Eleanor Gibson

- Born 12/07/1910 in Peoria, IL; Died 12/30/2002 in Columbia, SC
- Spouse – James J. Gibson, Ph.D.
- Ph.D. from Yale University (1938); M.S. and B.A. from Smith College (1933 and 1931)

Major Employment:
- Smith College – 1931-1949, Psychology
- Cornell University – 1949-1979, Psychology

Major Areas of Work:
- Perceptual Development, Visual Cliff Paradigm, Differentiation

SRCD Affiliation:
- Governing Council Member (1979-1985)

SRCD ORAL HISTORY INTERVIEW

Eleanor Gibson

Interviewed by Marion Eppler
In Middlebury, Vermont
July 4-5, 1998

Eppler: This is for the SRCD Oral History Project. I'm interviewing Professor Eleanor J. Gibson. My name is Marion Eppler. We're in Eleanor Gibson's home in Middlebury, Vermont, and today is July 4th, Independence Day, 1998. The first question is about your background, your family. Were there any childhood experiences or adolescent experiences that you think may have influenced you as a developmental--?

Gibson: I had the most ordinary possible childhood background in Peoria, Illinois. I went to the public schools, went to the public high school; it was very, very good in those days, no drugs, no problems, devoted teachers who tutored me for the college boards. It was all very ordinary.

Eppler: But you did skip some grades didn't you? You went to college a little early.

Gibson: Well, yes. I started at school when I was five and then skipped a grade somewhere, or two.

Eppler: So when you went to college you were 16?

Gibson: I was 16.

Eppler: And you went to the east coast?

Gibson: Yes. I was dying to get out of the Midwest, and I did.

Eppler: So you went to Smith College?

Gibson: Yes, where my mother had gone as well as various aunts, cousins of my mother and so on.

Eppler: So is that why you chose Smith? It was a family tradition?

Gibson: It was inevitable, yes, totally inevitable.

Eppler: And has that tradition continued?

Gibson: No, unfortunately. Well, it did in a way because my daughter went to Smith, but my granddaughter wouldn't hear of going to a women's college. I tried to tell her that it saved my life and gave me a career; it made all the difference between just being a housewife forever and being a psychologist. But she said, "Oh, it doesn't matter nowadays," and perhaps it doesn't. But I still think that women's colleges do a lot in the way of helping women to achieve all the potential they possibly can, you know, and they certainly did then every bit.

Eppler: You've often said that Smith College was very supportive of you in a way that Yale wasn't.

Gibson: Oh, they were wonderful to me, yes. When I wanted to go away to graduate school, Yale offered me absolutely nothing, but they were doing me a great favor to let me in. Don't know whether it was or not, but anyhow, they thought so. They offered me nothing whatsoever, but Smith gave me a scholarship to help pay my tuition. In fact, it paid all of my tuition. It was three hundred and twenty-five dollars, and it was called the Harriett Boyd Hawes Scholarship. I've always wondered who Harriett Boyd Hawes was because I feel that I really owe her, you know. The tuition was three hundred dollars, and I had twenty-five dollars left to spend on room and board.

Eppler: Wow! What were your parents' occupations?

Gibson: Well, although my mother had gone to Smith, she was nevertheless a housewife. When she went to Smith there wasn't too much else. Well, she actually did teach school for about two years when she got out of college, but then she got married and that was what you did in those days. My father was a businessman in the wholesale hardware business.

Eppler: Well, how is it that your mother went to Smith, but then she was married and had to give up her career and take care of her family? That was her career?

Gibson: Well, but she really didn't have a career, I mean, just teaching fourth grade or something was not much of a career for her, you know, because, in fact, she hated it.

Eppler: But when it was your turn it was different, you--?

Eleanor: It was indeed.

Eppler: How was it different?

Gibson: Well, the world had changed a little bit and Smith was really--everyone there was really determined that any girl who had ambitions or potential was going to get a good chance to fulfill it, you know. They helped in every way.

Eppler: But you were married shortly after you graduated from Smith, right?

Gibson: Yes. Yes, I was, but that was alright because I married somebody on the faculty right there, and I was a teaching assistant in the department. So that was great; it just rolled right along. So Smith gave me a teaching assistantship. It was deep Depression times and I wouldn't have been able to go away to graduate school then, but they gave me a teaching assistantship, which allowed me to get a master's degree there at Smith. You worked on the master's degree half-time and did your job the other half-time, and it was a very full half-time job too.

