Steve Glickman interview transcript

Irv Zucker: I'm Irv Zucker. I'm here to interview Steve Glickman. Steve and I have been colleagues and friends for 50 years, and I have been a witness to his accomplishments in that time. So, it is a pleasure for me today to revisit them for a broader audience.

I'm going to start by asking you where you were born and where you grew up, what your parents' occupations were and how the environment they created shaped you.

Steve Glickman: I was born in 1933 in the Bronx, New York and lived there for the first 10 years. Both my parents were teachers. My mother taught the piano at Juilliard and also later privately in our house, but that was much later. My father went to school every day, where he taught junior high school mathematics.

Because they were out of the house so much, there was a woman who took care of me during the day. We lived in the Bronx near the Bronx Zoo and this woman's brother was the keeper of the rhinoceros house at the Bronx Zoo. And we would go over there every day for lunch. She would go in and visit with her brother and I would be outside staring at this rhinoceros who I was sure was communicating with me. And that began a life of connecting with animals that has persisted to this day.

Zucker: Was there something in the environment that your parents created that had a lasting impact on you -- other than fortuitously giving you exposure to animals?

Glickman: My parents actually did not particularly like animals, so that was maybe a rebellious thing on my part. But what my parents did convey was a sense of the importance of teaching. And I certainly inherited that from both of them -- that teaching was an important and good activity.

Zucker: So, I guess the fact that you were exposed to the zoo explains why a city kid like you ended up spending so much of his time chasing after wild animals in Africa, and you and your students did so much field work. It's not the usual thing one associates with somebody with your background.

You first came to Berkeley as a Miller Fellow in 1962 under the mentorship of Frank Beach. What kind of empirical and scholarly work did you do during this fellowship? And how did it affect your subsequent research?

Glickman: I did two kinds of work on that fellowship. My experimental research project in Berkeley involved a group of striped skunks. I was able to move this set of skunks into the Animal Behavior Station in the Berkeley hills and study their social interactions and their threat patterns and things like that. They were de-scented, so it was safe to approach them and do those things. In fact, I had begun working with skunks when I was a child of nine in a children's camp in the Catskill Mountains. And in the summer of 1942, in the Camp Lakonda newspaper, they wrote: "If he leaves his group and doesn't come back, he's feeding his skunks at the nature shack."

And so, the first thing I did on the Miller fellowship was arrange for this group of skunks to become my friends. And Bernie Schiff, who you know very well, came out there as my graduate student and helped me with the project. And then, at the end of the project, in August, 1964, we released all the skunks in Tilden Park --- where their descendants are probably still ---

Zucker: Spraying animals....

Glickman: Well, yes, and having a good time. I consider Tilden Park to be a good habitat for skunks.

Zucker: OK, good. Did this two-year interval in Berkeley somehow stimulate you to want to come back later on as a faculty member?

Glickman: It certainly did. Now, I forgot to mention the other primary activity during those years. And that was writing up a set of papers on the research I'd been doing at the Lincoln Park Zoo in Chicago, where I had been studying curiosity in zoo animals. I began working there in 1959, when a man named Marlin Perkins was the Director. I had a grant from the National Science Foundation and I used to hire the curators who worked for Marlin to help me. At night, after the zoo was closed to the public, we would test the animals and throw novel objects into their cages. After watching us for about six months Marlin gave us the keys to the zoo and every animal's cage. I don't know of any researcher who has ever had that kind of access in a zoological park.

But when I arrived in Berkeley in 1962 I had a lot of data to write up. And a good chunk of my Miller fellowship was spent either researching with skunks or writing papers about data we'd collected at the Lincoln Park Zoo.

Zucker: Can you say a little bit about how you study curiosity? You just threw the same object into the cages of how many different species?

Glickman: It's probably a hundred different species. I tested every animal at the zoo, where I could retrieve the objects after six minutes and try new objects. I was looking for a general response to novel objects, not just specific curiosity about an individual object,

So, we used five different objects that had diverse sensory qualities (steel chains, rubber tubing wooden dowels / blocks and a piece of crumpled paper). They were roughly scaled to the handling capacity of the individual animals -- where they could pick them up in their mouth and bat them with their paws. Or if you were a primate, pick it up in your paws and see why the object was interesting. So, I tested virtually everything there; I tested both mammals and reptiles. I didn't formally test birds, but I did some informal observations on birds. They're not in any of the papers.

