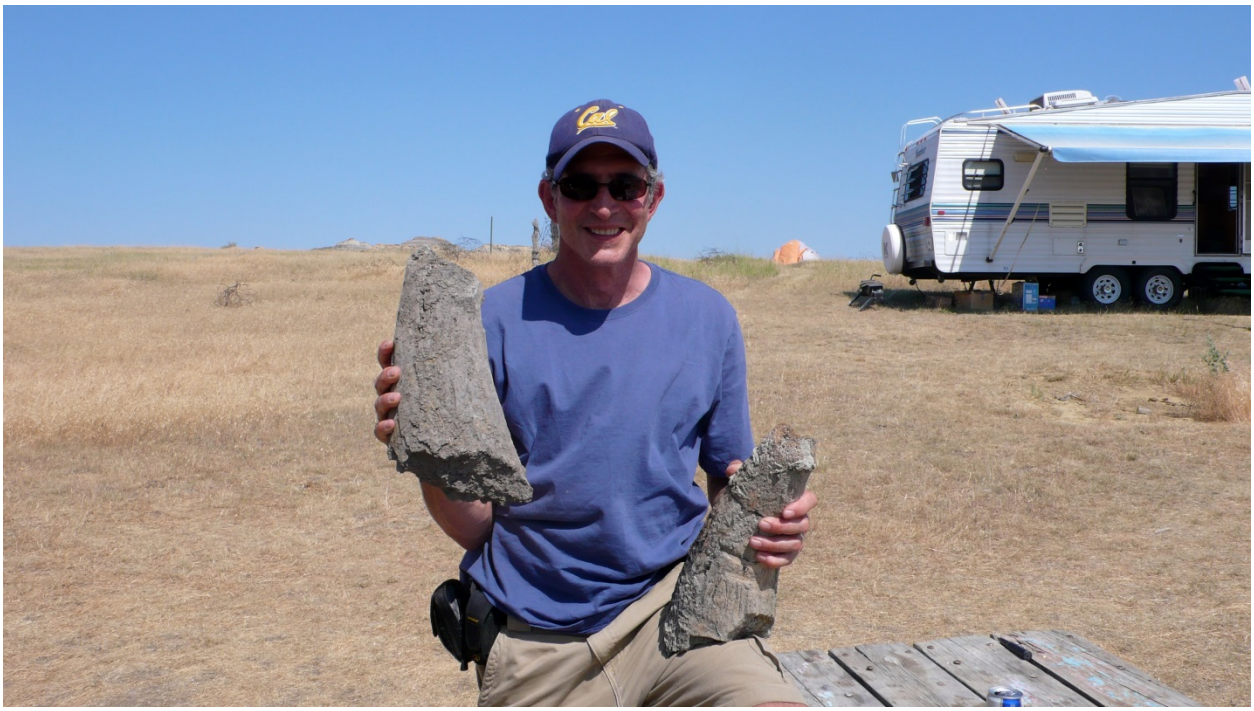
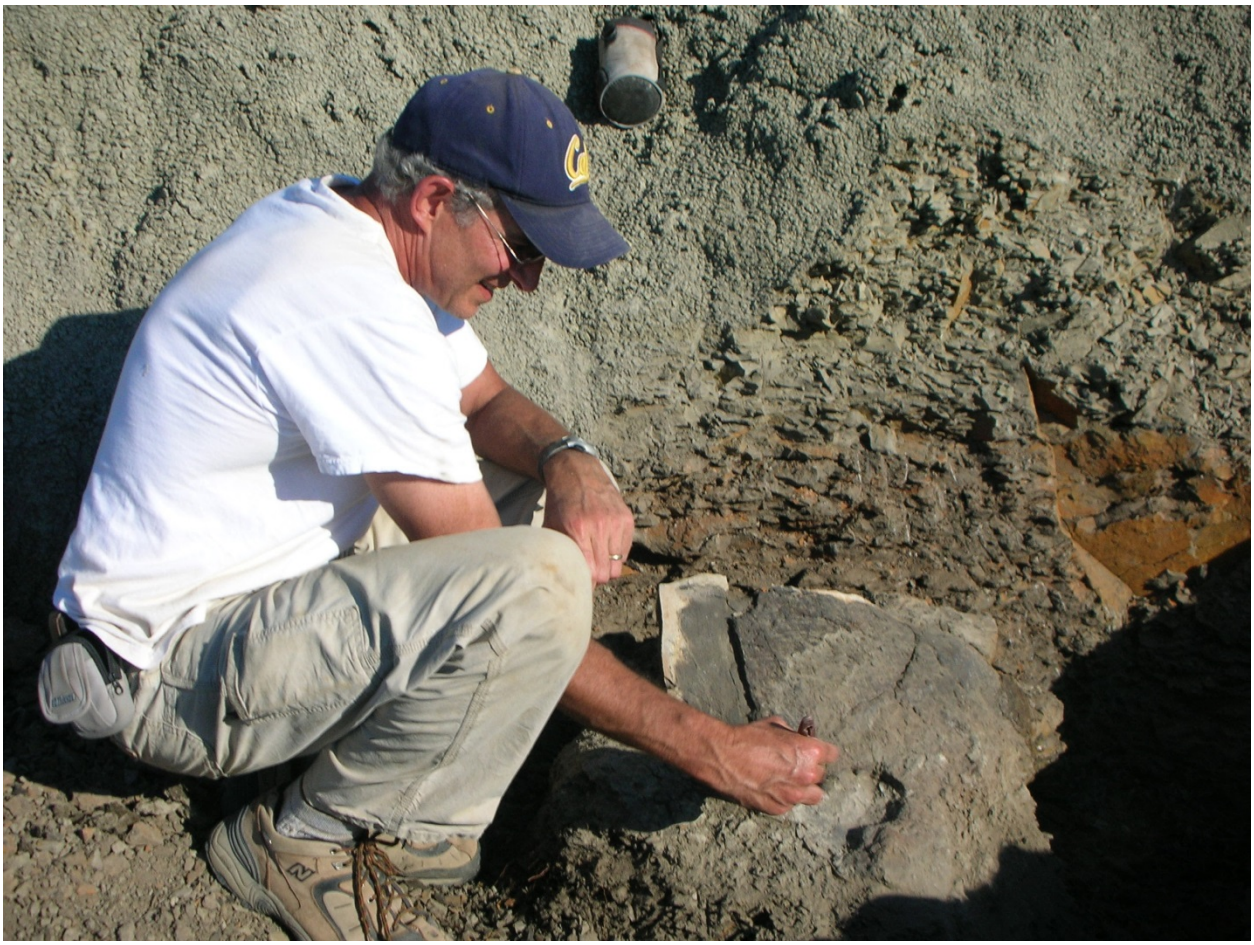
It is recommended that this oral history be cited as follows:

Mark Goodwin "Mark Goodwin: The Students of William A. Clemens and the Evolution of Paleontology at the UC Museum of Paleontology" conducted by Paul Burnett interviewer in 2015, Oral History Center of the Bancroft Library, The Bancroft Library, University of California, Berkeley, 2017.



