Legacy: Al Riley transcript

Steve Glickman: Hi, I'm Steve Glickman.

Al Riley: And I'm Al Riley.

SG: And we're friends

AR: and colleagues.

SG: My first question for you, Al, is when and where were you born?

AR: I was born in Oak Park, Illinois in 1922.

SG: And by my calculation that means you're 97 years old; you had a birthday in June.

AR: That is correct.

SG: And I find that very interesting.

Narration: The following video interview is part of the UC Berkeley Emeriti Association's Legacy Project, which preserves the recollections and reflections of Berkeley's emeriti. In conversation with a colleague of their choosing, emeriti are invited to discuss their academic careers – including contributions, accomplishments and challenges, especially as they relate to campus history. This recording is intended to provide a personal record of value to the family, friends, and colleagues of emeriti and to document the history of the Berkeley campus as it pertains to the individual's department, school, and college. In this video, you'll see Steve Glickman interview Al Riley. Both are emeritus professors in the Psychology Department.

SG: Al, would you like to tell us some things about your early life in Illinois?

AR: I had a wonderful childhood and it was very, very free; I did pretty much what I wanted to do. I think my father had given up on discipline with my older brother, and I was allowed to do just about whatever I wanted to do. When I was about 12, we moved to another suburb, Glen Ellyn, where I went to high school. While I was in high school, I took a course in sociology, and the teacher in that course had majored in psychology I think, probably at the University of Illinois, and talked about psychology. I had this feeling, "This is what I want to do."

So, I went through Glenbard High School and then looked around for a place to go to college. This is really quite improper, I'm sure, for this sort of thing but I had thought that the best thing I could do would be to go to a boys' school where I didn't have to be distracted. So, I went to Wabash College down in Crawfordsville, Indiana and stayed there for two years. But, I discovered that their facilities and opportunities in psychology were very limited so I concluded that I was going to leave there, and I did. I went over to Denison University in Ohio, which had a very good, small, four- or five-person psychology department, and I thrived. And I also met Annie McDonnell, whom I married subsequently and spent 50 years with her.

So that was very, very good. And at the end of my second year, World War II broke out, and the Japanese bombed Pearl Harbor. I and many of my classmates went over to Columbus and enlisted in the Navy -- in a program that allowed you to finish college and take some Navy courses while doing that. So, I finished college and ended in the spring of '43. And then went to midshipmen's

school at Notre Dame -- a famous place for midshipmen to go -- and spent four months at Notre Dame and was commissioned in the Naval Reserve and immediately shipped off to the South Pacific. While I was there (I was out there two years), I was on a vessel called a gunboat. Our job was to either support landings in intimate enemy territory or to patrol, which we did a lot of, around enemy-held islands to stop inter-island or intra-island shipping. Risky.

After I'd been there something over a year, we went over to New Guinea, which is which is west of the Solomon's, where I had been, and entered into a large group that was preparing to invade the Philippines. You may recall (probably not) that when Gen. Douglas MacArthur was driven off of the Philippines he said "I shall return." This was his return, and we were an intimate part of it. Several months later, we were part of a group that supported the invasion of Luzon, the main Philippine island, north of Leyte.

While we were doing that we were hit by a kamikaze and it hurt our vessel considerably and we had some people killed. It's worth pointing out, I think, that the guy who was the skipper of our ship -- my captain (I was the executive officer)— was a year older than I. After we were hit and our dead and wounded were taken care of, we continued with our mission even though the front part of the vessel had been destroyed, and continued with the invasion and provided invasion support. Then later, we returned to New Guinea and were repaired on a little island called "Mios Woendi" in Dutch, which is "very windy". And the main thing I can remember is that we'd see a movie every night and then there'd be a cocktail hour, either before or after, and we'd drink too much. And the song that still rings in my ears was "Rum and Coca-Cola." It was played all the time practically; it must have just come out with the Andrews Sisters. [music]

We had some other further expeditions. And then, some months later, I was returned to the United States and spent the last year at the Naval Training Station at Great Lakes, Illinois. And then Annie and I got married and we had a cute little house out in the country and it was the way it's supposed to be.

SG: Al, one of the conversations that we've had involved a visit you made home on leave; you were home on leave when the atomic bomb was dropped.