Zucker: And what would be one main generalization that you concluded from this that investigation?

Glickman: That animal curiosity -- in terms of investigating novel objects -- is very much tied to the dietary habits of animals in nature. If you had a very narrow diet (where the food was easily available for consumption; you were an animal that just ate grass, which didn't require any manipulation, and all you ate was grass), you wouldn't be a very curious animal in terms of novel-object tests. But if you had a varied diet that required a lot of manipulatory behavior, you would be intensely curious and you would do all kinds of interesting things with the objects. And you'd also tend to have a bigger brain. And that was beautifully illustrated by the behavior of baboons and other very fancy primates.

Zucker: Very Interesting. You taught at Northwestern and at Michigan before coming to Cal as a faculty member in 1968. How were you affected by the tumultuous conditions on campus for the next few years? And were you challenged in any particular way by students?

Glickman: I remember that the first couple of years I was on the Cal campus there were helicopters overhead at Sproul Plaza. And that was entertaining. But to be honest, none of that stuff affected either my research or my teaching, and the teaching just went along.

Zucker: I was less fortunate, having been tear gassed twice on camp during that time.

You received the campus Distinguished Teaching Award. How would you describe your teaching style in large lecture classes and in smaller seminars?

Glickman: Well I never had a theory of teaching. When I watch the current recipients of these teaching awards get up and talk about their teaching, they've obviously thought about it in very creative, interesting and personal ways. I just got up and taught. And later on, people like you, and
my students in their evaluations, pointed out to me that what I was doing was telling stories. And when I was teaching history of psychology, I would make a point of telling stories about the backgrounds of the different famous psychologists that I was talking about. To get myself and the students involved in the lives and intellectual productions of these people and how their lives related to their intellectual productions.

When I taught History of Psychology, I didn't go back to the ancient Greeks. I started somewhere around the time that -- oh, about the beginning of the 19th century, when people were first beginning to understand the nervous system. And so, I talked about the lives of Galvani and Gall and Spurzheim --- leading up to people like William James and Sigmund Freud..

Zucker: I seem to recall that you were asked by a group of students to run a seminar on sickle-cell anemia. I didn't think that that was one of your areas of expertise. What was that all about?

Glickman: Well, sometime in the early 1980s a group of black students approached me and asked me to give them a seminar on sickle-cell anemia. I explained to them that I didn't know anything about sickle-cell anemia. And one of them said to me "Let me understand this: You can only teach things that you studied in graduate school and are totally familiar with?" So, I said "OK, look, I can learn about sickle-cell anemia and then I can give you a one-hour lecture, but

that's not what I could really teach you. And that's not a full course.

And I said, if you want to take a course with me, we can create something in which we will review, for example, the Jensen studies on heredity and IQ, and we can figure out what's wrong with them. And also, you could do research yourselves if you come up with some ideas. And they did and it was a wonderful seminar.

They had developed this hypothesis that residents who lived in low-rise projects like Acorn would be much more content in their residence if they knew their neighbors in low-density projects, as opposed to the people who lived in high-rise projects in San Francisco. And they developed a questionnaire and then they went out and got data on the relative satisfaction with their living conditions of people in low-rise and high-rise projects. And it was a beautiful study that was in many ways ahead of its time and it couldn't have been done by white faculty members. It required black students who could go into these projects and approach the people who were living there and get good honest answers. And I admired it so much. And that was perhaps the most memorable seminar I ever taught. I followed the careers of several of the students subsequently. One of them went on to get a Ph.D. at Stanford in psychology; another went on to law school and became a serious lawyer in Los Angeles. And it was a happy outcome in every way.

Zucker: You mentioned about the fact that you taught the history of psychology. I believe you started that course in the local department. Isn't that correct?

Glickman: I can't remember -- which is not an uncommon experience for me at this age.

Zucker: But it brings me to two related things -- which is, more generally, which courses engaged you and why do you think you emphasized historical antecedents in your courses more than other faculty did?