Mark Goodwin, 2008



Mark Goodwin

Mark Goodwin is Assistant Director (Collections and Research) of the University of California Museum of Paleontology. He received his Master of Arts from UC Berkeley in 1987 and his PhD from UC Davis in 2008.

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Audio File 1

Interview #1 June 9, 2015
Begin Audio File 1 goodwin_mark_01_01-09-15_stereo.mp3

01-00:00:00
Burnett: This is Paul Burnett interviewing Dr. Mark Goodwin for the Bill Clemens UCMP Oral History Project and it's June 9, 2015 and we're here in the Bancroft Library. And this is audio file one. So welcome, Dr. Goodwin.

01-00:00:22
Goodwin: Thank you.

01-00:00:23
Burnett: So can you tell me a little bit about where you grew up.

01-00:00:27
Goodwin: Well, I grew up primarily in Central Massachusetts, in Worcester.

01-00:00:33
Burnett: In Worcester.

01-00:00:32
Goodwin: Yeah. Or if I lived there now it would be Worcester. [puts on Massachusetts accent]

01-00:00:35
Burnett: That's right. [laughter]

01-00:00:36
Goodwin: So that's where I grew up. And I was born in Maine, in Portland, moved to Worcester when I was about five years old. And so I'm a Central Mass kid.

01-00:00:45
Burnett: And how did you get interested in science generally?

01-00:00:50
Goodwin: I was always interested in science. I had a curiosity for the natural world, I guess, and dinosaurs in particular. But it stemmed from when my parents took me to the New York World's Fair, the '64 World's Fair in Flushing Meadows.

01-00:01:09
Burnett: Was there an exhibit, a paleontological—

01-00:01:12
Goodwin: There were reconstructed dinosaurs, life-size. I think the Rush Studios in New York made them. And there was a science museum in Worcester that had a reconstruction of a *Stegosaurus*. And I was just intrigued by paleontology and reading about Roy Chapman Andrews and those expeditions and I always wanted to see the world. I think that kind of drove me when I was little.

01-00:01:43
Burnett: Yeah. Many kids get interested in dinosaurs and it's become sort of a phase and, of course, now there's a lot of enthusiasm around it. It's in the general

popular culture. What about your upbringing or your temperament drew you towards that?

01-00:01:59
Goodwin: Well, that's a good question. I'll back-up a half-a-step. I know I always liked museums. Dinosaurs are found in museums. And since I was little, I think maybe I saw on the cover of *Life* magazine something about the Smithsonian Institution, so I always wanted to go to the Smithsonian Institution, wherever and whatever that was, because of all the things in their monstrous attic to see.

01-00:02:27
Burnett: What is it about the museums that you like? What's the romance of museums?

01-00:02:33
Goodwin: I always feel comfortable in those walls, I guess, the history and just sort of being able to participate. And more importantly, the people that I met in the museums I worked in up to and, of course, including the UC Museum of Paleontology, were instrumental in forging my career. No question about it.

01-00:03:01
Burnett: It's not just the place and the secrets in the attic and the collections but it's the culture.

01-00:03:08
Goodwin: It was the people. The culture. Yeah. And I think anyone familiar with natural history museums knows that. I was never the type of person who wanted to wear a tie and jacket to work every day. I loved being outdoors.

01-00:03:23
Burnett: Okay, yeah. That's a common theme for the folks—

01-00:03:25
Goodwin: Yeah. So I think taking geology classes as an undergraduate, taking zoology classes, meeting professors who were paleontologists.

01-00:03:40
Burnett: Well, you had a bit of a slightly different trajectory from a number of the folks that are being interviewed in this project.

01-00:03:47
Goodwin: Probably from all of them. Yes, yeah, yeah.

01-00:03:49
Burnett: Yeah. So did you start taking classes in geology first, before you did museum work, or did you—

01-00:04:00
Goodwin: Kind of simultaneously. I graduated from high school in 1973 and I went to the University of Massachusetts, a large state university, huge school. I don't know, 45,000 students. Margery Coombs, who was a vertebrate paleontologist who retired a few years ago, and her husband Walter, who was a dinosaur

paleontologist, Margery was on the faculty at UMass in the zoology department and I took my freshman year or sophomore year—probably my freshman year I took comparative vertebrate anatomy with Margery and, of course, not knowing at the time who she was and her pedigree, if you will, her academic pedigree. She graduated Columbia and AMNH and her husband Walter worked on Ankylosaurs. He was a professor at, I think, Springfield College at the time. And they both had offices at the Pratt Museum at Amherst College. So I worked for them doing fossil preparation.

01-00:04:57
Burnett: So you got into the technical side of—

01-00:05:00
Goodwin: Yeah, the hands-on side.

01-00:05:02
Burnett: The hands-on side.

01-00:05:04
Goodwin: Early on. And I was good at it and I liked it. I always made models as a kid. And then I wasn't a very good student or I wasn't very focused, I guess, is probably a—but I sure liked working and being independent. And then there was a program at the University of Massachusetts called Washington Center for Learning Alternatives. It still exists. This was in its infancy. So they had student internships in Washington, DC. So I thought, "Wow, this would be a good opportunity to finally go to the Smithsonian Institution." But ninety plus percent of the internships were for Capitol Hill, public interest groups, and I wasn't interested in political science and those internships. But I talked to the head of the program and they basically said, "Well, if you can get an internship at the Smithsonian you can be in the program. Certainly qualified." And they had an apartment building where they put up their students from across the country near DuPont Circle. This was in 1976, the bicentennial year.