Eppler: When did you first become interested in developmental questions?

Gibson: Later, much later, because when I was first a teaching assistant I was very interested in comparative psychology. And in terms of the big problems of psychology, the one that was really stylish in those days was learning, so I was interested in learning; that was the big thing. But comparative psychology working with animals was what I was really hoping to do.

Eppler: Then why didn't you do that?

Gibson: Well, at Smith we had rats, but a very small animal lab; there wasn't too much you could do there. And Smith gives a master's degree, but no more, so that was as far as I could go. And also, the person who taught comparative psychology was not a good person to get a master's degree with. You couldn't really do a master's thesis on anything of that sort, so I did one on a verbal memory problem with my husband as my sponsor. That wasn't his field particularly, but he was a very good experimental psychologist, and he knew how to set up the experiment and make sure that I was doing a well-planned experiment.

Eppler: What attracted you to Yale? Why did you decide to go there for your Ph.D.?

Gibson: Oh, because Yale had everything. Well, there were two places to go; I could go to Harvard or to Yale, but at Harvard there was Boring and Boring really bored me. I'm sorry, but his field was sensations, and I wasn't interested in that. He wrote a book on the history of psychology, which everybody had to read in those days and of course I did, but it was all about sort of 19th century people. His own professor had been Titchener; he was very Titchenerian. I wasn't interested in going there at all and the other place was Yale. But Yale had both comparative psychology and learning, you see. They had it all as far as I was concerned. So there was no problem about making that decision at all, except the problem of getting in. At that time there was no question of going to Cornell because Titchener had died and they just had sort of old hangers on of Titchener's. They had a sort of, you know, a dull period when they continued in a Titchenerian tradition, but there was nothing new or very interesting, so I didn't think of that at that time at all.

Eppler: Okay. So while you were at Yale you worked with Clark Hull?

Gibson: Yes, I worked with Hull, though I thought that I would work with Yerkes because he had a chimpanzee lab there and that I felt just eager about. I thought, "Won't that be wonderful, I can really be a comparative psychologist and work with chimpanzees," but Yerkes would have none of that. He wanted no women in his lab and made it extremely clear to me that I wasn't wanted there. In fact, he got up, held the door in his office open, and said, "I have no women in my laboratory, that's that." So then I went to Clark Hull who was the real big gun in learning theory at that time, and it was quite dubious for quite a while whether he would take me on or not.

Eppler: Why was that?

Gibson: Because he said, "I have only advisees who will work within my system, work on problems that I myself approve of." And, you know, I was a little unsure about that because he was doing what he considered a very hardcore theory using a kind of geometrical system of--it

was called a logical deductive system, and you had to set out all your premises and then derive things from them, very formal. And I wasn't sure I was keen about that. And the principles that he took as his givens were all based on the conditioned reflex, and I wasn't very sure about that either. But I finally decided that I could use two of those principles, generalization and differential inhibition, and try to apply them to human learning. And so I went to him and suggested that, and he was sort of halfway positive. He said, "Write it out; I'll have to see what it looks like." So I wrote out a sort of précis of what I was thinking of, and he decided that would do and accepted me.

Eppler: So he started your experiments?

Gibson: No, no, no. Not then because I only had one year at Yale, and I had to take every exam there was in that year, so there was no time for that. I took the pro-seminar exams, and there was one every two or three weeks all through the year with a different emperor, so to speak; every professor there had his little empire, and he got two or three weeks with the pro-seminar, three weeks if he was very important and only two if he was slightly lesser. Then there was an examination at the end of that, and then you went on to the next one; and you had to get through that first, but there was a lot of reading on them all. We were told that there were ten people in the pro-seminar and that we would be ranked on each examination from one to ten, the number of people there. At the end of the year, the two at the bottom of the list when they added up their ranks would be dropped, that'd be that. So you really had to be sure that you did those examinations properly. Then you had to pass two language examinations, reading exams. And then you had to pass a statistics exam, and that was quite hard because I hadn't had a lot of statistics at Smith. I'd had very good master's degree preparation, so the pro-seminar didn't really bother me much because I had lots of preparation that helped with that, except for occasionally with Gesell for instance, but then you could--we had to memorize the Gesell schedule and just give it back, so you know, we rehearsed each other, all ten of us. The idea that two would be dropped didn't matter; we helped each other, we heard each other recite the Gesell schedule for a day or two before, and we all got through that one just fine.

Eppler: Who were some of the other professors in the pro-sem?