AR: That is correct.

SG: On Japan

AR: Yes

SG: And that was very consequential for you, because you thought you were going to have to go back and support the actual invasion of the Japanese mainland.

AR: That's correct.

SG: So,

AR: That was all laid out.

SG: So, give me your thoughts when the bomb was dropped.

AR: Well, I was in downtown in Chicago on an elevated platform, waiting for a train to go north up to visit my fiancé. I heard these guys shouting from down below -- newspaper guys – and I went

and listened and they said that the war was over, in effect. I can remember the enormous sense of relief I had. I knew that I wasn't going to have to go back – that it was all over. I didn't have to go back and I spent the next year -- before I got out of the Navy as a so-called "civil readjustment officer" up at Great Lakes and what we did was to discharge Navy personnel.

SG: And now you're released from the Navy.

AR: Yep.

SG: And you're ready to go to graduate school and you chose the University of Colorado. And how did that come about?

AR: Very interesting question. Well, when I got out of the Navy I didn't know what I wanted to do. I got a job as a management trainee at an outfit called Bauer & Black, which was a big bandage and cotton-producing company -- had headquarters someplace in the South. I was going to become a manager there, and I found it incredibly boring. So, Annie and I packed up one weekend and went back to Denison, and spent a long weekend with our pal Lynde Steckle, who was the mentor for both of us as undergraduates. Someplace along the line he said "Well, if you want to think about going to graduate school, I can probably help." He called up his pal Larry O'Kelly at Colorado and said "Could you take this guy on?" And Larry O'Kelly talked to his chairman -- guy named Karl Muenzinger, who said "Yeah, send him out." It's incredible -- it's the way things happened then; it was word of mouth. Steckle said, "Al is a good person; trust him." And in Colorado they said "Yeah, we'll take a chance on that, if you say so."

So, Annie and I went to Colorado; we spent one year there and it was a very consequential year for me. When I look back on it, as I have many times, the really important thing was not my relationship with Larry O'Kelly, with whom I got a master's degree, but with Howard Kendler, who was a young assistant professor who had just come out of the Army himself. He was an Army clinical psychologist. He was really an experimental psychologist; he was Kenneth Spence's protege. He was on fire and he was a brilliant young man. I took a seminar that ran two quarters with him – it was all of the entire history and ins and outs of conditioning and learning --Pavlovian conditioning. It was a wonderful course, and changed the way I thought about things. I think one of the things that it did was to make it clear to me that what I really wanted to do was experimental research. It was clear to me at that time that the future lay in doing carefully controlled experiments that could disprove hypotheses.

So, I looked around and both Larry O'Kelly, my mentor there, and Steckle, our mentor, were both from Ohio State. And so I thought "Well that's a possibility." And I applied to Ohio State and Michigan and Illinois -- places that I knew. Ohio State came back first and offered me a nice assistantship, so I took it. I knew who was there, and it was a very strong group in experimental psychology. And so I went to Ohio State and spent three years there and got a PhD with Art Melton.

SG: What kind of research did you do at Ohio State?

AR: I did both human and animal research. I did research on conditioning with animals and I did research on human memory with Art. I must have talked to Art and said "What am I going to do? What happens next?" He said "Well, leave it to me," or something like that. I had job offers from Johns Hopkins and Berkeley. I went to Johns Hopkins and spent three or four days there; that was quite wonderful. But it was clear to me that the system at Hopkins was that there was a professor and two or three associate professors and six or seven assistant professors. If you were successful, you would get a job someplace else after you'd been there for seven or eight years. And if you went

to Berkeley, everyone had a chance to get tenure. And I said, “This is silly. Why would I go to Hopkins when I can go to Berkeley?”

Annie and I, my first wife and I, first came to Berkeley in 1950, and shortly after we came here with a couple of moves we moved up to the top of the hill on Grizzly Peak Boulevard and spent 50 years together up there. She was a major force in my life and in my career, and in determining what I did and the kinds of research I did.

After we had lived together for several years, we adopted two children with a couple of years apart -- first Carol and then later her brother, Tom. They were major additions to our life and have remained so. Now, Annie died in 1995 after we had lived on Grizzly Peak for 50 years I guess. Six years later, I had met and married Carolyn. We continued to live there and she also had a major influence on my life and my career and continues to have that influence today. Both marriages were enormously happy and successful.