Glickman: When I came here as a faculty member in 1968, my assignment, at the undergraduate level was to teach Physiological Psychology, as we called it in those days. And I enjoyed that, but I enjoyed the next course I taught a bit more -- which was Animal Behavior. And we also had an interdisciplinary version of Animal Behavior, which was taught with colleagues in Zoology, and that was a nice course and I enjoyed that.

But then, when Lucy Jacobs joined our department and was ready to teach animal behavior, I said "Fine, Lucy you teach Animal Behavior and I'll teach History of Psychology," which had always been an interesting topic for me. It was just a gift to be able to learn all that stuff and talk about the people and recreate the history of psychology in my own style.

Zucker: I sat in on that course the very last time you taught it, and it was remarkable for the breadth of the coverage. But I was also amused by the fact -- I sat in the back of the room and in front of me were students on their laptops, some of them were playing video games. And you were writing on the board at a time when most instructors were using PowerPoint presentations. You have any thoughts about that?

Glickman: Well, I'm glad that I wasn't focused on those students that were playing video games.

Zucker: I wanted to yell at them.

Glickman: But, there were enough students who were paying attention and I would make eye contact with them. And several students came up to me and said, "When we saw you writing on the blackboard, we knew we wanted to be in this class." So, there are -- there were other rewarding parts of my antiquated mode of instruction. And then, as you know, at the end of the class -- the last class -- a group of students from the class baked a cake and presented it to me at the end of the class. And it was a chocolate cake with a hyena on it, and I'm supposed to be riding the hyena.

Zucker: Can you say a little bit about your committee assignments at the university? You were there for a very long period of time; you must have done some committee work.

Glickman: Yes. I was on the original Title IX committee and that was an interesting experience -- glad to see Title IX implemented in Berkeley. And then the other really interesting committee was the Budget Committee, where I had the pleasure of sitting and learning about all wonderful research that goes on at the University of California, Berkeley -- from colleagues in Classics and many Departments that I knew nothing about.

Zucker: Can you say something about the mandate of the Budget Committee?

Glickman: Well, on the Berkeley Campus, the Budget Committee reviews every pay raise, appointment to the faculty, and promotion, and makes recommendations to the administration. And I met people on there, in History and English and other areas, who are friends to this day.

Zucker: Wonderful. You also served five years as Chair of the Psychology department. And I'm wondering what your greatest challenge was during those five years and, with hindsight, your most satisfying achievement.

Glickman: Well I'm not sure how to answer that. The greatest challenge was to get a group of 30 or 40 faculty, each of whom had their own domain of research, to cluster around a common theme of psych -- to regard themselves as members of a unified department. And I tried various strategies to get that to happen, including inviting guest lecturers like Janet Taylor Spence, who was later President of the American Psychological Association. And I tried to have people like that in as lecturers—giving lectures that would attract the whole department. I also worked on the issue of getting more minority psychology taught in the department, with the help of people like Phil and Carolyn Cowan. And that's where we created a new seminar. We invited Robert Guthrie up to lecture. Robert had written a book called "Even the Rat Was White." And he came in and gave a wonderful lecture, and then other local people came in and lectured. And it was the first course with with diversity as a focus. And I felt good about that.

Zucker: Was it during your tenure as chair that women finally got to become regular faculty members?

Glickman: Well they'd been appointed when I got in. And at least three of them received tenure while I was Chair of the Department, and I prepared their cases. Now the credit for that goes to the women, not to me. They were the ones who did the work. But it was a good feeling. And I remember two of them got tenure on the same day. And the provost stopped me on campus, told me about their promotion to tenure, and congratulated me. We talked about congratulating them, and that was a very rewarding part of being Chair of the Department.

Zucker: And you were also the Director of the Animal Behavior Research Station for something like 14 years. You succeeded the founding director if I recall correctly. Was that a fun job?

Glickman: Not as much fun. I think that we did have some nice projects there while I was the Director. The idea in that Animal Behavior Station, as created by Frank Beach, was to provide semi-natural habitats for animals so that they could have a life that was richer than that experienced by most animals at the University, who lived in laboratory cages. And it would be a semi-natural environment for many of them and it was fun doing that. Lovely work was done with birds and birdsong and primate social behavior -- following that arrangement -- and as you know eventually I was to introduce hyenas to the Station.

Zucker: We'll get to that shortly.

Glickman: OK.

