Carolyn Rovee-Collier

- Born: 4/7/1942 in Nashville, TN
- Spouse: George H. Collier
- B.A. (1962) Louisiana State University, Sc.M. in Psychology (1964) Brown University, Ph.D. in Psychology (1966) Brown University

Major Employment:
- Trenton State College – 1965-1970, Assistant Professor, Assistant Professor I, and Associate Professor
- Rutgers University – 1980-1989, Professor
- Rutgers University – 1989-Present, Professor II

Major Areas of Work:
- Behavioral Neuroscience
- Cognitive Psychology

SRCD Affiliation:
- Member since 1970

SRCD Oral History Interview

Carolyn Rovee-Collier

Interviewed by Peter Gerhardstein
Stockton, New Jersey

February 4, 2006

Gerhardstein: Carolyn Rovee-Collier of Rutgers University. My name is Peter Gerhardstein. We are in Stockton, New Jersey, and today is the 4^{th} of February 2006. So we'll begin with—

Rovee-Collier: You have to ask me the questions.

Gerhardstein: Right. We'll begin with the list of general intellectual history questions. Could you describe some details about your family background, and any pertinent details in that area that you'd like to include?

Rovee-Collier: Well, I was born at the beginning of the war, World War II, and my father was a young professor at Louisiana State University, and got his Ph.D. at Vanderbilt. He's a zoologist. And my mother and he met in college. At that time it was very unusual for women to go to college, and she had to work to earn her way through, and after they got married at the end of their college lives where they met, and he went to graduate school, while she worked as a seamstress to support him and help pay the bills. And they--and for her entire life she worked as a professional seamstress and earned money, because that's what she knew how to do. But she majored in foreign languages, which was very unusual, but she had no intent of using them. My father was a biology major at a very small college in Tennessee before he went to graduate school in comparative anatomy at Vanderbilt University. After he got his doctoral degree he moved to Louisiana State University, where he spent his entire career, beginning as an assistant professor and ultimately becoming a named professor of great distinction. He published nine editions of a world-wide accepted text on comparative anatomy of the vertebrates, which is still in use in 39 different countries in the world. So I had a very strong background in science.