Gibson: Well, to begin this there was Professor Angier who was Chairman of the department and a very sweet old man, a darling person. He started us out on the history of psychology and had notes yellow with age that fell apart occasionally while he was talking. We read Boring, of course, and other very, very dull things. And he was the one who explained to us that we would be ranked from one to ten, since we started out with him. After that there was Hull, of course, and Yerkes, of course, and Gesell, of course. And then there was Walter Miles who was considered a very important man at that time, but he was extremely dull. He would come to the seminar each time with a little trick or an illusion of some kind, which he would exhibit to us, but he wouldn't really have anything much to say. Why he was considered so important, I don't know, but he was. And then there was Robinson who was quite well-known at the time; he worked on human memory. I don't think anyone remembers him anymore. I suppose I could have worked with him, and if worst had come to worst I could have done that. But then there were younger men in the department, and then there were other people in the Institute of Human Relations who came once or twice, people from anthropology and sociology and so on. Although it never became a real institute of human relations--it was fairly new when I went there--but there was the idea that there would be a lot interaction among social sciences that were based there. And it was situated down in the slums of the city because, presumably, you would get a lot of human relations to work on there. Anyhow, it was way off the Yale campus, but it was near the medical school, so we used the medical school library always. But you had to walk through very bad slums with very dubious looking people. Going back at night there'd be prostitutes standing in doorways calling at you. Well, obviously they were harmless to women. After all, it was a very peculiar territory. There were younger men too; for instance,

there was Neal Miller, a very young man on the faculty. Then let me see, Don Marquis. When I went to Angier and said that Yerkes had brutally turned me down, he said, "Well you can't expect one of our important professors to take you. Go to one of the younger men." Well, I would have done well to do that perhaps, you know. There was Leonard Doob, but he was a social psychologist and I didn't want to do that. But Don Marquis and Neal Miller were very nice guys. I could have asked them, but perhaps I was a little put off by being told that I should--that women should go to the younger men.

Eppler: So you went straight to Clark Hull's office?

Gibson: Yes. I never regretted it either, really. It was very good training. Not that I have any faith in the conditioned reflex as the basis of anything, but the method was a very stern, logical method, and I think that it was good for me at the time.

Eppler: Well, what kind of work did you do early in your career, and how did you end up starting to do developmental work? What first got you interested in developmental questions?

Gibson: Well, that really happened much, much later because I went back to Smith and finished my thesis, which was not developmental at all. That was on learning, of course, and transfer, and then I taught at Smith for quite a while. The war came along and for four whole years I was out of psychology completely as we were moving all over the country all of the time.

Eppler: Why? Where were you moving?

Gibson: First, we went to Texas. We were 18 months in Fort Worth and then we were two and a half years in California. Afterward, we went back to Smith and I taught again. That's Smith for you, they took me back after four years of psychology, you know. I was really worried about that, but it was alright and everyone was very supportive. Then I got to teach the animal psychology course, which I thought was nice. They had decided that maybe a huge introductory course was not a great idea, and we had a number of smaller introductory sections of about 50 students in each, and I taught several of those. And then I had a couple of master's students. I had two young children, so I didn't have a lot of time to start on something new. Then we moved to Cornell and Cornell, unlike Smith entirely, did not offer me a job.

Eppler: Why not?

Gibson: They had nepotism rules. They wouldn't have a man and his wife on the same faculty then, no indeed. But we went there; it was very important to move because my husband needed graduate students. It was just very important that he should have more time for research, more laboratory space available, and graduate students to work on the many things that he was thinking about. So he got various offers, which he thought about; we talked them over, but didn't go. And one day I said--we had talked about how he really needed to go where they had graduate students a lot, and I said, "Well, where would you really like to go?" And he said, "A place I'd like to go is Cornell." And the very next day he got an invitation to go to Cornell.

Eppler: That's great.

Gibson: It was amazing. He went as a full professor at what we thought was a princely salary then. So there was no question of refusing. He wrote and said, "What about my wife?" And they said, "No wives on the faculty here." But there was a new chairman, and it was Robbie McLeod; he wanted my husband to come very much, and he knew that I was a psychologist too, so he spoke to his department and said, "We've got to find a job for Mrs. Gibson."

Eppler: Mrs. Gibson.

Gibson: Mrs. Gibson, of course. And Howard Liddell who had the behavior farm so-called said, "Well, I'll take her on as a research associate for a thousand dollars a year," and that was part-time, you know. But that meant working with sheep and goats on the experimental neurosis, which was what he worked on.