Zucker: You have had a longstanding interest in the 19th-century biologist Alfred Russell Wallace, who is co-formulator of the theory of evolution through natural selection. Why Wallace rather than Darwin?

Glickman: Because when I read about Wallace I loved how this poor self-educated boy in England had taught himself biology and made himself into a very serious biologist. First he worked as a surveyor for his brother, who was in the building trades in England in the 1830s and '40s. And in the late '40s, on his own, Wallace just takes off for South America with the goal of understanding the transmutation of species. He spends four years there studying animals. Then, he comes back and a few years later he goes off to the Malay Archipelago and spends eight years in the Malay Archipelago. And while he's in the Malay Archipelago, in 1855, he gets the idea for the mechanism accounting for the transmutation (or evolution) of species, and the mechanism is natural selection. Afterwards, Darwin and Wallace had a major disagreement and over the evolution of the human brain.

Wallace lived with the local people thoughout his travels. He wasn't rich; he didn't live surrounded by Englishmen. He lived with the people in the Amazon of South America and the Islands of Indonesia and believed that their brains supported intellectual activities and moral acts that were equal to those of Europeans. Wallace' view was expressed in the last pages of his book on The Malay Archipelago, when he wrote about the high moral standards of the people that he lived with, and how each of them respects the rights of his neighbors. And Wallace contrasted that with the situation in England, where you have to have all these lawyers and law courts, and he writes about the percentage of the English population that were paupers, or in trouble with the

law and so forth. And so, he thought that these people were every bit the moral superiors of the average Englishman, unlike Darwin. He also thought that they were remarkably smart, and he made a dictionary of their languages while he was out in these places. And he thought that they were as smart as people in England and it didn't make sense to him that this could have occurred through natural selection. Because he thought that their daily life did not require the kind of things that they were capable of doing if they'd been sitting there in England. And Wallace was wrong about that. Life was pretty challenging out there and they knew a lot more than he understood. But he decided that the human brain had been created by an unnatural force -- a supernatural force -- and he began attending seances and decided that there were forces beyond those we're seeing on Earth and that we all would have eternal life. And he went on from there.

Glickman: But you were going to ask me, I think, about the trip that Krista and I did.

Zucker: That's a good place to go.

Glickman: In 1997 Krista and I traveled around Wallace's Indonesia and out to some fairly remote (for us) places, like the island of Ternate, where Wallace lived while composing his paper on natural selection. Wallace mailed the paper to Darwin because Darwin was the only biologist he knew to be working on the problem. And it was fascinating to go around to these places and see what could still be seen, and a lot of what Wallace viewed could still be seen. And that's what we were looking at on Ambon and Sulawesi.

Zucker: You're most identified as the leader of an unusual long-term project investigating the biology of the spotted hyena, primarily in a colony that you founded in the Berkeley hills above campus, adjacent to Tilden Park. Can you say something about how you got involved in this enterprise and what is unusual about this species that drew you to it?

Glickman: Well, in the fall of 1982, two old friends -- Lawrence Frank, who was a graduate of the Ph.D. program in zoology and had been working with hyena's in Kenya, and Julian Davidson, who I'd been a postdoc with in Berkeley and was now an endocrinologist at Stanford, invited me to lunch at Shattuck Avenue Spats. They had a proposal: they had been working on some peculiarities of hyenas in the field, but had reached some limits -- I'll have to tell you what those peculiarities of hyenas were. And they thought hyenas needed to be brought into captivity.

And so, because I was Director of The Animal Behavior Station, they thought maybe I could arrange it. They also knew that I'd spent some time working with strange animals, and so they invited Krista and me to Africa to see hyenas in nature at Lawrence's field site. And I came home and I told Krista that I been invited to do this. But, I was just completing my five-year term as Department Chair. I was worried that my research career was in a shambles and I needed to really get things going, and this work with hyenas was so crazy that it was never really going to happen. And Krista said to me "Let me understand this: Lawrence Frank has invited us to Africa to live in a tent on the banks of the Talek River, surrounded by elephants and zebras and giraffes and hyenas, and you don't know whether we should go? Why are we married?"