It was encouraged; people thought about science in my house. But the role of the arts was there through my mother and her experience as a language major in college. There were always lots of books in our home. I went to a very fine grammar school, elementary school, and high school that was affiliated with the University. I was very privileged. I started the first grade at the age of five in a private school and learned how to read and write well, but I had learned how to read and write a little bit before then. Then I moved to the second grade at the elementary that was affiliated with the University. It was a school in which all the faculty in the school had PhDs, and the student teachers were people in education from LSU who did their practice teaching in that school. So I had a very, very fine high school education, and just moved naturally into LSU the summer after I graduated from high school, which was a year earlier than everybody else. Because I lived only a mile away from the University, I went to summer school every year, and I graduated from the University in a very short period of time at a very young age, because I just always went to school. And so I think that going to graduate school was just expected. I mean, I never considered anything but going to graduate school, because my father had gone to graduate school, and that's what you did when you graduated from college - you went to graduate school. And so I was very fortunate in that respect. I was born in Nashville, where Vanderbilt is, maybe six months before my father received his PhD, and grew up in Baton Rouge and actually have never returned to Nashville since. Another thing that my parents infused in me was the idea that you always had to work, because my mother always worked, and my father always worked, even when very young, because the Depression was very hard on their families. And so even though the tuition was only $35 a year at LSU when I went, that was a lot of money at the time. Fortunately, I got a registrar's scholarship that paid the $35, and that was very good for my family. When my father started working he made $1,200 dollars a year, and that by today's standard is a drop in the bucket. But then it was a very good income, and it was a guaranteed income, and that was very important. But my mother continued to sew professionally for her neighbors; she always worked. And so I was a typist and a research assistant and various things at the University. I always tried to earn money. I'm very sensitive to the fact that people who don't have to work or who haven't ever worked, whose parents give them everything, may be at a disadvantage because they may have lost--I don't know--they may have lost that Horatio Alger work ethic. For them everything comes so easily, but in science things don't come so easily. I grew up in hard times; there was always a grad student living at our house who couldn't afford to pay rent somewhere else. So I'm always thinking, maybe I should help graduate students out a little bit, maybe I should take in students for off campus housing and so forth. So that’s my background. And in college I ran--well, in college there was a course that was offered in learning--I guess it was called educational psychology. My father really didn't like persons trained in education, he thought that because Vanderbilt had this teacher's college, if you weren't at Teacher's College or the affiliated teacher's institution at Vanderbilt, if you were from anywhere else, you weren't worth your salt--so he wouldn’t let me take this learning course that had to do with--well, he was so afraid the learning course would be taught by the College of Education that he called to check and make sure that it was taught by the Psychology Department before he would let me enroll, because he thought I was too young to make those kinds of strong decisions. And so I took one course in learning; it was taught by the Psych Department. And it turned out that the course was taught by a fellow named Robert Branson, who was very--I still thank him, I actually met him 20 years later and thanked him again--inside, I thank him all the time for introducing me to learning. He used a text, which was a very strong Skinnerian-type text, by a fellow named Reed Lawson. I still have that text. It turns out that my current husband of 30 years, George Collier, was Reed Lawson's graduate advisor. And so my husband, George Collier, taught Reed Lawson in graduate school, who then taught Branson (Branson was his PhD.) I used the text, which is what led me to be in experimental psychology, so what goes around comes around. It's a small world phenomenon. But that learning course that I took--I took it in the summer--was very hard for me. It was very different from anything I'd ever heard about, stimulus generalization, conditioning, I'd never heard of anything like that before. I had not taken intro to psych, so this was like a new language. At LSU, the courses began very early in the morning, because they didn't have air conditioning, and it got very hot in the day. So the course started at 7:00 a.m. and ended at 9:00 a.m., and another section immediately followed at 9:30 a.m. and ended at 11:30 a.m. and I had so much difficulty learning this material-- but I really liked it. But it was so new to me, and it was a very intense course in the summer, so I took both sections. I went to the 7:00 a.m. section, and I sat through it again. Branson taught it again in the next section, and I made a really high A, like 99%, on all the exams, which were essay. I can remember walking around and around pacing the floor of my house while memorizing acquisition, extinction, stimulus generalization. It was the most intense experience I can remember having. And that started my love and my interest in learning. It must have been cognitive dissonance. If you've got to spend that much time and effort doing it, then you must really like it or else you're really stupid, and I didn't want to waste my whole summer being stupid, so it must have been important. Anyway, after the course ended, I volunteered to run rats for Dr. Branson, who was doing magnitude of reward studies. This was in the early '60s. (I was born in '42. I went to college, started in 1959, and graduated in '62, and went immediately to graduate school. So I was 20 years old when I went to graduate school.) But I ran rats for Branson and every afternoon, I would go over to some old Army barracks where the lab rats were, take out a bunch of rats one at a time, and put them in a straight alley maze to see how fast they would run depending on how many pellets they got at the end of the runway. The problem was that rats are nocturnal, and they basically slept in the runway. They'd fall asleep halfway down the alley. They didn't really care about how many pellets they had. That was somewhat of a disaster. So I started running younger and younger rats. You know, when rats are just weaned, they're not nocturnal yet. They're diurnal, because their mothers are nocturnal. They can only nurse in the daytime, and so they're awake and busy in the day. So the younger rats would run more easily to different magnitudes of reinforcement. And that was a real commitment spending two or three hours every afternoon to go over and run these rats for two years! In the meantime, because I lived so close to campus, I was always in there, and the graduate students who were running their experiments at the same time in the same building realized that I lived close. And so I would run all their studies for them when they went home for vacation. So I had a lot of experience dealing with different problems of handling and managing rats. I must confess say that every now and then, the little baby rats would crawl in my lab coat and get inside my blouse, and I'd go to class and there'd be things moving in my clothes. I couldn't get them out. There'd be animals running up and down my arm during class, and it was very--well, you just wondered if anybody else noticed this at all. But I obviously noticed it. Then I got involved with other research and volunteered also to run some studies for Dr. Don Hoffeld, who'd graduated from the University of Wisconsin and had worked in the fields of perception and learning at Wisconsin. I also ran some studies using college students that weren't very interesting to me, but I was good at