SG: So when you arrived here, in what was it, like in 1950, and had Tolman and the loyalty oath figured in your calculations?

AR: Indeed they did. I knew about it only from reading about it in the newspaper and knew nothing about it. I wrote a letter to the Chairman, C.W. Brown, and said “Should I be worried about this?” He wrote back almost immediately and said “Don't worry about it. Come on out.” This was in the late spring of 1950.

SG: Al, say something about what the loyalty oath, because it was more than just the loyalty oath. It was also a disclaimer affidavit -- as I recall.

AR: At the time, as I understand it, when people came to any institution that was part of the University of California, they had to sign what was, in effect, a statement that I am a loyal U.S. citizen. But the loyalty oath that was imposed was special in that it said, “And I am not a communist.” A whole bunch of people, like Edward, believed that they had signed the oath, saying that they were loyal citizens of the United States and they didn't have to have a special test. And so they said “no”. Edward Tolman was the leader of the non-signers on the Berkeley faculty and I think he was a remarkable, brave, wonderful man

SG: and a wonderful psychologist.

AR: Absolutely. And he certainly had a profound impact on the rest of my life because I became the kind of animal cognitive psychologist as he was. Virtually the entire staff did not resign because of the loyalty oath. There were three or four people in psychology that did. Edward and I think Nevitt Sanford and Hugh Coffey and maybe someone else, I can't recall. And everyone else stayed on, but they contributed money out of their salary every month - the entire faculty did, I think - to support the people who were out, who had decided that they could not stay. What happened was that it went to the Supreme Court and the Supreme Court said you can't impose a special oath on a selected group of individuals in the state and threw it out. And so Edward was reinstated, as was Hugh Coffey and Nevitt Sanford and someone else who I can't recall.

I can remember when Tolman came back, one of the first things he did was to have a seminar and talk about what he was thinking about and what he'd been thinking about. And all the troops came. It was a small seminar room, but it just jammed with Krech and Leo Postman and Mark Rosenzweig and Benbow Ritchie and a whole bunch of students. It was absolutely marvelous because you got to see what Tolman was like.

SG: You've had a long and distinguished research career and from the beginning you're interested in the factors that control learning and memory in animals and people, with animal research predominating in the later stages of your career. But early on at Berkeley, you were still working with both animals and people and worrying about the factors that control learning and memory.

AR: That's true. Memory loss and how it happens. That's right. And it was certainly very important to me. At the time that I got through with graduate school, there were two really major lines of intellectual activity in the field of animal learning and cognition. One of them was Tolman and his students and people like him, who were essentially descendants of Gestalt psychology and thought that they could infer from animal behavior the kinds of mental processes that were going on in the animal's brain. Things like, "where is the goal?" and, "how do I get there?" Mental processes, not reflexes.

The other school of thought descended from Pavlov, who said we think we can account for all of these things by the way reflexes, or "conditioned responses" as we came to call them, hooked together. And that was where I came from, in the Middle West, the so-called Hull-Spence position was the dominant position. Out here on the West Coast, it was more Tolmanian. Fortunately, I had one mentor at Ohio State, Delos Wickens, who was very open minded and had, I think, no strong convictions one way or the other. He was interested in what the data told him, and that influenced me greatly.

So, I had two big influences when I came here in animal cognition-- one of them was how Howard Kendler, who set my pants on fire, and the other was Wickens, who was a moderating influence. I had this seminar and talked about some of the problems in conditioning and learning. And one of the things I talked about was the problem of transposition. Transposition is essentially - was invented as an experimental procedure by Köehler, the famous Gestalt psychologist. And what Köehler had done was to take two animals -- either chickens or apes or something else -- and train them with two stimuli that differed in, say, either brightness or size. A quantitative dimension. And one of them would be positive and when the animal went to that, he'd always get a reward. And the other is negative. And when he would go to that he would never get a reward. And then after they had been trained, he moved it over like this, so that the original positive was the smaller or the dimmer one and there was a new one. And the animals almost invariably went to the new one. They avoided the original positive and went to the new one.

SG: That was larger?