Eppler: So you had to do the kind of research that he was doing?

Gibson: Well, as it turned out in the end, I did. It turned out alright because I got to work with animals again, and the sheep and the goats were great; I enjoyed them. I liked working with them. And I discovered very quickly that the experimental neurosis was pure fraud, you know, just a nonsense kind of thing. The idea was that you gave the sheep or the goat inevitable shock, meaning that it would hear a tone or a buzzer and then it would get a shock, but it couldn't get away from it. It would try to; it struggled and struggled, you know, but there was nothing it could do about it. The idea was that would make it neurotic. The only thing I could see that was really different about the animals that got this treatment was that when you went out to the pasture to get them they ran like hell, which seemed to me extremely sensible. But I got very interested; maybe that's when I started being interested in development. I got very interested in the maternal behavior of the animals, but especially the goats. The goats were very, very interesting. The kids were always born in pairs, or sometimes triplets, but these tiny animals would stand right up. Their mothers would lick them off and clean them up, and as soon as the mother was through with two or three, whatever it was, she'd get up, start walking around, and they'd get right up and follow her. It was wonderful. Then I decided that the behavior of these animals (the little ones and the parent) was very interesting and I would like to work on that. And I thought I would do some rearing experiments on the young goats. It was alright with Liddell; he never had a new idea. In fact, he very seldom came out to the behavior farm. Once in a while I would insist that he come and see what was going on, and he'd say, "Well if you'll pick me up at 11:00 this morning, I'll come out for an hour." I'd pick him up at home and after about an hour he'd say, "Okay, I'm ready to go home." So I could do anything I wanted really, and I discovered that there were barrels and barrels full of records. We made records with writing pens in those days. They'd write on long, long sheets of paper that would unwind as you went along. We measured heart rate and what else--something else. Anyhow, we had miles and miles of records, and they were all rolled up and put in barrels and nobody ever read them, so it soon became clear that I could do any experiment that I wanted to. And I did do a wonderful experiment on conditioning actually. I did that first comparing conditioning with inescapable shock of the kind that Hull was using with shock that the animal could escape, but otherwise the same conditioning circumstances. That was one of the best papers I ever wrote, as a matter of fact. So for the first time in quite a while I had a nice, beautiful, new piece of research to talk about at a meeting of the Eastern Psychological Association. So I sent in my abstract. Yes, sure it was accepted. I had lovely slides of the animals pulling their feet back: the interesting thing was that the animals that had the unavoidable shock showed the most extraordinary behavior. They were struggling in every conceivable way to avoid the shock, and I had lots of films of that, and so that was a very nice paper. Liddell said, when I told him about this, "Well, I think I'll come to the meeting." He said, "You know, if you don't mind I think I'll just give the paper for you."

Eppler: What? He didn't, did he?

Gibson: I said, "No thanks, I'm giving that paper myself," because he had nothing to do with it, absolutely nothing. But anyhow, then I began working on a rearing experiment. I had pairs of kids, and I divided them. I'd take one of each pair away from the mother and I was rearing them separately from the mother. Before she had a chance to lick them or anything, I washed them in a detergent solution, and that's when I discovered about the cliff--because in one case,

just as I had finished with one kid, another one was appearing, and I said, "Oh, here's this one all nice and clean." The farm manager was standing there watching, and I said, "Oh, what will I do?" He said, "There's a high shelf up there, just put it up there." I said, "Oh, I can't do that, it'll fall off." "Oh no it won't," he said, "It won't fall off." And I put it up there and it didn't; it just stood there watching with great interest what was going on. But anyhow, I started out my experiment. I had started out with quite a few pairs by that time, and I was watching their behavior in the field, and I reared them in different ways artificially with other mothers and so on, and it was just starting off great guns, very nice. When I went in the day after Easter and I started looking for my control group (ones that had stayed with the mothers), I couldn't find any of them. I said to the farm manager, "Where are my kids? I can't find any of the control group." "Oh," he said, "a lot of people come in and wanted kids for Easter presents for their children, so I thought those were the best ones to give them." He'd given them all away, so I was absolutely furious, you know. My experiment was ruined, so I quit.

Eppler: You quit working at the farm?

Gibson: Yes. Well, it was no use. I couldn't do it. By that time I was very interested in what I was doing, but there I was, just stalled, you know; I couldn't start over then.

Eppler: No.