it, I guess. I was conscientious, and I was the only person who was working for Dr. Hoffeld. For my first two publications, he put me on these studies as junior author. I didn't even know that until I got to graduate school. They weren't published in a very good journal, but it made me realize the importance of being--of having a faculty member include an undergraduate on their publications, and now I always include undergraduates on publications if they make any major or significant contribution to our work of any kind. It really does help their career, and it makes them appreciate science in their lives. I think it's very important to involve undergraduates in research, and this early experience of my own made me realize how important it was to bring in undergraduates, to train them in research, and that's a major part of my life now. I think that probably does it. Oh, in addition, because I had all this research experience, and because my father was a scientist, he made me aware of the Jackson Memorial Laboratory's undergraduate opportunities that were available in the summer. Jackson Lab in Bar Harbor, Maine, had a behavioral science research station, called Hamilton Station, in which they did behavioral research of all kinds. And because I had worked as an undergraduate doing studies in learning, and because people were willing to support me like Branson, I applied for summer research. The application said, "Who do you want to work with if you come?" I got the list of people who were at the Jackson Lab Hamilton Station, and I got lists of their publications, read them all and decided that Walt Stanley was the person with whom I would be most closely aligned. And so I applied to work with Walt Stanley, and I was accepted, which was really quite amazing: there were only 14 people in the country that were accepted! Walt Stanley was a Skinnerian, and he studied conditional discriminations and early sucking experience, he was--and his big thing at the moment was how sucking is learned, and how practice affects sucking. They studied puppies at the Jackson Lab; they had a variety of species there, Basenji, Cockers, and Shetland Sheepdogs, and various mixes, and Beagles, and so as soon as the puppies were born in the litter they would be removed from their mother. And once they were removed you have to keep them alive, my job, every two hours around the clock for six weeks up to maybe eight, I would feed them. But the Jackson Lab Station was 20 miles away from where I slept, and so it was not easy all through the night feeding these dogs. I kept them in low square boxes initially, and I fed them with a tube, which would slide down their throats and you'd inject, you know, how much you thought there actually should be or that was prescribed, and when the litters were very large, it would take 1 ½ hours to feed them and in two hours I'd have to start all over again. And I really didn't sleep much during that period. But Walt Stanley was interested in this question: if they're not on their mother can they learn--does the sucking practice facilitate subsequent sucking? If they don't have the practice are they at a disadvantage? And so one of the things I had to do with these puppies was give them practice sucking on my finger every morning. I'd put a rubber glove on and give them my forefinger, and they'd suck for 20 minutes. Can you believe it? I'm standing there with my finger in a dog's mouth sucking. Or put them on the mother for 20 minutes, take them away, or just tube feed them. And it turns out that practice helped a little, but the tube feeding group was the most interesting group, because they got to the point where they would anticipate the feeding, and to get the tubes down their throats you'd have to hold their heads up and slide it down sort of straight, and they would have their legs splayed out like that and--so that the tube could pass down their throats easily, and it turns out that they would adopt that position when they saw me coming, and they would all splay out. Well, interesting, that's called conditioning it turns out, but not at that point in time. And so, I mean, that wasn't a big deal at that point in time. And so when they were at the age of weaning Evelyn Satinoff, who was there at the same time, and she would hold the mother, and I would try to get the babies to suckle off the mother and see who ingested the most, and we had to learn that you express all the liquid and everything out of them initially, and weigh them, put them on the mother, see how much they drank by weighing them afterwards and the difference in their weight after and their weight before was how much they ingested, because how else could you measure it. And it turns out the puppies who had been tube fed didn't have the right motoric behavior to nurse from the mother. They would put their legs out, and put their heads back, and they would slide off her, and so we'd put them back on again, and they would slide off. And they actually spent more energy during a feeding than the energy from the milk they got from the mother, and that was an interesting thing, because they had learned a behavior that was competing with the behavior that was required and the reflexes that were required to get milk. And that became later important when I subsequently worked with baby chicks. But I'll come back to that later. But that was a very interesting period, because one of the things that happened was when they were kept in these square boxes they would suck their paws, and their paws would actually be bleeding. They would get up in the corners of the boxes and curl around and suck, suck and suck. So you'd think, well, maybe they're deprived of sucking. But in point of fact you had to figure out a way to keep them from doing that. So I finally decided to get big ice cream cartons and put them in circular boxes so there wasn't a corner that they could crawl up and wedge themselves in, but they still sucked their paws. They managed to do it even so. And so finally I had to figure out a way of getting a Victorian collar that was stiff around their necks so they couldn't reach their paws, and that was--you know, that early business of problem solving became very important, because one of the things you have to do in science is solve problems. The apparatus is everything, and figuring out how to present the stimuli has become everything. And just the fact of having to deal with these problems and things creatively and flexibly with what you could get instead of just relying on calling some apparatus company became a very significant part of our laboratory. You have to watch, see what they're doing, and try and figure out why they aren't doing it the way the books say or you think they should, and then figure out how to deal with that and other competing responses and so forth, and that's also become very important. So Walt Stanley was a big influence in my life, and when I went back a second summer to finish the studies I had started the year before, and he encouraged me to apply to Brown University, because he'd been at Brown and so I did. My mentor at LSU had encouraged me to apply to Wisconsin, because he went to Wisconsin, and I did. I have always told undergraduate students, "Go where you get the best education. Don't go somewhere where the weather's good or something like that." Yet I--from southern Louisiana, did that. Hoffeld, my mentor at LSU, said, "Oh, when you come out the front door of the department at Wisconsin the wind from the lake hits you and, oh, you die of cold," and I thought, "Oh, I can't do that." And I didn't even know, I was so local, I didn't even know where Rhode Island was. I had to look it up on a map to find the state. That's how ignorant I was. I knew about Maine, but I didn't know about Rhode Island. And I thought, "Oh, I could do Rhode Island, that's probably better." So I went to Rhode Island because Walt Stanley urged me and because it was warmer. That is not a good reason to go to graduate school. However, it worked out extremely well, and I probably couldn't have made a better choice. I had a research assistantship offer at both places, I had very good grades at college, but going to Brown changed my life. Working with Walt Stanley, who encouraged me to go to Brown, also changed my life. And I'm forever grateful, so there've been a lot of fortuitous events along the way.