AR: Larger or brighter, whatever the dimension was. Or dimmer; I mean it could go in either direction.

SG: But they were responding, it looked like, to the relationships rather than the precise quality of the stimuli.

AR: Precisely, and that was Köehler's point, that they're not responding to the Pavlovian stimulus, positive stimulus, as a point, but rather that they were responding to the relationship between the two. Kenneth Spence, who was, I think, not the most lovable person in the world but a very bright guy and who was in the Pavlovian tradition and came from Yale, devised a scheme where he could explain transposition without responding to relationships.

I described this in the class and we talked about it and somehow or other (I cannot tell you how), someone -- either I or one of the students, I have no idea and couldn't reconstruct it several years

later --said “Well, isn't it possible that the reason that Spence shows this declining transposition, which he said was consistent with his model, occurred because they didn't change the background, they only changed the positive and negative stimuli?” And if that's true and you change everything, keep all the relationships constant, it turns out that animals respond relationally way, way out. There is no drop off. It's exactly as Köehler would have said. We did that in my lab and it changed my life in some respects, because it was the first piece of research I did where there was a sharp, clear answer to something that was new. And it was so much fun to do that I continued doing, persisting and doing, more and more animal research kind of in this vein.

SG: But you had colleagues at Berkeley who are also engaged in human research and you were rather close with some of those colleagues and published with them.

AR: Indeed, I was very close to Leo Postman -- who in some respects I think was my main supporter here at Berkeley. We became very good friends and did a lot of research together and did some things that were a lot of fun. And, we did a huge piece of research on memory and forgetting.

SG: Human memory?

AR: Yes, human memory. Leo was strictly a human guy. But, we did other things. We published a paper -- I can't recall the name of it, but it was something like “A Disproof of Köehler’s Theory of Association,” and it was great fun to do. And it infuriated Krech. He was very upset that we little people would pick on this great man.

SG: One of the things I noticed was that in your publication list, there are some very interesting and varied experiments with John McKee involving children and also with Krech and Rosenzweig involving echolocation in rats.

AR: That's right.

SG: So, would you like to tell us a little bit about it?

AR: Well, I don't know whose idea it was -- possibly Krech’s -- but we had this thought. We sat and talked about this for quite some time, as to how animals, what they do, when they get to the goal in a maze and whether, in fact, they have any way of signaling what's been going on. Whether they tell themselves or tell the world anything. We used blind rats and we'd have them learn a maze, and we discovered something absolutely astounding, and that is that when they get to the goal, when they get to the correct end, they, in effect, stand up and holler. They emit high-frequency cries, that are very high frequency -- way beyond the range of human hearing and we had fancy equipment and you could pick this up -- and we told a story about it and it was interesting and fun. I did this with him, but it was not where my heart was, and so after that I moved on.

But with McKee, he was a developmental child psychologist and I think he might have been kind of looking around for the next thing he was going to do. We talked a lot about transposition in animals and thought, wouldn't it be fun if we could see what happens to children as they get older and their ability to detect relationships between stimuli. It turns out, in a nutshell, that they don't need any help on quantitative dimensions like louder or brighter. That that's there. And if you train them on two stimuli, and this one is the positive one and it's louder, and this one is the negative, and then you move up; they just go right out with you. Children in kindergarten do that just as well as children in third or fourth grade. But, if you do things that involve dimensions that are not so clearly quantitative -- like the number of things they see -- my recollection is that with loudness they transpose perfectly all the way out. With frequency, youngsters have great difficulty with it, and as

they get older -- into third or fourth grade -- they understand what it is. And so, we ask questions about what was going on and whether in fact they didn't understand the underlying concepts, like the concept of numerosity.

And so, we did experiments that would ask questions about how much they understood of the dimension and how well that was related to their ability to respond dimensionally. There was a close correspondence. So, that was fun and interesting.

SG: You arrived here in 1950 and Tolman was a very unusual behaviorist because he already had cognition in his head.

AR: Absolutely. He started it there.

SG: But what happened in the 1960s was that the rest of psychology began catching up to Tolman and figured out that cognitive explanations were the way to go.

AR: Absolutely.

SG: And that caused great difficulties for people we know. But somehow you adapted to that change very , well it seemed very easily.