So I applied. I called up the Natural History Museum (National Museum of Natural History) and talked to someone in the paleobiology department. They said, "Well, we don't have internships for undergraduates." But they put me through to Fred Collier, who at the time was the chair of the department. They had post-docs. It's the traditional route. But I got Fred on the phone and he said, "Well, so you're telling me you can come down here, get college credit, and have a stipend and work for free here?" And I said, "Yes." And he said, "Well, come on down." So that's how it started. So I was there my spring semester, in 1976, of my sophomore year.

And I ended up getting a paid position over that summer as a museum aide in the Natural History Museum. I loved it. With a group of similar aged students. We did field trips on weekends, collected fossils, worked in the museum. And I had an opportunity to work with many different paleontologists. Erle Kauffman, Bob Emry, Frank Kellogg, Arnie Lewis in the prep lab, who was

very welcoming, and that's where I felt most at home. So I spent a lot of time there.

And then I went back to school that fall semester in 1976 and then left UMass for good, moved to Boston, enrolled in Boston University as a geology major and worked at the Museum of Comparative Zoology with Farish Jenkins and Bill Amaral, and Chuck Schaff. Bill was in the prep lab, Chuck was the collections manager, and Farish, of course, was a very esteemed faculty member and curator of vertebrate paleontology.

Arnie Lewis, who I worked with at the Smithsonian, was previously Al Romer's preparator for twenty-two years. Alfred Romer is a major figure, a major figure in vertebrate paleontology. And he unfortunately passed away in the early 1970s and Arnie left Harvard and took the job of heading up the prep lab at the Smithsonian. So all of a sudden I was seeing that—vertebrate paleontology, at least in those days, was a relatively small community and when I went back to UMass, school was okay but I really missed working and I moved to Cambridge, Mass, worked, like I said, at the Museum of Comparative Zoology, enrolled at Boston University part-time, and finished my undergraduate degree.

01-00:09:09
Burnett: So you got your BA in geology from Boston University?

01-00:09:16
Goodwin: Well, I didn't quite finish. I was close. I was like one semester short when a preparator job, fossil preparator position opened at the UC Museum of Paleontology. I had previously applied for a couple of prep jobs, one at the AMNH in New York. They made an offer but I really wasn't interested at that time in moving to Manhattan. Plus, I wanted to finish school and there was no guarantee about getting into Columbia at that time to finish my undergrad and it was expensive. And then I applied for a job at Michigan. Didn't get that one. So I always give Phil Gingerich a hard time, even today. Because, I said, it's a small community and you end up seeing people for the rest of your career.

So a job opened up at Berkeley. At that time there was a Department of Paleontology and the U.C. Museum of Paleontology (UCMP), obviously. The director of the museum was also chair of the department and that person was Bill Berry. So I applied. I got an interview. And I don't know if Bill liked me because I had sort of a Boston accent and Bill was born and raised in New England. But the other thing is he went to high school with Fred Collier, who was my boss at the Smithsonian. They went to competing high schools. They both ran track apparently. But they knew each other since they were in high school. Listen, I had never been to California before. I flew out here. Some friends of roommates that I was living with in Cambridge, they took me out to Point Reyes. I was sold.

01-00:11:06
Burnett: You fell in love?

01-00:11:07
Goodwin: With the Bay Area. Yeah, who wouldn't? I mean, I was a kid from Massachusetts who hadn't really traveled very much.

01-00:11:12
Burnett: Yeah, yeah. And it was probably a different—it was both more vibrant and also quieter at that time in some ways, right?

01-00:11:21
Goodwin: Yeah, it was quieter. More vibrant. People made fun of me because they said I talked funny. Growing up, New England's so provincial. We didn't have an accent.

01-00:11:33
Burnett: Right, right. The rest of the world does. [laughter]

01-00:11:35
Goodwin: Yeah. And I would get envelopes addressed to "Mahk" Goodwin, not Mark Goodwin. But I've lost it over the years. So I was here for a week and they offered me the job before I left.

01-00:11:52
Burnett: Right. And so did you have an eye to finishing up the BA there? Like you were just going to enroll at UCB?

01-00:12:01
Goodwin: Yeah. As part of my package I said I would accept the position if I could transfer to the paleontology department and finish my undergraduate degree, which I did. Yeah.

01-00:12:11
Burnett: So what was your experience? Maybe we could talk a little bit about museum preparatorship because I guess you're the only voice in this project from the technical side. You mentioned you loved model building. Can you talk about what the work is and what you liked about the work?

01-00:12:33
Goodwin: Yeah. I love working with my hands. You're the first person to uncover this fossil that was probably collected in the field. You get a chance hands-on to see it, work closely with a museum, faculty member, academic paleontologist, research paleontologist who's working on that particular fossil. So for a twenty-three year old kid, I was in heaven. I had a whole prep lab to manage and great fossils to work on, world-class collection. The only academic Department of Paleontology in the country and I was part of it. So I was happy as a clam. I landed in a good place. And California's very welcoming, Bay Area, California. It's just different than the East Coast, obviously.

To be a fossil preparator, you don't go to school for that. It's all sort of—

01-00:13:28
Burnett: Artisanal, yeah.

01-00:13:29
Goodwin: —hands-on training, mentorship or apprenticeships, if you will. My experience and others, you can either do it or you can't. Students want to learn how to prepare fossils and it's like—

01-00:13:40
Burnett: So people wash out of it? They're just like, "It's not for me."

01-00:13:45
Goodwin: Yeah. And then they've got a reputation. Don't let that person near a fossil. Don't let that person in the collections. It involves chemistry, it involves metal working. It involves molding and casting, artistic skills, knowing anatomy. So it actually gave me a very good foundation for later going back to graduate school in paleontology.

01-00:14:12
Burnett: Well, you worked for a while. So you started in 1978 as the principle museum preparator and you hold that position until 1988. In that interim you graduate with a BA in '82. And then there's this five-year period, until '87 when you get the MA in paleo. You're obviously working full-time while you're doing this.

01-00:14:39
Goodwin: Right. I've always worked full-time. Yes.

01-00:14:41
Burnett: And so you enroll in the graduate program. When did you start the graduate program for the MA?

01-00:14:48
Goodwin: Informally probably in the early eighties because Bill Clemens, who I worked with both in the museum and in Montana, in the field, I was his field assistant. And preparators are often good cooks. I would do the cooking in the field and the shopping and manage the crew and handle logistics. That was my role in the museum. We have enough tents, we have vehicles, where are we going to set up? And I was Bill's right-hand man, if you will.