Gerhardstein: So what early adult experiences were important in your intellectual development, in particular, collegiate experiences and--

Rovee-Collier: Well, nothing--I just enumerated those, and there hasn't been much. In my adult life, which started very young, I think that probably reading, and reading, and reading--at Brown you had courses in learning one, learning two, learning three, it was the golden age of Brown. All the big names in the history--now in the history of psychology and the recent history of psychology--were there. Schlosiberg, I had courses with him. Al Shrier, who went to Wisconsin in comparative, I had courses with--you had to take a variety of courses. It was outstanding that you had to take a variety of courses, because I'd never been exposed to any of these, and the biggest thing now in the field is specialization and Riggs taught me sensory processes. Pfaffman taught me physiological psych at the graduate level. There's Don Blough, who just retired, was on my master's thesis committee. You know, there were--and Lew Lipsitt of course, in whose lab I did my work, but with whom I did not study. I was his teaching assistant for experimental child psychology, but I only worked in his neonatal lab. And my mentor, true mentor, was Trygg Engen, who studied psychophysics, and unbeknownst to most people, I got my training in olfactory psychophysics with infants, and that was an important step, because that taught me how to think about stimuli and how to ask experimental questions. I always think of how babies perceive things as psychophysical or subjective functions. I don't think in terms of absolute physically presented stimuli. I always think in terms of power functions. I always think of how you can subjectively equate this stimulus with that stimulus, and that has altered the way I think about research. My criticism of many of the studies of habituation and memory is that the stimulus is not viewed correctly. And I also think in terms of ratios instead of absolute magnitudes--adding or subtracting differences--and it turns out that when you appreciate the power of thinking about things psychophysically, you enter into new views of how behavior actually is--the scale on which behavior operates. And I think that thinking in terms of ratios, you could look at how long infants remember something. Well, how long can it be that you can still reactivate or bring back a memory that's been gone? It's four times the length of time infants originally remembered, and that function is flat over age. That's a broad scale--but if you think in terms of absolute magnitude, they remember increasingly longer, and you can recover a memory increasingly longer with age. You lose the ability to compare across age groups once you stop thinking in terms of subjective magnitudes. So my baseline ratio is a log scale, and so there you have it.

Gerhardstein: So what did you actually--how did you end up in Lew Lipsitt's lab and what were you doing with Lew?

Rovee-Collier: Well, thank you. Trygg and Lew had done a study of habituation and response decrement as a function of the strength of the olfactory stimulus, and that was published--I don't know--early on, and they were just finishing that work. And Trygg was interested in olfaction from Norway, and psychophysics is big there. And he was looking at infants, because infants were available, because Lew had just gotten a center grant--there were 16 places in the country that got them--for a prospective study of infants at the Providence Lying-In Hospital. And they had all of the clinic mothers enrolled in this study, and private doctors also enrolled, and what happened in these 16 centers in the country was the mothers would get free prenatal care and free postnatal visits up through age of 16 if they would bring their child in periodically for various kind of measures and allow their child and infant to be in various studies. And at the end I think the last data point was collected in 1984. It started in 1958, and there were 42 thousand subjects that were enrolled in this project! Well, they were taking all sorts of measures, everything you could think of, and so Lipsitt was looking at early learning, and reflexes and so forth, so he enlisted Engen to look at responses to olfaction and so forth, and that's why Trygg was in that lab, and that's why Lipsitt started the early classical conditioning studies in that lab. At that time babies were in the hospital for four days, so they had a four-day period over which they could study them. Today it's sort of like a drop-in center. You go in, have your baby, and go home that night. Most of the long-term care is with risk babies who are in intensive care units, and so the ability to study early learning and early behavior shifted, and so there are many more studies today in preemies and risk subjects than there were then, and today that's where a lot of the money for research is invested. So a lot of people who were doing research in the heyday of learning turned to doing risk studies like Mark Strauss at Pittsburgh, and their students have subsequently studied intellectual abnormalities or early risk. At any rate, Trygg was doing olfaction studies with Lipsitt in this project. Because I had studied sucking in puppies, it seemed natural I would study sucking in babies. And when I went to Brown I got into experimental child psychology, and this was what Schlosiberg told me when they admitted me, "What do you want to be--in experimental psychology?" "Learning." "Well, why don't you be in experimental child psychology? That's a good field for a woman." This was 1962. But I wasn't offended. I wasn't offended. I was too ignorant to be offended at that point. And he was right. It was a good field for a woman. And so I had entrée to a lot of things that I might not have had entrée to later on when I tested babies in their homes. But I worked with Trygg because Lipsitt had a lot of students, and because he was gonna be on sabbatical in my fourth year and didn't want to take on a student who would be a fourth year student when he was away. And I initially looked at the pacification effect for my master's thesis with Levin, measuring how much babies wiggle on the stabilimeter as the magnitude of response to having a pacifier stuck in their mouth. This grew out of work that Kessen had done at Yale and had published, and the notion was pacification always reduces activity. Everybody knows that. If baby is crying, mom sticks a pacifier in his mouth. However, Hebb said there's an optimal level of arousal, and so we found that if babies were quiet, putting a pacifier in their mouth actually would increase their level of activity. And so that was interesting, and it upset Kessen a lot, and in one of the early handbooks he took me to task on two pages for that. But then I moved on using the stabilimeter with the babies asking how they respond to different members of an aliphatic series of odors--that is, odors that differ only by one carbon in a chain--I had to take an undergraduate course in organic chemistry at Brown in order to understand my stimuli. Can you believe? At any rate, it was very interesting that babies were very responsive to the number of carbons in the chain, and the odors vary quite a lot going from propanol, a very cooling wood alcohol odor to a very oily, long lasting, sweetish smelling octanol, and they gave different magnitudes of response to these. And then using psychophysics, I equated a 25% solution of C3 propanol in terms of the magnitude of jiggle with 100% C3 hexagonal solution, and we did that by simply drawing a parallel line across the curves of responding to different alcohol chains in all concentrations on the y-axis and finding at what point the horizontal line intersected different curves for the different alcohols. And once you equate the stimuli like that you can see that infants respond equivalently if they're tested with the two concentrations. For my dissertation, I asked do infants habituate to the two equally because they were subjectively equal even when they were tested with two different quality alcohols. So now I have matched stimuli and I'm looking at habituation to them. And it turns out infants don't habituate equally to them. It depends on the physical properties; that the oilier stimuli obviously persist longer. It also turned out--and this became very important in my current work and has perfused my thoughts ever since--that the timing of one stimulus in relation to another was really critical. You could get an increase in response or you could get a decrease in response over successive presentations of this stimulus followed by that one, subjectively equated in terms of magnitude, and yet you got differences depending on the timing of the presentation of one and the presentation of the other. Our question was initially if they were equated would it be the same as if you presented one stimulus, the other, then one, then the other, or if you presented one kind only or the other kind only. And that's an interesting question today still I think. At any rate, that evolved into my appreciation of the temporal parameters in conditioning studies and gave rise to my current thinking about time windows, which is the interval between two successive stimuli or events that permits them to be integrated.