And I capitulated and in January of 1983, Krista and I went to Africa, saw all the things that Lawrence wanted us to see. And I came back and spent the rest of the spring writing a grant,

which was submitted in July of 1983 and ultimately funded in July of 1984. And I thought it was pretty astounding that we got funded -- given that we were proposing to bring over squads of baby hyenas from Africa, transport them 9,000 miles to Berkeley, California, have them grow up in captivity and behave like hyenas in Africa, and reproduce. We hadn't shown that we could do any of those things, and one could have reasonably been very skeptical. But for some astounding reason a grant committee gave us the money and then it worked! Partly thanks to the incredible arrangements that Lawrence Frank made in Africa, coordinating with the local Masai, who were essential to collecting hyenas. Now, the Masai culture is very much involved with cattle and sheep, and hyenas were their enemies. And they were poisoning hyenas in the area where we collected them. So, the Masai were happy to help us and they said "How come you only taking 20? Take them all!" But we only took 20 and brought over ten each year over two years and we designed a facility in Berkeley that can house them -- house them there, and they grew up and reproduced. And why they're interesting now has to be talked about.

Zucker: OK, good idea.

Glickman: OK, so hyenas are social carnivores that live in clans where they have matrilines like many Old World primates. And they're very active hunters. By hunting in groups, they can bring down prey as large as buffalo and zebra. And they're not just scavengers -- although they'll certainly scavenge. But, in one part of the Serengeti, Hans Kruuk reported that 60 percent of the lion food was originally killed by hyenas. And then, the lions drove the hyenas off their kill and the lions scavenged.

In addition, within these hyena clans every adult female totally dominates every adult immigrant male. The females are much more aggressive than the males and the males are ordinarily terrified of the females. Now as long as they stay in the clan of their birth they have their mother's rank and they're dominant to all the immigrant males. But if they're going to reproduce, they're going to have to leave the clan of their birth, go elsewhere to mate, and join a new clan. And that's a tough process and takes a lot of years before they rise in the male hierarchy. So, it's a fascinating social system and we were going to study all their behavior in captivity and how it developed.

But then there's another characteristic of hyenas that had attracted attention since antiquity and had been written about by Aristotle. Female spotted hyenas -- not brown hyenas or striped hyenas -- have external genitalia that, to the casual observer, are not distinguishable from those of the male. They have a scrotum like a male, instead of an external vaginal opening. The clitoris has developed until it looks just like a penis. They get erections like males, and they use those erections when they say “hello” to one another. In the hyena meeting ceremony, the subordinate animal gets an erection and comes up and offers it for inspection by the dominant animal. And we were going to study that in captivity too. Another point: although there is no external vaginal opening, the clitoris is traversed by a central urogenital canal. And female spotted hyenas mate, and ultimately give birth, through the tip of this large clitoris to a two-to-three-pound baby. So, the clitoris expands and ultimately babies emerge from the tip of the clitoris.

The whole arrangement seems so unreal. And it still is amazing to me. Also, it's a costly arrangement. A large number of the first births are stillborn, because the fetus gets stuck in the

clitoris and can't get out. The umbilical cord detaches, and if you don't come out in a timely manner, you die of asphyxiation.

The whole system seems so strange and Lawrence and Julian had begun studying the mechanism that produced these “masculinized” genitalia in female hyenas at Laurence’ study site in Kenya. To this day, the only theory of why male mammals have male external genitalia and female mammals have female external genitalia involves the early action of male hormones, or androgens, at critical stages in development *in utero*. So, if the current theory of how you make male genitalia was true, these females had to have high levels of androgen circulating at this critical stage of development, unlike all other female mammals. Spotted hyenas are the only mammals on Earth that have this arrangement. And Julian and Lawrence had been looking for androgens in nature and found out there are limitations to what they could do out there.