AR: I mean, yeah, you know I think it was easy. And I think partly it was that I had a mixed parentage. And that I not only had had this very influential young man, Howard Kendler, but I had been influenced by Wickens and Melton and people who were much more open in their views about the dominant theory. So I think it was relatively easy; I never experienced any particular problem in moving over.

SG: But when you moved over I was going to suggest that the work on selective attention was immediately relevant but you can pick any one of your research themes involving the new cognitive approach to animal learning.

AR: Yeah well, selective attention is a good one and I spent a fair amount of time asking questions about what happens. And it was all animal research. What happens when an animal is put in a situation where it must attend, meaning must see for example, more than one stimulus, but only one of them is the relevant one. And what happens to the ones that are irrelevant but must be seen. Are they are registered? Or does the does the brain have a machinery for ignoring them? This was a kind of a dominant theme for some time and it turns out that what animals do -- in some of these situations at least – is the stimuli that they must ignore, even though they see it, are harder to recover. In other words, they are suppressed, rather than just being there and neutral. Which I thought was very interesting.

SG: And that work primarily involved pigeons?

AR: Yeah.

SG: And you had developed all these less complex technology to both present the stimuli….

AR: That's correct. And it involved, at the time, very fancy equipment, stuff that would be child's play today, I suspect.

SG: But you developed this technology to present the stimuli and then analyze --figuring out just what the pigeons were responding to.

AR: I had some very talented graduate students that worked on it

SG: Which gets me to the next question on here: I have here in front of me that you had a fine record of graduate training. Any thoughts about that? What did you do with your students that made it so…

AR: I was very lucky, I think, and I don't know whether it's just luck. I was kind to students and I suspect that might have been part of it; that the students were my friends and we were colleagues together. I had a string of wonderful students.

SG: Yes, you did.

AR: Remarkable. And I thought I was very fortunate. And it's fun to have people around who are as smart as you are -- smarter. That helps a great deal in doing interesting research.

SG: Well, I'm going to come back to that in a little while. But let's switch gears for a moment and talk about your administrative jobs at the University of California, Berkeley. So I'll focus on just two of them. One was the fact that you were Vice Chancellor for Academic Development.

AR: What does that mean?

SG: You're going to have to tell us that. From 1976 to 1980. When we talked about this before, Al, I hadn't known just what you did in that job. But there was one thing in particular you mentioned that sounded very interesting -- in terms of equal pay for equal work or something.

AR: Well, allocation of resources was the thing that interested me. And that is that at that time the T.A. money was not evenly distributed across the campus at all. There were some powerful groups -- that I will not mention – that has a virtual monopoly on teaching-assistant money. And other places that were remarkably deprived. I decided to be unpopular, and removed money from some of these powerhouses and put it in places that I thought needed it. That took time and kind of pushing things around. It wasn't easy. But I thought it was well worth doing.

And the other thing that I remember -- it's been a long time of course.

SG: Since 1980.

AR: Yes that's 45 years or 50 years; it's hard to remember. But the Distinguished Teaching Award was kind of lackluster. And I did some things that helped build that up and make it more obvious and important thing on campus. But for the most part, I must say, that although I spent four years doing this, and I think worked very hard, I remember very little of it

SG: And then in 1982 you succeeded me as chair of the department of psychology, right?

AR: Right.

SG: And I think we both have the same attitude about that -- that our predecessor, Geoffrey Keppel, had really done the hard work.

AR: That's right. He was the mover and shaker. And I think after Geoff essentially reorganized the department -- and in effect forced it to come together as a unified group – it became easier and much more comfortable to be chair. I thoroughly enjoyed being chair.

SG: In 1991, the year you retired, there was a *festschrift* in Berkeley, involving both your former graduate students and an assortment of distinguished researchers in animal and human learning and memory. And they all assembled here and you had a set of papers. What are your memories of that honorific occasion?

AR: Well it was tremendous. And, of course, I was enormously flattered and pleased by it. I remember feeling that I had to comment on every paper -- that it was my job.

SG: That's a lot of pressure.

AR: Yes indeed. And to say something wise and sensible about every paper that was given -- and of course nobody can do that, but I tried and I remember that. But it was a lovely occasion and a wonderful collection of people.