Gerhardstein: Alright. So would you characterize the development of your ideas in the field of child development as evolving in a straightforward fashion or in a way that involved sharp turns in theoretical views or research style? And you know, why--effectively, why would you characterize those views in the way that you do?

Rovee-Collier: Well, my research is really very boring, and my story's very boring. And I don't think I'm very smart. Some people say I'm creative. I don't think I'm very creative. But I'm sharp, and I put two and two together in ways that other people don't, and I profit greatly from my past experience, and I apply it to things that may not be related or that people may not think are related. And so all of my ideas have evolved in a relatively linear fashion, except that they evolved as a function of my appreciation of events and applying that to the next thing. My research has always been very programmatic. One thing builds on another, builds on another, leads to new things--always an adventitious unexpected outcome. I've never been successful in predicting anything yet. Whatever I say is going to happen is going to be exactly the opposite. But I always seize on that and then move forward. And early on, when I was running the rats, I realized that you have to appreciate the environmental or biological constraints, because there they were, falling asleep in the runway in the middle of the day. Well, why not? They're nocturnal. But experimenters didn't appreciate that, and so I learned, well, you run nocturnal animals at night and you run diurnal animals in the daytime. Subsequently, the work with Stanley led me to realize that you have to solve problems, and that when you're studying a young organism theory doesn't matter if the apparatus gets in the way or the way you're doing the study gets in the way. So much for that. At Brown, I learned--and that developed my appreciation for experiments and my love for running experiments--at Brown I discovered the importance of psychophysics, and that has since been in my mindset, and I ask questions very differently than everybody else. And I'm trying to teach my children--well, they are my children--my students how to think that way, because I think that that influences your approach to experimental design; it makes it better. When I was at Brown, Lew Lipsitt and his lab went to visit Harvard, because a girl who was working with Lew, named Barbara Morgan had worked with Ogden Lindsley at Harvard as an undergraduate. Lindsley was Skinner's graduate student. Barbara had done some early conjugate reinforcement work with him on adult music preferences. Nobody knew what conjugate reinforcement was, so Lipsitt took his lab up to visit Lindsley and find out all about this conjugate reinforcement. And I happened to be there when they came back and were discussing it, so I sat in the room while they were discussing this visit. And apparently one of the things about conjugate reinforcement was that the organism, an individual could control the stimulus along an intensity or a value of 0% to 100%, and by the subject's own responding it could control the magnitude or the intensity of the reinforcing stimulation it got. And Lindsley had done some very interesting things. He had discovered this reinforcement schedule one night--he was working at the Massachusetts Psychiatric Hospital or something in Walden, and he was setting up some experiments in which patients would press a button to turn off an aversive sound in their ear, and as long--in headphones that would come--was coming through headphones in their ear--ears--obviously that's where headphones go--and he was stuck checking out the apparatus one night. The apparatus worked like this: as long you press the button it would drive down the sound to nothing, and if you kept pressing it, it would stay at nothing, and then if you slowed up, the sound would slowly rise until it got to its maximum intensity again. In essence, you had to drive down the sound by pressing rapidly, and rapidly, and rapidly, until it got to nothing. He fell asleep in the apparatus one night in a chair with the headphones on. And he discovered the next day when he looked at the cumulative records that there were periods of time in which there was a lot of responding, and he didn't remember any of it. So while he was asleep at some points he would start driving down the noise and then he would go apparently back into deep sleep, and it didn't really matter anymore. And so he became interested in the phenomenon, and he checked it out with motivational studies--in motivational studies with people sitting there hearing conversations, I guess, between nurses and supervisors. He published a study, in which supervisors were giving nurses' feedback, and it would get staticky and the nurses would have to adjust the--remove the static by pressing a button, and they actually had two buttons, so one had to do with the clarity, and one had to do with the intensity. And when the supervisor was giving a bad feedback they would go slower on the intensity. I thought that was hilarious. But at any rate, Lindsley subsequently published a couple papers in *Science* on this, and one was very interesting to me, and has evoked some interest in the pediatric surgery world when I bring it to their attention. Namely, that when somebody was fitted with these earphones and the noise in there, and they were going under anesthesia, they would press, and press, and press, and then their loss of awareness would be tracked by the diminution in pressing, and when they weren't pressing anymore they would be "out." And the anesthesiologist was also checking reflexes and so forth. This was before surgery. After surgery, what was interesting was people started responding to reduce the noises in their ears before their eyes were responding reflexively to lights shown in them. So they were actually coming out of anesthesia or coming out of this coma on a behavioral level before they showed any reflexive signs that are used by anesthesiologists to indicate that they were out of this coma. So that was the Science paper in 1957, and I thought that was really interesting. And at Rutgers there was a--incidentally when I got to Rutgers, Peter Nathan was there, and he had worked at Harvard with Lindsley in that hospital, and he had a conjugate programmer, and so I--and he was gonna loan it to me, but we never got around to it. But he was very supportive in getting me a lot of reprints. And these things, like, sort of follow you around. It's a really small world phenomenon. Well, at any rate, Lipsitt happened to be talking to this class, and I happened to be sitting there, and I was thinking, Oh, isn't that interesting. When I was in my third year of graduate school