Gibson: The birthing season was over; I'd have had to wait another year. It was absurd. I said, "Well, but now I can't do my observations of those animals." "Oh," he said, "I can tell you what they would have done."

Eppler: So what did you do instead?

Gibson: By then I was very interested in rearing experiments, and I approached Richard Walk who was a young man in the department that had rat labs. He did kind of ordinary rat research. And I said, "What if we write a grant, you and I together, to do some rearing experiments with rats?" "Okay," he said, "Why not?" So I wrote a big grant proposal for rearing rats in the dark, and we were going to test them in various ways when we brought them out in the light, comparing them with their littermates who were reared in the light, and actually we did lots of really nice experiments with them. In the last one we did, we had reared a lot of animals in the dark and this was a terrible lot of trouble. We did all this ourselves. We had two assistants, Herb Pick and Tom Tighe, who were two of my dearest friends from then on and great guys. They were graduate students then and they were our research assistants. So Dick Walk, Herb, Tom, and I had to feed all the dark-reared rats in the dark, clean their cages in the dark. This was all one heck of a lot of trouble, and we carried it on for a long time. We decided that this was so much trouble that we should do other things besides the discrimination experiments that we had planned. So that's when we devised the idea of the visual cliff, and we were going to test the dark-reared rats with their light-reared littermates on our visual cliff. We got this all planned in one day; we were going to get the rats out the next day. So Tom and I built the visual cliff out of just junk that was sitting around in the laboratory. We found an old red and white tablecloth (I think I brought it from home, I don't know), and we just set it up with pieces of glass that we found in the lab with metal poles and clamps. We set this all up and we had it fixed so that we could have the red tablecloth on one side and the other side was just glass, and the floor was way down below at that point, you know, and we put a little board across to separate the two sides, which we were going to sit the rats on when we took them out. The great day came, and we started the experiment. We took the dark-reared rats out first, and--no, we took the light-reared rats out first, and sure enough the light-reared rats all peered at the deep side where the glass was. Then they looked over at the other side and they all strolled off and walked around on the side with the red and white checked table cloth. So we thought, okay, alright, they can tell the difference. They've got a nice red and white surface over there, not that they can see red. It could have been black and white and that

would have been fine, but anyhow they all walked around on it, not on the cliff side. So we said, "Now here comes the big test." We got out the dark-reared rats; they did exactly the same thing, just exactly the same thing and much to our astonishment; we were so surprised. Then Dick said, "Well, now wait a minute, we've got to have a control for this," and so we spread the tablecloth out over both sides of the thing, thinking maybe there's something funny about one side of it. We got both sets of rats out, put them back, and they all just walked around over both sides. I remember Tom standing there saying, "I wouldn't have believed it if I hadn't seen it," so you know, that was great fun. And at that point we thought, "Oh, this is really fun; we've got something here," so we wrote a grant proposal then to work on what we called the Visual Cliff, which is probably a terrible name, but anyhow, it was a lot of fun. Then we did more and more experiments, and I guess that's when I got interested in development. We had kittens, puppies, baby chicks; they were wonderful. We got good apparatus, you know, properly built, and finally we said, "Alright, the time has come; we'll try human infants." So that was a big day. Then we thought, "How do we get human infants?" because none of us had ever worked with babies before at all. There was a department at Cornell of Human Development, and all of the developmental psychologists were in that—in the School of Agriculture, not in the School of Arts and Sciences. There weren't any developmental psychologists in the psychology department, but we decided that we would work with babies, and we weren't even sure how to get the subjects. We thought, well, the thing to do is put an ad in the paper, so we wrote an ad. It said, "Do you have a crawling baby? Call this number and make an appointment for your baby to take part in an experiment. Your baby can earn three dollars." I guess we put that first, "Your baby can earn three dollars," but we also said, "By taking part in an experiment." I came home and showed it to my husband. I said, "Look what we put in the paper." He looked at it and said, "Nobody will call you, they'll all think you're going to shock the babies." But we gave the lab telephone number (there was just one in the hall then). It rang and rang and rang, and we got any number of babies, which was a great deal of fun, of course. But since we'd never done this with human infants before, we didn't specify how old they should be or how long they should have crawled, we just said a crawling baby. Well, most of the crawling babies avoided the deep side. But as I looked back much, much later, there were a few who didn't. We thought, "Oh well, I hope their parents won't think they're abnormal or something, that there's something wrong with those babies." But on the whole, most of them did avoid the cliff because most of them had crawled for quite a while probably; we didn't know that then. But anyhow, that was all very exciting, and I got very interested in development after that. Okay, that's all on that story.