So, at Shattuck Avenue Spats in the Fall of 1982, they approached me about bringing some to Berkeley in order to really figure out how this system worked. And the Berkeley Hyena Project began. But then, something happened that was going to change the nature of the project. And what happened was that one of my colleagues, Julian Davidson, developed early Alzheimer's disease and had to close his laboratory. But before Julian did that, he arranged for a faculty member at the University of California San Francisco -- who was an endocrinologist in the Department of Obstetrics and Gynecology -- to join our project. And his name was Pentti “Finn” Siiteri. And that changed the whole nature of the project, because “Finn” -- who had worked on problems of sexual differentiation in animals and people, said: "You know, Steve, we need a real anatomist to look at the anatomical details of the clitoris and the penis." And he recruited Jerry Cunha, who was a Professor of Anatomy at UCSF. And then Jerry began working with us and he said "Steve, there are human medical conditions where little girls are born with male genitalia and that is studied in the Department of Urology." And so, Jerry recruited Larry Baskin, who's the Chief of Pediatric Urology at the medical school. And then “Finn” also recruited Professor Jean Wilson (University of Texas Southwest Medical Center), who is one of the fathers of the field of sexual differentiation. And Jean recruited Dr. Michael McPhaul, also on the faculty of the University of Texas Southwest Medical School, who is a molecular endocrinologist, and Mike studied the molecular structure of the hyena androgen receptor.

And now, a bunch of things now began to happen. First my wonderful colleague at Berkeley (Paul Licht). I needed an endocrinologist in Berkeley. I'd entered this field of sexual differentiation at the age of 50 with the level of knowledge that one would have in an Introductory Textbook. But talking to all these people was beginning to educate me and I learned that gee, all endocrinologists aren't alike. And so, I began recruiting other endocrinologists -- first Paul Licht at Berkeley to replace Julian Davidson as the primary endocrinologist on the project. And then people like Al Conley at UC Davis, who is an expert in the enzymatic transformation of one hormone into another hormone -- androgens to estrogens and so forth and so on. And gradually the network began to build up: Dr. Bernard Steinetz (NYU Medical School) joined the Project., Dr. Steinetz was an expert in relaxin, a hormone that relaxes female tissues to promote the birth process in humans and other mammals. He had read about the hyenas in a story in The New York Times, and he wrote to me and initiated the study of relaxin in hyenas.

So little by little we built up this big network of collaborators on the Berkeley Hyena Project. And that included people you know --- Geert De Vries and Nancy Forger --- two of our friends who came in as the primary nervous-system people to examine the hyena nervous system and ultimately published things on sexually dimorphic systems in the brain and spinal cord.

Zucker: So, would you say it's fair to characterize you as the conductor of an orchestra made up of many, many people from different disciplines, most of them not in Berkeley, some of them in other countries? And you managed this for more than 20 years.

Glickman: Yeah. All the hyenas were here for 30 years and I managed to keep funding going for almost 30 years --- although that was always a challenge.

Zucker: So, were some people in the medical establishment interested in what you were finding and did they come and visit?

Glickman: They did more than visit. For example, Larry Baskin was very interested in the maintenance of sensitivity in the human clitoris, and had been one of the first scientists to map the distribution of nerves in the human clitoris. And those nerves are on the dorsal surface right off the midline. But, some clitoral surgeries involve women who've been exposed to high levels of androgen in utero and have a clitoris that is the size of a penis. And so, they have to go in there -- if these people are going to have a normal socio-sexual life --- and do clitoral surgery. And while they're doing it, Larry wanted to make sure they preserve the sensitivity of the clitoris. And so, Larry initiated a study with hyenas where I gave him some hyena genitalia and Larry initiated studies of the peripheral nerves in the hyena clitoris as an animal model of the human hypertrophied clitoris. And indeed, the nerves were in the same location, pretty much, in the hyena; they just go further down toward the end of that "naturally hypertrophied" clitoris. So that is now an animal model that is referred to in the human medical literature.

And another case: there was a little girl born at the medical school with male genitalia and they'd been following her mother's hormone levels during pregnancy. And an awful lot of androgen was in there that shouldn't have been there. Ordinarily, in pregnant women there is a high concentration of the aromatase enzyme in the placenta, and that enzyme converts any androgens to estrogens so they won't "masculinize" the developing fetus. But in this woman there'd been a mutation and her aromatase enzyme wasn't working properly, so she developed very high levels of androgen that affected the developing fetus during pregnancy. We had previously found that, in the hyena, one of the main sources of androgens at critical stages of sexual differentiation is the placenta. There are all those enzymes in there making active androgens. And what had happened in this woman was that her placenta was behaving like a hyena placenta and those androgens were "masculinizing" her daughter's clitoris and, in the paper that my colleagues at UCSF published about this case, they refer to the hyena and our work on the hyena placenta, by way of understanding what happened in that woman.