my first husband, David Rovee, got a job at Johnson & Johnson. He was a year ahead of me in graduate school in biology, and so I came to New Jersey in my third year of graduate school. And I was--I hadn't taken my prelims yet, but I had collected my doctoral data. My last piece of doctoral data was collected the day before my child was born, which was awesome, I'm lucky--you know, very lucky. At any rate, I was studying for my doctoral exams, which are like prelims. At Brown, you took them before you could present your dissertation research. And I was studying really hard. It was four days of exams on four topics, and closed book, and I had to go up to Providence for this, and my son, who was a wonderful baby, had colic, and he cried, and cried incessantly, and I didn't have any money to hire a babysitter or anything like that. My sister-in-law had given me a highly detailed mobile, and when I shook it, it distracted him, and he would watch it and he would be quiet. The only other thing that would keep him quiet was if I drove him around the block over and over again in my '62 Volkswagen Beetle, but you can't study very well--and that would put him to sleep, but as soon as it stopped moving, he would wake up again. So that wasn't very conducive to studying, so I'd shake this mobile and study. And finally one day my--I realized what my--I remembered what my grandmother--and I've told this story many times--from east Tennessee said, "Oh, darling, if you could only harness the energy of a two or three year old to run the windmills in Holland," and I thought that's it, let him do it himself. So, I had on a silk belt on my dress, I untied it, tied it from the mobile to his leg, and let him kick to move the mobile. So, every time he squirmed he would start activating the mobile, and he didn't have to move at all, and it would be totally silent, or he could have it really going, swinging back and forth if he was really kicking. And it occurred to me maybe a day later, this is conjugate reinforcement, isn't it, because it can go from zero to 100%. And so the reinforcement, and it would keep him occupied, and he could sort of shop for what "he felt like at the moment." So he could make it just swing a little, or he could make it go a lot, and that would keep him occupied and happy, and I could study in the next room and hear him go thump, thump, thump in the next room until he fell asleep, and he woke up again, thump, thump, thump, and it was wonderful. So I decided that this was learning, and so I thought, Well, maybe I'd better take some data, because you have to take data, and so I tried hooking it up, attaching the ribbon, detaching the ribbon, attaching the ribbon, and kicking always went up or down, depending on whether the ribbon was attached or not. And so I called Lipsitt and said, "You know," I was just talking to him, and I said, "You know, I think that this is an example of conjugate reinforcement. I think this is learning. He's only six weeks old. But they're not supposed to learn at that time." And Lipsitt said, "Get a group, get a group," he yelled into the phone, and so I said, "Okay." And so I started the first study on conjugate reinforcement. But because Benjamin had his mobile since birth and by the time I got started he was eight weeks old, I had to wait until--you know, I had buy mobiles and give them to other kids and wait until they got eight weeks old so they'd all be the same--they'd all have the same procedure. And they all learned, and they all extinguished, and they relearned, and I could get sessions as long as 15 minutes with a three-minute baseline that would go sometimes up to 45 minutes with extinction and reacquisition, and re-extinction, blah, blah, blah. And so I did pass my prelims, I did try to write this up and publish it, and I thought I'd publish it in--maybe in *Science*, because it was so remarkable, because babies weren't supposed to learn at this young age. And Harriet Rheingold had published something with Walt Stanley in *Science* on visual exploratory behavior in infants in which they touched a globe, and, she was measuring globe touching. Every time they touched a globe it would produce visual stimulation of different images and so on. Well, she could--you know, this was three month olds or something, maybe they were older, but I could do that. And so I sent it to Science, and I guess over--I sent it in originally in '65--over a three or four year period it kept being rejected, and the reviewers were saying, "Well, you need an arousal control," or, "You need this control or that control." And I ran all the controls they wanted. Piaget was the biggie who said they can't do this until they're six months of age. He had hung a “mobile” object over the bassinet of one of this children, and he had said--he had noticed that when the baby moved the “mobile” moved, so the baby moved a lot, and he'd interpreted it as an elicited joy reaction, the movement of the “mobile” made the baby move, and so it was important to have an arousal control. No studies of infant learning before that time had ever had an arousal control. I had an arousal control. Studies of infant learning up to that point in time had been four minutes a day at most--and maybe two minutes of that was learning. They thought babies couldn't learn, but they certainly couldn't learn in that brief period of time. Whatever. At any rate, I ran arousal controls, and I ran arousal controls, and I ran even more. Every time *Science* editors or reviewers would send the paper back saying, "These are interesting data, but you need to check out this, or you need to check out that." And eventually I had it all done, and the reviewers said to a person, "These are wonderful data, but we don't believe them, because Piaget said babies can't do this." And that was a very interesting experience for me. Four years it took to get this thing finally published. Finally I sent it to the *Journal of Experimental Child*, they published it in 1969, and that started like studies, but not for a while. But it was interesting, and it taught me my first real lesson; namely there's politics in science, and if somebody says something can be done if the zeitgeist is against you, then you're gonna have trouble publishing it. And may I say that every--there hasn't been one publication I've ever had--or article I've ever submitted--that hasn't been a fight to publish. Everything is a fight, even today my most recent submissions have been fights, and rejected, and rejected and rejected. And they said, "Oh, this is old-fashioned, or, nobody believes it," but--and we'll get into that when we worry about the field. But that was, that was an important lesson, just because somebody says something can't be done doesn't mean it can't be done.