Eppler: Okay. Now we're back and today is July 5th, 1998. I wanted to ask you, yesterday you were talking about the Visual Cliff, and you talked a little bit about some of the grants that you received that enabled you to do your work. Who were the institutes that you were awarded those grants from?

Gibson: We always applied to the National Science Foundation. I'm pretty sure that's where we got them, but I wouldn't swear to it now. I would have to go look at the papers, and there would be a footnote saying that we thanked whoever it was. But NSF was the one we always applied to first.

Eppler: What would you say were some of the strengths, some of the benefits, the pros and the cons of these grants? And did you have any problems with the granting institutions?

Gibson: No, at that time granting was just fine. I never had a grant turned down in my life, a grant application. It's just as well I stopped when I did.

Eppler: Was there an issue about being officially affiliated with a university?

Gibson: Well, no because I had to work with somebody who had a lab, you see. Except in the field studies--I had forgotten about them--but I was asked to do them by Arthur Melton who

was at that time working for the army, I think, in some army lab in Texas. And he wanted--I was asked to--let me see, maybe Bob Gagne was working with him too. I was asked to write a bulletin or a sort of bulletin article but report for them on perceptual learning, especially relating to distances, places, and so on. And then to do experiments on distance judgments and they wanted them done outdoors because nobody had before. Some people had done them inside over a table or something, you know, but they wanted them outside. So I did a lot of those, and I had to get permission from the university to use--we used the athletic fields and the quadrangle actually because I wanted to do transfer from place to place. So I had to get permission from the university. When I said that the army was requesting that this be done and so on, they all said, "Oh, certainly, certainly." So I was able, with the aid of campus police, to keep the quadrangle clear on the days that we were doing experiments there. And I got permission to use the athletic fields at certain times too. So the university went along with that, but they certainly got their reward out of it; they got paid for it too.

Eppler: You mean in overhead costs?

Gibson: Oh, yes. They got very high overhead from it.

Eppler: How did you first get interested in reading?

Gibson: Well, I wasn't particularly. That was after my husband and I had gone to Princeton for a year to the Institute for Advanced Study. And I'd already done the field studies, the cliff and so on, and I was thinking about writing a general book on perceptual learning and then started it. And we had a visit the second semester from two people in the department of Human Development, and that's where development was taught at Cornell, not in the Psychology Department at all; there wasn't a single course in it. So these two people, Alfred Baldwin and Harry Levin, both full professors, came to visit me in Princeton and said, "We have been approached by some foundation." In this case they hadn't written a proposal asking for it; they'd been approached by some foundation offering to give them an extremely generous sum to get together a sort of consortium of researchers to work on reading and especially learning to read. They wanted me to join this group and made it very tempting. Everybody would have two research assistants, we could plan our own experiments, and we would have a weekly meeting to tell each other (the whole group) what we were doing and so on. And I said, "No, I've started a book on perceptual learning and development, and, no, I've really gotten into that. I'm really concerned with that." And they said, "But that's what this is; you can work on perceptual learning and development." And after a while--they stayed a couple of days and talked and talked--and I thought about it for a while afterwards and I thought maybe I could. So--and I never regretted it because it was better that I take a long time to think about the book, so I did work on reading with them. And they saw to it that I got laboratory space and so on, though I still wasn't a faculty member. But they arranged that I get laboratory space and two good graduate students to be research assistants. The university got its chunk out of that too, plenty. And I could choose what I wanted to do so I started reading about linguistics, which I didn't know much of anything about. I got very deeply interested in that and gradually began to plan experiments, and it led me into learning to read. They were perception experiments; that's what I did, but perception of materials that had to do with reading then. So on the whole and as I consider it, I stayed with this for about three years, I think, and they were very profitable. I had good graduate students, and a big plus was I made very good friends with the people in the Human Development Department. They were scorned by Psychology.

Eppler: Really?

Gibson: Those people were in the Ag School, you see.

Eppler: Why is that?

Gibson: Well, because they were in the School of Agriculture and Psychology was in Arts and Sciences. And Titchener never heard of a child--then there was no such thing as a course in developmental in the Psychology Department, but I found that this department was full of very interesting people, and I got to know two who finally became excellent graduate students of mine and came from that department. One was Roberta Golinkoff and the other was an Israeli girl, Ruth Sharabany. She went back to Israel and, I think, she teaches at the University of Tel Aviv. It sounds kind of oriental, her name, but it was middle-eastern. But anyhow, I got to know these people because we met every week with the graduate students who were working together. And the faculty members--Henry Riciutti was one too. So there was Al Baldwin and Harry Levin who became my very, very close friends. Then Henry, myself and,--I can't remember the other faculty who, maybe occasionally now and then, would come to our seminar meetings, but there were always very good graduate students too. And so it turned out to be very, very profitable. And I got to know a lot of developmental psychologists well then.