Bill was interested in another field area in Montana, in the Judith River Formation. Bill had a project in the latest Cretaceous Hell Creek formation. And Jack Horner and his best friend at the time, his best friend and field colleague, if you will, Bob Makela, they were—Jack was always interested in finding baby dinosaurs. Bob was a high school teacher in Rudyard, Montana, north central Montana along the Canadian border. We were working in Garfield County in eastern Montana. So Bob Makela took a window screen to

a fossil locality, to a microvertebrate locality, sifted some dirt, found a whole bunch of Mesozoic mammal teeth, which are Bill Clemens—

01-00:16:09
Burnett: Area, yeah.

01-00:16:10
Goodwin: —area of expertise. And at that time, maybe two or three, four teeth had been found, partial teeth, in the whole Judith River Formation of the entire state of Montana. Well, here Bob goes to one locality and finds like a dozen or so. Puts the dirt in a brown lunch bag, mails it to Bill Clemens at Berkeley. Says, "Hey, I found this site in the Judith River Formation. I know you work in the Jordan area. Why don't you come up and take a look?" So we did. And I went with Bill, I accompanied Bill. The following summer Bill sent me out there, probably 1980, '81, and I met Jack Horner. Got to know him well back then when he was still at Princeton. And, of course, my interest in dinosaurs, I would do whatever Bill wanted me to do and collect lots of matrix and haul dirt and wash it and collect microvertebrates. At the same time I would prospect for dinosaurs. And Jack would come and visit and help me identify what we had. And I knew how to collect big bones, small bones. I was trained by Chuck Schaff and the Harvard crew. So Bill gave me a lot of responsibility early on.

I have to be honest, back then in the day, graduate students, I probably put them on a pedestal. I didn't necessarily have the confidence you need to go to graduate school back in the day. But that slowly changed. And then discussions with Bill, I thought, "I could study the dinosaurs from this formation and I could do that as a master's if Bill would chair my committee." And he agreed to. So Bill, Howard Hutchison, who was a museum scientist, PhD museum scientist at the time, and Garniss Curtis from the geochronology lab, in the geology department at Berkeley.

01-00:17:57
Burnett: Wow. Wow.

01-00:17:58
Goodwin: So they were on my master's committee. And the rocks had never been dated. Terrestrial Judith River formation never been dated. And they do this by identifying volcanic ashes that contain glass crystals, biotite-sanidine from volcanic eruptions. And these ash falls settle out. So I sampled and that was my first published paper, first argon dates from the Judith River Formation.

01-00:18:23
Burnett: Right. That's the potassium-argon dating that they do?

01-00:18:25
Goodwin: Correct, yeah. Yeah.

01-00:18:26
Burnett: Okay. And that's what Garniss Curtis is famous for.

01-00:18:28
Goodwin: Yes, yes. So I got to know Garniss and work up at the Berkeley Geochronology Center. So people are always very welcoming, very nice.

01-00:18:39
Burnett: And you're in your twenties at this point and this is a bit dizzying for you to sort of be around some of these people.

01-00:18:45
Goodwin: Yeah. And I was curious. And I think in science, I think in our field—and even when I have students, undergrads that are curious and want to do more, we're happy to give them that opportunity. That's why we're here. This is a teaching institution. And even though I was a staff person back then I was given a lot of responsibility and decided later on, as my kids were a little older, to go back to graduate school and get my PhD and then completing that and moving over to the academic side within the museum.

01-00:19:20
Burnett: Right, right. Curiosity as a leveler?

01-00:19:21
Goodwin: I think so. I think so. You still have to have the chops to be successful.

01-00:19:28
Burnett: Absolutely.

01-00:19:28
Goodwin: But that's a driving force, I think, for all of us. And you realize you're just never going to know everything and it's okay. It takes a while to get comfortable with that because you're surrounded by the brightest graduate students in the country and you can see why one would feel a little insecure until you get that training, that union card, if you will. So I did it slowly, in incremental steps. Like I recommend to my own children, I wouldn't take nine years, and they didn't, to get their undergraduate degree. Maybe don't start your PhD at forty-five. But you can take a little time off. That's okay.

01-00:20:09
Burnett: Right, right, right. But you also talked about you were comfortable in museums and you're fascinated by working as a preparator and having your own kind of lab to manage. But you're also drawn to the field. And the field is kind of a leveler, too, I imagine. Others have talked about going out into the field. You're all living together out in the open. There's something about that that maybe—

01-00:20:43
Goodwin: Yeah, you're camping out there. Shared camaraderie. There are some paleontologists, some vertebrate paleontologists that do not do fieldwork or do

limited fieldwork. They work in collections and that's okay. There are a lot of fossils in collections, a lot of projects just sitting in collections. But at Berkeley, and particularly with Bill Clemens, who was very active doing fieldwork every summer in Montana. And I was thinking of it today, in fact, on the way into work because I'm going to Montana on the 20th, in a few weeks. It's my thirty-seventh summer going to Montana. Almost everyone in some way, shape, or form related to a project with Bill or going with him or rendezvousing with him. We even worked at a project that I later developed in Ethiopia with colleagues, C.B. Wood from Providence College and Chuck Schaff at the time. And Bill joined us there, as did Howard Hutchison, a museum paleontologist. So Berkeley's a unique place for that because of the type of work we do and the type of people that we have and had.

01-00:21:56
Burnett: Yeah, yeah. It's at that time, I think, in the seventies into the eighties when it becomes possible—I'm suggesting this as a question mark—when it becomes possible to be purely or solely a laboratory paleontologist. Is that right? Or did you always have to go in the field to be considered a paleontologist? That ends at a certain point. When does that end?

01-00:22:25
Goodwin: I don't know if it ends. I think it's part and parcel. You get employed as a museum scientist or a curator or a collections manager or a faculty member, you're going to be supervising preparators or students doing fossil preparation or you're doing your own fossil preparation. It's a skillset that you need, just like having a geology background is a skillset, or coursework that you need to be, I think, a successful paleontologist. In our field you need quantification skills, statistics. You need more tools in your toolbox than just being a comparative vertebrate morphologist, for example.

01-00:23:14
Burnett: Sure. Sure.

01-00:23:15
Goodwin: Yeah, I'm comfortable in the prep lab. Most paleontologists are. I'm comfortable working with collections. That's what I love to do. I like to do research. And now I can mentor students. I can be on PhD students committees or graduate student committees. I was a little late to the game but it was the pace and it was just a natural pace for me that was most comfortable.