Rovee-Collier: --other things to say.

Gerhardstein: Okay. Very good. So we'll just--

Rovee-Collier: So in continuing about the research, does it evolve in a linear way? When I finally was hired through a series of adventitious things (and I haven't said where I've been, but I will do that at some point in time)--when I was finally hired at Rutgers I taught Comparative Development of Behavior, and I taught an Animal Behavior lab, and it seemed natural that I should do that, because I had studied rats, and puppies, and infants, and I was very interested in comparative development and animal behavior. And in that lab I--it was hard to arrange laboratories that students could do, and so one of the things I was going to do was run a study on imprinting with chicks. And because baby chicks--you know, they don’t have to have a mother to nurse them, they come like prepackaged, and apparently they imprint shortly after hatch on the first moving thing they see when they dry off, and so I thought that that would be a good experiment for the students. And what we were doing was testing three-day-old chicks, and Walker and Gibson had published a study in which they hung geometric figures on the cages, on the sides of the cages of--I guess it was baby rats. I think it was rats. And they asked if being surrounded by these stimuli would influence their discrimination later on. So I was interested in asking whether chicks who had been housed in cages since they were hatched that had geometric figures on two walls, and there were the same geometric figures on both walls when you tested them in an open field (which has squares marked off in the center of it--well, the whole field is marked off with squares) and I asked if you lined the activity box walls with the same figures that they'd been reared with, would they approach these figures and stay near the wall? And would that--and then the next question was, well, would that make them feel "more secure" and would they then explore or traverse more squares in the cage, or would they just hang near the wall, would they hang near the two walls that had the figures, or would they go just to the other walls and the figures didn't matter and so forth? And so basically we were looking at perceptual learning and imprinting, and the question was, how many squares would they traverse, would they go to the wall, blah, blah, blah. Well, I got these three-day chicks and we all sat around the open field with our pencils poised ready to count the squares, and I leaned over to make sure the--I'd start the chick in a central square, and I had to make sure its feet weren't touching a line or anything so we could count the squares. And I put the chick down and I leaned over it and looked, and then--to make sure, and then I let go of it, I released it, and we sat back ready to count the squares it crossed, and eight hours later the chick hadn't moved, and it was just sitting there frozen like. And we tried to--and these students had other classes, and they'd sneak out and then they'd come back after the class and the chick was still there. It was really--we were all hungry, so we tried to spur it into action, we'd sort of--our behavior disintegrated, and we--the students were throwing like pieces of wadded paper at it, and erasers, prodding it with a pencil. It still didn't do anything. And the next chick did the same thing, so I thought this wasn't useful, so I imposed a five-minute limit on the test, thanks--the students were very grateful for that. But I mentioned this to Dick Lore, and he said, "That sounds like tonic immobility," and told me Gordon Gallup had written something on that and had been studying that recently. He was an animal behavior type, and he said that Salzen had written that you weren't supposed to be able to see that before ten days of age. And oh my, this is--these are 3-day-old chicks. So to make a long story short, I went to the literature and discovered that the way you induce tonic immobility usually in adult chickens is to flip them on their side or back and then they lie there--the other word for it is death feigning--and you've all seen that with possums, death feigning--or mice death feigning when cats bring them in, all species do it, and the problem was that the old method--another old method that had been abandoned--was placing a chick down and tipping the head while holding it down on its stomach, which I had inadvertently done when I leaned over. I applied pressure on the chick when I leaned over to make sure that it wasn't touching any lines in the square, you know, box. And so it turns out that scientists subsequently, when they were looking at this phenomenon developmentally, had not used that technique, because adults didn't do very well when they were just held down on their back, so they didn't use it anymore. With the babies they'd only flipped them on their sides or on their backs, and they didn't show tonic immobility until they were 10 days old. Well, that was a true phenomenon, ten days, I tried it, that's