Eppler: But none of them in the Department of Psychology?

Gibson: Not at that time, but I went back and told the department, “These are good people; these are first-rate people, and we ought to have development represented in Psychology.” Eventually, when I finally got hired, it was because I re-presented it then.

Eppler: And what were the circumstances around Cornell finally hiring you?

Gibson: Well, that didn't happen for a long time. I think it happened because I was elected to the National Academy of Sciences, and perhaps it was embarrassing then. Oh, and my husband was reaching the point where he had to retire at 65, you see, and then it wouldn't be nepotistic anymore, so they couldn’t argue that.

Eppler: So were you the first developmental psychologist they hired?

Gibson: Yes. Oh, sure. Yes. But after that--now they work with the Department of Human Development very closely.

Eppler: Good.

Gibson: Well, there’s another thing. Once I got hired, I got a training grant in Developmental Psychology, which covered me in the Department of Psychology then and Human Development too. And we shared it; we got a very generous training grant, and it paid for graduate students for both departments. There were other faculty members from Human Development on it then too, but I can't remember that there was anybody in psychology but me. Anyhow, I wrote the grant, got it, and it worked beautifully. I mean we got working together very well and shared a lot of excellent graduate students. Maybe that was when Roberta came along; I'm not quite sure.

Eppler: Okay. I was going to ask a question, but I forgot what I was going to ask.

Gibson: I was just gradually--so much happened and you forget a lot of it.

Eppler: And you're working actually on two books now aren't you?

Gibson: Yes.

Eppler: Well, one has gone off to be reviewed on perceptual learning?

Gibson: Oh, I'd like to get that back and get to it again very much--the other one I'm doing because I had to have something to do in the meantime, you know.

Eppler: And what is that about?

Gibson: Well, you know what that's about. That's about the lives of two psychologists who happen to be married to each other and how it worked, especially in the days when women weren't popular around university faculty, except at women's colleges.

Eppler: Well, one question that we haven't talked about at all is when did you first join SRCD? Do you remember when you joined and what role you played at SRCD, or what role it played in your career?

Gibson: I suppose I joined while we were working on the reading project. I don't think I would have realized until that time that I really had a major concern with development; but then I did because while some of the people in the group did research on just mature reading and, you know, how to measure eye movements (there was somebody who specialized in eye movements in mature readers) and so on--oh dear, I should remember his name; it was in Human Development. He was very good at that. That did not interest me at all. I was interested in learning to read and how children did this. So I think it was about that time I realized my concern was really very much with development, and it fit much better with my interest in comparative psychology too; they always fit together pretty well.

Eppler: Do you see your work on reading as being applied research?

Gibson: It wasn't particularly, no. It didn't need to be; they didn't ask that. In fact, the eye movement stuff was about as unapplied as you could get.

Eppler: Do you have any memories or reminiscence about SRCD itself as an organization?

Gibson: Well, they gave me an award once.

Eppler: Did you ever hold office, or--?

Gibson: No, only the award.

Eppler: Do you remember what the award was?

Gibson: Well, if you let me look at my--

Eppler: Distinguished Scientific Contribution Award?

Gibson: Yes. Yes.

Eppler: April 1981. So when you first joined SRCD that was when you were doing your reading research, when was that? Early 1970's?

Gibson: I was a member of the Governing Council in 1979.

Eppler: '79, that's for SRCD?

Gibson: Yes. So I must have joined it well before that.

Eppler: Okay.

Gibson: But, you know, how would I know exactly when I joined; and I don't. Who would remember that? It was before 1979 obviously.

Eppler: Well these days, I suppose, we all joined as graduate students.

Gibson: Well, I suppose I would have if I had known when I was a graduate student that was what I wanted to do, but I didn't.

Eppler: So when do you think you really got interested in developmental questions?

Gibson: Well, I think probably when--I actually think that the babies and the Visual Cliff got me interested because I never worked with infants or children before. I'd worked with young animals and I knew I liked that so--and I was interested in rearing and different kinds of rearing, so I really was interested in development. But in terms of development, if you think about it as just human children, I didn't settle down especially to that until I got involved in reading. And, of course, I would go back and forth on animals any time if I still could--

Eppler: So that early--?