01-00:23:41
Burnett: Yeah, yeah. But it sounds like at UCMP, in particular, and perhaps with Bill in particular, there's more emphasis on getting out to the field, at least in terms of your formation—

01-00:23:53
Goodwin: Oh, sure.

01-00:23:53
Burnett: —if not in your ultimate career.

01-00:23:56
Goodwin: Sure.

01-00:23:56
Burnett: And at other institutions it's not necessarily the case.

01-00:23:58
Goodwin: Yeah, it could be less so. Could be mixed. But yeah, if you look at Bill's students, which you are, many of them have active field programs of their own. But others are active in museum world, exhibits. Others are successful writers. Most have faculty positions in established programs at major universities and colleges across the country.

01-00:24:26
Burnett: Right, right. So in the late eighties, in 1987, you do your master's in paleontology at UCB and it's on that Judith River Formation, on the dinosaur fossils that you found and studied. And you move from becoming principle museum preparator to senior museum scientist. Is there a change in the type of work you're doing or is it more of a professional advancement?

01-00:25:04
Goodwin: No, no. I guess it's professional advancement. I was working as a senior museum scientist for a few years, at least. It's just a title. My responsibilities did change in the museum. I was supervising more people. I was actively involved in programmatic decisions. I was still running the labs but I wanted to do more. Everyone wants to make a little more money. I had a family.

01-00:25:37
Burnett: Sure. Of course. Yeah. Can you talk about your family a little bit? Do you have children?

01-00:25:42
Goodwin: I have three children. Ross is my oldest, Beth is in the middle, and Graham is the youngest. So Ross is twenty-eight, Beth is twenty-four, Graham is twenty-three.

01-00:25:58
Burnett: So the family was growing during this period, as well?

01-00:25:59
Goodwin: Yeah, the family was growing. I have to say, I was thinking about this even on the way in. I grew up in the UC Museum of Paleontology. I grew up there both literally and figuratively. I came when I was twenty-three, I was single. I thought I knew everything. I didn't know anything. Everybody in the museum was invited to my wedding. I got married in Tilden Park, in the Brazil Room. Everyone in the museum, in the department of paleontology, because we still had a department at that time, chipped in, bought me an Old Town canoe and

that was sitting in the foyer of the Brazil room. It was filled with presents. So everyone was invited. It was my family because my family was on the East Coast. Of course, it's not like that today. When I first came I was the same age as many of the graduate students, so they were both friends and now colleagues. So that's changed, that just sort of perspective. But UCMP has been my home since I was twenty-three years old. And I was thinking I've worked for six directors now. And Bill Berry was very influential, who hired me. Bill Clemens was one of my early mentors in the museum and in the Department of Paleontology and accepted me as a graduate student and gave me the confidence to publish with the understanding that I could do research. And the museum gave me that platform to grow both intellectually and professionally. So I'm very grateful. I don't know if you could do that today.

01-00:27:39
Burnett: Yeah. Well, it sounds like paleontology as a discipline has gone through some transformations. It's expanded greatly, the numbers of people doing it.

01-00:27:49
Goodwin: Right. And the field is more integrative and it's moved to the Department of Integrative Biology, reflects that, and our most successful students reflect that and their training reflects that.

01-00:28:02
Burnett: I wanted to ask you about that because it's at that time, during the time that you were senior museum scientist, that they're getting ready to move over. I think it's 95 that they opened Valley Life Sciences Building.

01-00:28:13
Goodwin: Right, right. Correct.

01-00:28:15
Burnett: So can you talk a little bit, because you would have been deeply involved in the movement of collections and that kind of thing, I imagine.

01-00:28:23
Goodwin: We had just begun our fieldwork in Ethiopia, a project in the Mesozoic. So I came back from Ethiopia, I had some students pack up the prep lab, and actually I moved over to Valley Life Sciences Building before everyone else because I thought of—when I had seen the building plans, it was this three-story atrium. I think about that same time Jurassic Park came out. So the original plan was to mount three Triassic skeletons at the base of this atrium in the Valley Life Sciences Building. But with the movie and the discovery of the Wankel T-Rex in eastern Montana in the early nineties, MOR 555, Museum of the Rockies 555, it's now known as "our nation's T-Rex". Last summer it was moved to the Smithsonian Institution. I was working in Montana at the time it was excavated. We had graduate students helping excavate it. I knew Jack Horner very well and Pat Leiggi, the administrative director of the Museum of the Rockies.

And I thought it would be a great idea to have a T-Rex in the middle of the building. So I talked to our director at the time, Jere Lipps, into starting an Own-A-Piece-of-the-Rex campaign as a fundraiser to purchase casts. And Jere and I went to Bozeman and went out to dinner with their director at the time, Art Fox, and Pat Leiggi, and met with Jack and they agreed to sell us a cast of this T-Rex. It was a museum in Japan actually that wanted a copy for display and paid to have it prepared rapidly. So I knew casts would be available. So we were able to purchase it—excuse me—at a discount. And I had never mounted anything that big before. I think the biggest thing I had done was a mastodon for an exhibit at Blackhawk—

01-00:30:14
Burnett: That's pretty big.

01-00:30:14
Goodwin: Yeah, it was pretty big. Also mounted a Smilodon, a saber-toothed cat, it was pretty small. So we had a very successful Own-A-Piece-of-the-Rex campaign to raise funds to purchase the cast and figure out how to mount a forty-foot animal in a thirty-five foot space with a deadline that the chancellor was attending. So I moved into the Valley Life Sciences first, moved the prep lab, and then the casts were delivered in two giant wooden crates with, as they say, with no instructions.

01-00:30:52
Burnett: Yeah. So that challenged your preparator skills to the limit, I imagine?

01-00:30:59
Goodwin: It did, it did, it did. But I knew some people in the field who had been doing this, building exhibits professionally. I went up and visited Peter May outside of Toronto at the time, in Canada, and got very familiar with schedule eighty pipe, that weighs ten pounds a foot, three-and-a-half inch diameter pipe, welding techniques and the like. But that's what a preparator does. So I kind of reached back to those roots. And if I didn't know how to do something I asked someone who did. And that's, again, how these skills are passed down in fossil preparation. And it was standing this morning so I must have done a good job.

01-00:31:36
Burnett: That's right, that's right. Well, there's some physics and engineering involved definitely in mounting something that big and that heavy, because the casts are lighter, I imagine, than bone.