So, you had basic science proceeding along its track. But because of all these collaborators with different expertise, a network developed which translated basic science into useful medical discoveries.

Zucker: Neat. So, I want to switch gears now and ask you, when did you formally retire from teaching and committee work?

Glickman: I retired at the age of 75 in 2008.

Zucker: OK. And did the hyena project continue postretirement?

Glickman: Yes. The last hyena left Berkeley in December 2014.

Zucker: So, six years after you formally retired.

Glickman: Yeah and I should say something else about that, because -- I'm still writing papers about hyenas. We might have been able to keep the project going beyond 2014, but a likely way to go would have involved euthanizing the hyenas and studying their nervous systems. And they were all our friends and we couldn't do that. So, we decided to shut the project down.

Zucker: What did you do with the hyenas?

Glickman: Excuse me?

Zucker: What happened to the hyenas?

Glickman: And all the hyenas went to good homes in zoos and animal refuges. We were actually the single largest supplier of hyenas to zoos around the world. If you go to Paris you could see spotted hyenas at the Paris zoo that were.....

Zucker: Are you saying that you can't order a hyena from Amazon?

Glickman: Not yet. Next year. But, I should mention the woman who managed the hyena project from its inception and interacted with this network of collaborators, arranged all the research and everything and ultimately arranged to give all the hyenas away. And that's Mary Weldele. None of this would have happened without Mary's oversight. Also, there were a succession of remarkable postdoctoral fellows who worked on the project in Berkeley, each of whom brought unique skills to bear on our research questions and interacted with scientists at other institutions. These included Dr. Christine Drea, Dr. Susan Jenks, Dr. Ned Place, Dr. Cynthia Zabel and Dr. Elizabeth Coscia.

Zucker: So, I have more questions I want to pose to you. The first one is could you have foreseen your scientific trajectory when you completed your graduate work.

Glickman: Not at all. When I left graduate school at McGill, I was trained in in the study of brain / behavior relationships in lab rats. But, while teaching at Northwestern University, and living near the Lincoln Park Zoo, I began to go around to the zoo for fun. And one day I just knocked on Marlin Perkins door. I knew I really wanted to get in there and play with these baby snow leopards and baby tigers. So, I knocked on Marlin Perkins' door and I got in to talk to him and I proposed studying color vision in big cats.

We won't go into the reasons for that. But I never did it. Instead once I got in there I began studying all these other things. I did study color vision in brown lemurs at one point. But the zoo was just a playground for me.

Zucker: OK. And the last thing I'm gonna ask you is: If you were going to do it all over again, what would you do differently and what wouldn't you change? ?

Glickman: I'm not sure that I'd do anything differently. It was all -- I feel I've led a privileged life. Sometimes it just amazes me that the people of California paid my salary all those years when I was just going over to the university and doing things that I loved to do -- whether it was standing up in front of a class and telling them about the history of psychology, or pursuing research on unusual animals.

One of the nicest things that I would get in student evaluations -- one of the things that warmed my heart the most -- was when students would write how they expected the history of psychology to be really boring and then, because I'd managed to engage them -- largely with stories about the lives of these psychologists -- they found they enjoyed it. And I just think I've had a privileged existence.

Zucker: Having had the privilege of teaching with you for 10 or 15 years was a remarkable experience -- to learn from these stories that were so much more accessible than a professor getting up and pontificating in technical kinds of things. And the remarkable thing was that at the end of the story you managed to communicate all the important scientific points. As one of our senior colleagues who sat in on your course came in to say to me "He's up there talking to 300 students, telling stories. And at the end of it, he's covered all the important points!" And then he paused and he said, "I can't do that." And he left. So, it's been a privilege. Thank you.

Glickman: And thank you, Irv. It was wonderful teaching seminars with you. Observing you was crucial to my being able to write grants that would actually attract some money -- which was required to support those expensive hyenas. And you were the most successful grant getter I knew at Berkeley. I could never write grants that were as tight as yours. But you did change me from writing "I wonder what would happen if?" grants to writing grants that had well-defined questions. And I learned that all from you. None of that stuff would have happened if we hadn't been friends. And I feel very lucky about that as well.

Zucker: Thank you. I know you didn't give me any of the money that you got.

Glickman: No, I was cheap.

Zucker: OK.