Gibson: --well that early development of animals. You can do experiments with them, you see. You can change rearing conditions and see what happens, and I was very, very interested in that. So really, my interest in development began long before I was particularly interested in child development.

Eppler: I wanted to ask, what courses did you teach while you were on the faculty at Cornell?

Gibson: Well, they only hired me half-time, you know, so I could get the rest of my salary out of grants.

Eppler: Okay.

Gibson: Boy, they were pretty cheesy to women in those days. They felt they needed to hire me, but--what did I teach? Oh, I think I taught perceptual learning probably, which was what I wanted to do.

Eppler: Was that a graduate seminar?

Gibson: Oh, yes I did, indeed, because I had a lot of graduate students then.

Eppler: A lot of graduate students, all working on perceptual development, had some questions?

Gibson: Yes.

Eppler: Did you teach any undergraduate courses--

Gibson: At Cornell?

Eppler: --any general developmental courses?

Gibson: No. No. By that time I was well along and they were only paying me half-time. I had a lot of graduate students. I'm sure I had some undergraduates because I remember some good ones, but I think that it was in an advanced course that I had graduate students in too. And at Cornell you weren't expected to teach a hell of a lot, you know.

Eppler: So you probably taught a lot more at Smith than you did at Cornell?

Gibson: Oh, yes. At Smith I taught a lot. Oh, sure. You were expected to teach three courses a term there.

Eppler: And what courses did you teach?

Gibson: Or if not it had to be very big courses.

Eppler: Right. So what courses did you teach at Smith?

Gibson: Well, I taught mostly introductory courses at Smith, but eventually I got to teach the comparative psychology course, which was very much to my liking. But I never taught developmental psychology there, never. And I wound up before the war finishing the social psychology course because my husband left early in the semester. And then they got Dick Solenberger from Mt. Holyoke, who taught it there, to do double duty and come over to Smith and give a couple of lectures a week in Jimmy's social psychology course. But then he went off in the military too, so in the end I found myself holding the bag, knowing nothing about it.

Eppler: What are some common themes that you see running through all the different areas that you seemed to have worked in?

Gibson: Learning.

Eppler: Learning was there from the very beginning?

Gibson: That's it, from the very beginning.

Eppler: Because it does, at first glance, look as if you've worked in a lot of different areas that are not related.

Gibson: Yes, but learning always plays a role in it somewhere.

Eppler: What would you say, in your opinion, are your most important works, your most important publications?

Gibson: The book that I wrote on perceptual learning and development certainly was. I've got it; the publishers were so pleased they sent me a special leather-bound copy for Christmas with gold lettering.

Eppler: That book won an award, didn't it?

Gibson: Yes, it did. It won the Century Award.

Eppler: Any other papers in particular that you feel are especially important?

Gibson: Well, lots of them I suppose, but I don't think of any special one. I don't think of the reading book as especially--well, it was alright. It was sort of the culmination, three years of work, and I started to plan it and then Harry Levin said to me, "What are you planning to do next year?" And I said, "I'm getting the only sabbatical of my life and I'm going to write a book on reading," and he said, "Well, I'll help you; I'll join you." That was alright because he had gotten me into it and we had worked on it together and I thought, “Oh yeah, of course, it wouldn't do for each of us to do it separately,” so we did it together. It was an okay book, but we sort of divided things up and it was nobody's point of view, strictly speaking, all the way through. But it was good, and it was a good finish of what we had been doing. It was good at the time and sold lots of copies.

Eppler: What are some of your hopes and fears for the future of our field?

Gibson: Oh, I'm just worried to death about it. I think that people are forgetting what psychology is supposed to be about. I think that people are so concerned about consciousness and representation. For example, are babies conscious; when do they have representations; can we prove they have representations? You know, I'm not sure the ways they use to prove it are really legitimate, and you don't know what the representation might be like anyway. You have no idea.

Those who inspired and were influenced by Eleanor Gibson:

Mentors
Professor Angier
Howard Lidell
Clark Hull

Colleagues
Alfred Baldwin
Harry Levin
Robbie McLeod
Don Marquis
Arthur Melton
Neal Miller
Herb Pick
Henry Riciutti
Richard Solenberger
Tom Tighe
Richard Walk

Students
Roberta Golinkoff
Ruth Sharabany