01-00:31:46
Goodwin: The casts are lighter than the armature and they're lighter than bone and it's an internal armature as opposed to an external armature, which means more craftsmanship involved if the steel is on the outside. Basically like making a sandwich of fairly lightweight casts. But still, some of the casts, like the pelvis, I needed a hoist in there. And I had the electricians come every morning to shut the smoke alarms off because we'd be grinding and welding and there

was all sorts of trouble in that building. But we did it. And at the eleventh hour someone donated funds to purchase the pterosaur cast that you see suspended from the light fixture. So I rented like one of these three-story hoists, a lift that you could drive into the building. And at the top drilled three holes in the light fixture and ran the cables. My friend Mark Songey, who was great, I met him the beginning of the project. And he's one of these people that can do anything. Couldn't have done the project without Mark's support and his help by my side the whole time. But he couldn't look at me mounting the pterosaur, the cables to the light fixture, because the top of the lift was swinging like this, back and forth. And I didn't really notice it when I was up there. So it was a great time. We had a lot of fun.

01-00:33:09
Burnett: Well, that's emblematic of the magnitude of the move, I imagine. Getting everything over, getting everyone organized to do so. And it signaled also, as you said, this integration of paleontology into the kind of larger field of integrative biology, which was a new transition for UC Berkeley. But it was also, in some ways, late in coming because other universities had begun to fold paleontology into the life sciences a little bit. Not institutionally but in terms of the questions being asked in paleontology. You've got the—

01-00:33:49
Goodwin: Good point. Right. The questions being asked needed more integrative kind of skillsets, use of stable isotype geochemistry, comparative vertebrate morphology, anatomy, looking at the extant record to test evolutionary relationships. So you had paleontologists who worked primarily with modern forms. Maybe they didn't collect fossils but they would do gene sequencing. So yeah. PCR came on the scene at that time when we moved into the building. So we had students doing molecular paleontology, looking at ancient DNA and the preservation of biomolecules in deep time. And that's something I got interested in my own research on fossil preservation. What's a fossil? How do you get from A to B? How do you get from unaltered bone to B, this permineralized or not, minerally enriched or not, organic rich or not bone. People say, "Well, that's not a bone. That's a fossil." No, it's still a bone. It's calcium phosphate. It's just been altered to a degree. And to what degree and what does that mean?

01-00:35:03
Burnett: Right. The significance of that is key. And there's so much interesting work, yours and those of others, that has been done in that area.

01-00:35:12
Goodwin: Right. So when I went back to school, graduate school for my PhD—one of the reasons why I went to UC Davis, because people didn't know me there. It was humbling enough just going back to graduate school at forty-five. My first test I bombed. Had to take two years of coursework. But I did it with an emphasis on stable isotope geochemistry. Worked with paleo-oceanographers. Howie Spero was on my committee. My chair was actually an invertebrate

paleontologist, Sandy Carlson, who also did early work as a master's student, undergrad, on tooth enamel and preservation. Richard Cowen, a very broad paleobiologist.

01-00:35:51
Burnett: That was the place to go if you were going to do that kind of research. Can you talk again about what that specialization was for your dissertation?

01-00:36:01
Goodwin: Well, I looked at the effect of the burial environment on the chemistry of fossils primarily. I looked at the elemental signatures. So I did particle induced x-ray emission, pixe probe of dinosaur bones. Very limited work and analysis had been done previously. There was a lot of work in the 1960s, backscatter electron microscopy, where they could see a fossil was enriched in iron and manganese. But I wanted to know how much and where. What's the microarchitecture of the bone? Is that effecting the distribution of these authigenic minerals, that uptake, because there's ionic substitutions that occur in life, in vivo, and in death in the burial environment. And if we're using the chemistry of fossils to test hypothesis of diet, niche partitioning, physiology, paleoecology, paleobiology, then that better be a paleobiological signal that you're measuring and not a signal from the burial environment. And if it is a signal from the burial environment, maybe that can still tell us something. So sometimes you never know. The path you're taking diverges in mysterious ways. So it was great going back as a PhD student.

01-00:37:26
Burnett: So prior to that people were beginning to look at what this paleobiological signal is, as you put it. And your research and your concern was how do we reduce the noise or eliminate the noise, the chemical noise from the fossils so that you can pinpoint what actually the organism was eating or what it's ecological niche was.

01-00:37:56
Goodwin: Well, at least you can hopefully partition out, if you're doing a whole fauna. But even taking a step back, just looking at the chemical and elemental signature in the fabric, the texture, if you will, of bioapatite, which is a mineral. And other people had been asking similar questions. And prior to that, stable isotope geochemists would come to our Society of Vertebrate Paleontology meetings and give talks. And to me, just conceptually it was a black box. Where did they get that data from? They got it from the tooth enamel? They ground some of the tooth enamel? What's the chemistry behind it? So I had a basic chemistry background but when I went back to graduate school I had to sort of bear down again, drill down, if you will, on basic chemical principles and organic chemistry and geochemistry. Because bone is a mineral and its mineralogy, and how is it affected? So that was one or two chapters. And then asking those same questions on Paleogene mammals, Paleocene and Eocene assemblages and also looking to see if there's a difference between the same species buried in a non-marine terrestrial

environment and a marine environment. Same thing with dinosaurs and marine reptiles from the Mesozoic. Is there a chemical difference in those dinosaur bones buried with the marine reptiles in marine shales, and those same species of hadrosaurs found in non-marine continental deposits. There's even more work to be done. My work and Mary Schweitzer's pioneering work. Mary and I collaborate and I would posit that fossilization is a preservational process. So a lot of my more recent research is trying to answer those questions using the synchroton, the advanced light source, and microtomography and using CT scanning and micro-CT scanning.

01-00:40:06
Burnett: Can you talk about the ALS stuff?

01-00:40:09
Goodwin: Yeah.

01-00:40:09
Burnett: That was 2008 to 2010, is that right?

01-00:40:12
Goodwin: Well, it's ongoing but that's when I started it, I believe.

01-00:40:16
Burnett: You got beam time?

01-00:40:17
Goodwin: Beam time. Yeah. It's like Star Trek. Yeah.

01-00:40:19
Burnett: It is, it is. Yeah. I am fascinated by that stuff. You got beam time to study the soft tissues of demineralized bones.