SG: And it was both your former students, and in particular was Tom Zentall the organizer of that, or

AR: Yeah Tom Zentall and Bill Maki.

SG: But then in addition to a number of other former students, there were these distinguished folks from around the country.

AR: That's right.

SG: So quite a ceremony and an occasion. Did publications come out of it?

AR: Yeah. There's a whole series of papers that are included in a book

SG: And the book was edited by Tom.

AR: There was a major conference. It was during the time I was chair of the department there was a conference celebrating Edward Tolman's centennial year and many former students came back, including a group of my former students and we had a picture taken together and that's a picture that I treasure and wanted to have in here. The people in the picture, from left to right, are Tom Zentall. And then there's somebody in the background that can't quite be seen and it's Herb Roitblat. And then me, and then Charlie Leith and Terry Lewin and Bill Maki, in that order. It's a very nice picture and was taken at that time. They were all former students of mine, who had gone on to have very interesting careers.

SG: You were given the Alumnus of the Year award by Ohio State University?

AR: I'm not sure it was the whole university. I think it was probably mainly the psychology department, but I didn't pay much attention to the fine details. But I went there and was properly honored and gave a talk. And talked mainly about my mentors at Ohio State, which I thought was something that they needed -- to tell them about the people that had been there many years before.
SG: So, you're talking to them about Art Melton

AR: About Art Melton and Delos Wickens and Paul Fitts and their impact on me and what an exceptional place it was when I was there. And I thought they should be proud of that.

SG: Well I knew at least two of those people somewhat later in their lives, and I thought both Wickens and Melton were exceptional psychologists and really interesting people.

AR: I thought so too.

SG: Then in 2010 you were given a lifetime achievement award by the Comparative Cognition Society.

AR: That's right.

SG: Could you say a little bit about that?

AR: Yes. Well the Comparative Cognition society is exactly what it sounds like, it's all of the people who are interested in animal cognitive research, and they come once a year and that's a grand meeting and lots of people come from England and Canada, as well as the United States and Spain. It's a meeting of people who think about these issues, comparative issues, both in animal cognition and in comparative psychology, very broadly described. And so there will be people there interested in whales -- and all sorts of interesting things that go on at that meeting. But, it's mainly behavioral rather than physiological. There's some physiology that's there, but it's mainly people who do animal cognition the way I understood it. And so, every year they select somebody that they think is worthy and invite him to come give an address and dance around the room.

SG: And, of course, you were a child in your late eighties at that point.

AR: Yes.

SG: In 2017, when you were a mere 95 years of age, you were celebrated as the Emeritus of the Year by the Berkeley campus. The latter award was given in part for your work with the retirement community on campus. Could you tell us a little bit about what you've done for retired folk like me,

AR: Well, for retired folks like you I'm afraid not very much, but when you get older it may help.

SG: I participated in your Learning in Retirement series.

AR: Yes, you did. So you're all right. But, many people have. I was one of the founding fathers, or founding fathers and mothers, of the Retirement Center as it exists today. Before we built it, it was a secretary sitting in an office and she did secretarial service for both the Emeriti Association and the retirees' association. And we thought we needed something much more serious for our retired faculty and staff in general, and got some money. Bob Connick and I went and talked to the chancellor and outlined what we wanted to do. And he said it was a wonderful idea and he wished he'd had money, but he had no money.

SG: Which Chancellor was this?

AR: Tien. And about six months later a package of money came through. And so, we started and it was essentially a full-time administrator and half-time secretary, I think. And I and a guy named Mel Webber, who was a good friend of mine, we were both on the committee. And we thought that if the administration was going to give us this kind of dough -- which was essentially, I think, a

hundred thousand dollars -- that we ought to justify it some way so that they would feel that it was a good expenditure of administrative -- out of the teaching funds essentially. And so Mel and I started the Learning in Retirement program. And what we would do is find a topic. We sat around and invented topics - it's not hard - and then find speakers and inveigled them to each give an hour talk. We generally have four speakers and each one would give an hour talk and Q and A for an hour. We didn't pay and we didn't charge. We made it very explicit. And people usually agreed to talk. Usually. And so, we had about six or seven of those every year.

SG: And that followed a theme.