01-00:40:30
Goodwin: That's correct. And Mary Schweitzer pioneered this work with her PhD dissertation. Inadvertently she left some bone in acid, in EDTA, and it dissolved and what was left behind was a three-dimensional soft pliable network that looked, for all intents and purposes, like blood vessels. So Mary is working diligently on this, and has graduate students working on this, and probably about eight or ten years ago she had a meeting in Raleigh, North Carolina. And I said, "Mary, this is great, great work. But you need to figure out what is it made of and how did it get to be preserved." So that's where I came in as a collaborator and started doing work up at the ALS on the beam line. And there are some really smart people up there and they like biological questions. And that facility is designed to accept proposals from a variety of disciplines and provide free beam time to scientists on campus and across the country, across the world really.

01-00:41:35
Burnett: Across the world. Absolutely.

01-00:41:36
Goodwin: Yeah. So there's so many resources at UC Berkeley and ALS is one of them. We have the best biosciences library one flight up from the Museum of Paleontology. When we get indignant, "What do you mean you can't find the proceedings from the 1871 meeting on fossil baleen held in Berlin? You don't have that?" "Sorry, Dr. Goodwin, but we'll find it for you." [laughter] They have great staff and they're always very nice. My colleagues will email me all the time. "Can you get me a PDF of this particular journal or series?" So I come to work every day, I'm a kid in a candy factory, candy store.

01-00:42:20
Burnett: And you're a mammal who survived across the McCone-Valley life sciences boundary.

01-00:42:27
Goodwin: Yeah, the boundary. Not without a few scars. Right. Hopefully wiser for it.

01-00:42:33
Burnett: So you were a witness to the kind of propulsive consequences of integrative biology, the development of integrative—

01-00:42:44
Goodwin: Absolutely, yeah.

01-00:42:45
Burnett: It was universally positive for paleontology?

01-00:42:49
Goodwin: It was somewhat controversial at the time because we were losing our identity as the Department of Paleontology. But we were a very small department. Nonetheless, we were losing that identity. How would the paleontology group, I mean the faculty members are still here but they're no longer faculty members that are in a department of paleontology, which was the only one in the country. My undergraduate, my bachelors and my masters degrees are in paleontology from UC Berkeley. But I think Bill Berry had a lot of foresight and was prescient, if you will, on looking at the field, both in outreach and education, and as well as just seeing how integrative the future will be, and it was moving. But I also had a perspective because when I came here in 1978, many of the faculty members and paleontologists from the fifties and sixties, the forties, and fifties and sixties were still at Berkeley and still alive. Sam Welles, Don Savage. It's just a unique opportunity to work with people like that and to get to know them as people, and as well as scientists. You need personalities. Wyatt Durham. And the staff at the time, as well. The Department of Paleontology produced a number of very prominent vertebrate paleontologists of which Bill was part of that cohort, when he was an undergraduate and a graduate student. Malcolm McKenna, Dave Webb, Mike Woodburn and Mike Novacek, and many others that went on to do very influential work in vertebrate paleontology. Berkeley was a center for biostratigraphy in West Coast paleontology and a center in the 1940s, 1950s

for micropaleontology. All the oil exploration and Zach Arnold's work here and Kleinpell and then paleobotany. Ralph Chaney accompanied Roy Chapman Andrews to the Gobi Desert and China and rediscovered the Dawn Redwood. He's a Berkeley paleobiologist. Come on.

01-00:45:10
Burnett: So there's this legacy. A tremendous legacy.

01-00:45:12
Goodwin: There's a legacy, founded by and endowed Annie Alexander, a woman who took classes with J.C. Merriam. And J.C. Merriam came here to study geology with Joseph LeConte, who was one of the first professors hired at Berkeley right after the Civil War. In fact, the *Oakland Tribune's* headline was "University hires Confederate sympathizer." And then they hired his brother Robert, who's an engineer. LeConte was a colleague and best friend of John Muir. We have fossils collected by John Muir in the museum. Charles Camp, who was a Berkeley vertebrate paleontologist. Bill took classes with him as an undergrad and graduate student. Ruben Stirton. These are major figures, associated with the Museum of Paleontology and the department of paleontology. And Charlie Camp. He was also a well-noted California historian, one of the founding members of the Clampers. Bill Huff, I just found out recently, he actually was not employed full-time by the museum. He was a patternmaker at Alameda but he was an artist in the museum. He did a number of commissions and projects and scientific illustrations and sculptures for the Pan-Pacific Exposition at Treasure Island, as well as artwork for scientists and Charles Camp and in the museum. So you have all these intersecting lines, California history, vertebrate paleontology, micropaleontology, invertebrate paleontology. We have the first and oldest collections, surviving collections from the earliest geological surveys in the 1850s up and down California that Josiah Whitney organized and half that collection went to the California Academy of Sciences, the other half went to the newly emerged chartered UC and paleontology department. And in the 1906 earthquake the collection at Cal Academy was lost, so we have the only surviving collection. And then when the legislature, in all their wisdom, disbanded the early geological survey of California, it was determined that about 200 type specimens, and half the remaining collection went to UC Berkeley.

01-00:47:38
Burnett: Well, it clearly has this tremendous legacy. But that transition seemed to be, and I'm speaking of the transition to the VLSB, to Integrative Biology, seemed to be essential given where the field as a whole was going. And it seems that that's, notwithstanding the tremendous legacy and identity of paleontology, paleontology seems to be thriving.

01-00:48:10
Goodwin: Absolutely. It's as strong as it's ever been, with multiple collaborative major externally funded projects, cutting edge research, global climate change,

understanding evolution. We have one of the most recognized websites for evolutionary biology, natural history, paleontology. One of the first museums to go online when there were literally fifty other sites on the web.

01-00:48:39
Burnett: Of any kind.

01-00:48:40
Goodwin: Of any kind. We were the second museum to have dinosaurs. But, again, that was UCMP Director Dave Lindberg's vision at the time. And we had undergraduates and an English lit major, I think, and other students who sort of pushed the museum in that direction, with a very supportive director who said, "Hey, it's okay if it's not finished. Let's just get it up online." Rob Guralnick, who was at Colorado for a long time did his graduate work here. But he wasn't a paleontology major as an undergrad. So Berkeley reflects almost California and Bay area fertile ground for innovative ideas and that filters down to even the academic, to the Department of Paleontology and the Museum of Paleontology. And this is ongoing. The other six member museums of the Berkeley Natural History museums today, we're a vibrant, innovative, influential community of scientists, curators, technical staff, programmers, and collections. And you need those collections to test ideas and hypotheses and try to answer questions about how the planet is changing, how it's changed in the past. What did these animals do? What are they doing today? How does it compare with more recent times in the end of the last Ice Age and beyond? So you have to look at deep time to answer these questions. You cannot do that without collections.

01-00:50:11
Burnett: Right, right. And Bill has said that paleontology has been interdisciplinary even before, quite a bit before.

01-00:50:21
Goodwin: He's right. He's right. We didn't come up with this idea. It has been interdisciplinary. We had stratigraphers and sedimentologists and functional anatomists.

01-00:50:34
Burnett: Geochronology and that's late fifties.

01-00:50:37
Goodwin: Yeah. So in the trenches it was very collaborative because, hey, listen, you have people, they're social for the most part, they like to go camping, drink beer, and talk about what they love. So how can that not be collaborative and cross-disciplinary?

01-00:50:55
Burnett: And a cross-pollination of questions, right?

01-00:50:57
Goodwin: Sure. Even when I first came here in 1978, Bill with Glenn Isaacs from anthropology taught an interdisciplinary class using our element collection. If you're looking at a shell mound and you find parts of bones, how are you going to identify those? Well, we have an element collection where the individual bones of modern skeletons are sorted and stored separately by element - unlike the Museum of Vertebrate Zoology where the skeletons are all kept together. Here, they're parted out, like car parts. So if you go, "I think that looks like a femur, it might be a tibia," you can just open the cabinet, see all the femora, and do the comparison. Our field trips, our annual spring field trips, we always had students from anthropology and other disciplines from campus, or someone who was just taking a class and wanted to go. And we're starting to revive that tradition with some of the newer faculty hires, like Seth Finnegan and Cindy Looy. And who's had a seat at the table the whole time? Bill Clemens, retired or not. I think he retired twice and they called him back. And he still has good ideas, he's a great listener. He asks incredibly probing questions. He'll be the first one to say, "Hey, did you see this paper in the *Anatomical Record* on the narwhal tusk? That it may be a neural sensor?" He takes an active interest in whatever you do. He's kind. The worst he'll ever say about someone is that maybe, "He's a twit at times." You know in Bill's book that's pretty bad. And back in the day you'd have to like knock on his door and take a number to talk to him when he was running his lab and had graduate students. In fact, for one April Fools, I think, or Christmas, they gave him a present. It was like at a deli counter where you take a number. They made up one of those to put outside his door. And we all knew his tricks. He would turn his light off and close his door when he was in his office. You could smell the pipe tobacco. So you knew he was in there but the lights were off. Or if his door was ajar a little bit you can knock and you'd knock on the door and he'd look at you over his glasses. Like, "Oh, you better have a good question."

01-00:53:15
Burnett: [laughter] Yeah, just to get himself some time to concentrate and work on his own stuff.

01-00:53:19
Goodwin: Yeah, he was a very busy man. And now he's two doors up from me. His door's always open. He comes in almost every day, 10:00 to 4:00. And there's a big comfortable chair to sit and talk with. It's sort of full circle. And he's my role model for when I retire. He's very productive.

01-00:53:38
Burnett: Yeah, yeah. A very active retirement.

01-00:53:40
Goodwin: Very active. And he has been. And he's been active his whole career and he's an excellent scientist. He's a great writer. He's really helped me as an editor in just looking at his work. Look, talk about Mesozoic mammal teeth? And keep

the attention of the reader? We found the first one from Ethiopia, and Bill was part of that crew. We wrote that paper up together and it was one tooth and it was a fragmentary mammal tooth at that. It was about eight-tenths of a millimeter in width. It was tiny. We had it CT scanned so we could blow it up, make a 3D model so we could actually look at the morphology. And he wrote the most eloquent and articulate paper on what it is *not*.

01-00:54:27
Burnett: [laughter] Right. Well, that's also part of his approach, right, is being very cautious, very precise. And the claims must be responsibly tentative and that's how you advance, right?

01-00:54:44
Goodwin: Or responsibly substantiated, as well. And not to disregard alternative hypotheses, but make your case. Make your point.

01-00:54:55
Burnett: Yeah, yeah, yeah. And epistemologically you've taken that as your approach, or would you characterize your own work as different?

01-00:55:03
Goodwin: I hope so. I think hopefully it's rubbed off. You can certainly hear voices of your mentors when you're writing a paper, when you're asking a question, or when you know it's BS. When you're arm waving. Even now at our VP lunch on Friday, or fossil coffee on Tuesday, Bill, he's listening and he'll ask a very perceptive question at the end. And never in a condescending way. He's a supporter of all of us. He's a big supporter of women in the geosciences, not just his daughter Diane, who is a geoscientist at Cal State Fullerton. His other daughter, Liz, is a professor at University of Chicago.

01-00:55:47
Burnett: Yeah, in sociology.

01-00:55:48
Goodwin: In sociology. But he's a big supporter of women in the geosciences. And as his daughter told me at the GSA meeting last spring, I was sitting down with Diane, she goes, "You know, I just learned something about my dad. That he's a big supporter of women in the geosciences. I always knew he was a big supporter of me," she said, "but I didn't know he supports all these other women." He always declines going to the acknowledgement receptions and the donor dinners. He won't do it. Doesn't do it. Doesn't need that.

01-00:56:22
Burnett: Right, right, right. But is silently supportive and helping and encouraging in real ways.

01-00:56:27
Goodwin: Yeah, in real ways. And if you look at the alumna from his lab there's very, very influential and successful scientists. Annalisa Berta and Anne Weil and Nancy Simmons. Marisol Montellano from Mexico City. And he was also

influential in some of the first students that came from China after the Cultural Revolution. Zhe-Xi Luo, who's now at the University of Chicago. So Bill's influence, it's at home and it's very broad. And he keeps in touch with all these people. And there's a staying power with them.

01-00:57:11
Burnett: So a model mentor and as one individual that has been really—a part of the message, too, from your own career is this deep attachment to the institution.

01-00:57:23
Goodwin: Yeah, the loyalty.

01-00:57:25
Burnett: And loyalty and helping it grow and helping it through transitions so that it becomes really fundamental.

01-00:57:31
Goodwin: Right. And being as positive as you can. And yeah, yeah. That's absolutely true. Yeah, yeah.

01-00:57:39
Burnett: Well, I want to thank you for taking the time.

01-00:57:43
Goodwin: Thank you, Paul. Yeah, it's been great.

[End of Interview]