AR: Each one had a theme. Each one had a wildly different theme. The first one we had was on the history of the Berkeley campus. And one of the speakers was Clark Kerr. I think it's been an enormously successful and rewarding thing. We generally pull about 100 people for each lecture. A lot of people come

SG: But this is open to the Berkeley public.

AR: No, it's not.

SG: Who else? Who is there?

AR: Berkeley retirees.

SG: in the audience?

AR: That's correct.

SG: Oh I didn't know that.

AR: Berkeley emeriti and staff retirees. And if and if somebody has a reason for wanting to come, yes of course. But in general it's not open to the public and it's been going on now for 20 years.

SG: The wonderful thing to have done.

SG: So one of the things that I neglected to ask you about, but I'd love to hear you say something about, is the research you did with an octopus.

AR: I'm glad you asked that question. I was teaching a graduate seminar and a couple of people in it said that they thought that the octopus stuff on color change, that was related to camouflage presumably, was interesting and how would they go about doing it. I said "Well I don't know, but I could apply for a grant and see if we could get three or four thousand dollars." And I did. And we did that.

So, we would go out on the coast of Northern California and in the tide pools out there. You can -- if you're clever and know what you're looking for -- you can find little octopuses. It's a variant of -- it's not like the octopus vulgaris, which is like this [gestures]. It's Octopus rubescens, which is like this [gestures]. And that's about as big as they get. And they're in the tide pool; you can scoop them up and take them home with you. And you don't need a fishing license at least we never had one. We started up at the Bodega Marine Lab. That's too inconvenient, but the good thing about the Bodega Marine Lab is that Annie and I wanted a place to stay on weekends if I was going to be up there. And we bought a house on Tomales Bay, which has been a source of great pleasure to us for

40 or 50 years. And has had a profound impact upon me and my family and Carolyn and my children. And it's quite wonderful. All sorts of ramifications came out of that.

But the octopus stuff: We moved down to Berkeley, because it was just too much work, and I got material and built some aquaria up on the fifth floor. I had half a dozen of them up there and we had octopus in them and we'd take them out and put them in a special aquarium where we would teach them to attack the prey. What we found really had nothing to do with camouflage, but it has to do with a series of color transformations that occur in octopuses when they see and make decisions about prey. And it's a remarkable sequence of events that happens, the changes in color as they see the prey, as they observe it, as they gather themselves together and take off and stream through the water and grab it. It's ferocious and remarkable. We played around with that for a couple of years and wrote a paper. They went down their way and I went back out my way. But it a wonderful thing to do. It was great fun having that lab up there. On more than one occasion an octopus -- it had a tight glass top on to of the aquarium for each creature -- was about this size. It had a glass top and they could find a way to pry open a glass top and get out. And more than once we would find one halfway down the hall, and bring it back. I don't think we ever lost any, but they would escape. They were very wonderful creatures.

SG: Last thing if you wanted it to be, you just reminded me that we taught a course together.

AR: Yes

SG: And what are your memories of that course?

AR: That I learned about comparative psychology from Professor Glickman.

SG: And I learned about animal learning from Professor Riley. Yeah, we had a very good time.

AR: Yes it was a wonderful

SG: Maybe the students even enjoyed it.

AR: I think they did.

SG: By way of closing this fun-for-me question and answer I was going to go back about when we met and how our friendship developed over the years -- So we met in 1962…

AR: Right.

SG: I was out here on a fellowship for a couple years…

AR: And I was well aware of that.

SG: But then we really became friends after I joined the faculty in 1968.

AR: That's correct.

SG: And you'd been here for 18 years at that point.

AR: Yeah. So, I was an old hand.

SG: You're an old hand that I was just, but it didn't take long to figure out

AR: Right.

SG: But there weren't that many people interested in animal behavior on the faculty of the Psychology Department. And you and I…

AR: Was Benbow still around?

SG: Benbow was still around but

AR: but not doing very much.

SG: Yeah. And not doing much. Not very often.

AR: Yeah right.

SG: I mean there were physiological people

AR: That's right -- Rosensweig –

SG: right, but just being really interested in the animals as animals.

AR: Right.

SG: And we had a good time.

AR: Yeah.

SG: And are still having it.

AR: Yes, and are still having a good time.