correct. But it turns out that when you flip a very young chick on its side or its back, and I've learned a lot about this from a guy named Kovach, who studied reflexes in chicks, and I had to read and try to figure--try to figure out this problem, why did this happen? When you flip a chick on its side or its back, it elicits a righting reflex, which is critical to the chicks being able to peck their way out of the egg, and when they peck on their back they have to be able to right and stand up, and so--and this righting reflex disappears between nine and ten days of age. And so the problem was that the induction procedure that we had used for immobility competed with the righting reflex, and so 3-day-olds could show immobility, but not in that manner, not if you used that procedure. And so this was an important--another important insight, that is, that procedures that are used with adult organisms may not be appropriate for use with immature organisms. And so when somebody says they don't do this, or they can't do that, or they can't do the other, they can't show fear and tonic immobility (was supposed to index fear). Well, it may or may not be, and that's what a predator does to a chick is turn it--or a chicken--is turn it on its side in its mouth or put it on its back, and that chicken plays dead, and what that does is it removes the cues for the predator breaking its neck. If the chick's limp or absolutely quiescent, movement cues are gone, and the predator may not kill it. And that's where the survival value comes in. Well, baby chicks can do this, but not with the adult method. So whenever you say infants or immature organisms can't do such and such before a given age, that is like a red flag to me. And you never say that, and I don't let any of my students say that, and actually I might kill them if they said that, because you don't know if something can't be done, because you haven't devised a way, a better mouse trap, to see if it could be done another way. And all you know is that according to what the procedures and parameters that have been used thus far, it can't be done. Andy Meltzoff ran into that with his imitation stuff. It’s a very long wait to see the infant sticking out its tongue to an experimenter sticking out his tongue at a newborn, and people all the time would say, "Oh, we can't replicate that." But Andy told me it may take two minutes and--because their reflexes are so slow, you can see the tongue pushing out the cheeks and so forth, and finally it starts protruding like toothpaste being squeezed out of a tube. But people wanted to see it right away, and adults do it right away, but babies don't do it right away. And so you have to allow for temporal factors, and you have to appreciate them and other factors which may influence your experiments. And this is very upsetting--was very upsetting. Subsequently, I mean, when people say you can't do this or that or the other--and I had a horrible time--people who study infants don't know about my secret life studying chickens, but I have lots of publications with chickens, and they have influenced me, because people don't know that, for example, the problem of a newborn of any species is to grow, and put on weight, and become fat and independent, mammals in particular. And sometimes things that you ask them to do in an experiment are things that compete with the problem of growth. For example, mammals don't thermoregulate initially. Neither do chicks, incidentally, until they're 21 days of age. They don't physiologically thermoregulate. They behaviorally thermoregulate by changing their posture. Humans don't thermoregulate physiologically either. Brain mechanisms that control body temperature automatically more or less don't kick in until sometime between four and nine weeks of age, and so they're not going to do things that involve a lot of energy or require a large caloric expenditure. And so people tried some conjugate things with newborns. They didn't do it. Well, they said, "Newborns can't learn. Four-week-olds can't learn. They can't learn the conjugate problem. They can't learn." But the problem is, again, if you appreciate the energetic relations going on here that are involved in this problem for these babies, the kicking activity is very costly and energetically expensive. And these babies are generally quiescent, in fact, preemies sleep a lot, newborns sleep a lot. Why? Because that raises the threshold for--increases the threshold for responding to external stimulation, which allows them to conserve calories. Well, they don't do it on purpose like that, but evolutionarily it's been selected that way. And so what these people didn't appreciate was, it wasn't that babies couldn't learn, it was that the task is competing with this other, more critical problem of conserving calories and so forth. And so you have to find a task that does not compete with this, just like the righting reflex must not compete with death feigning. And so I think that one of the things that I've been most interest--one of the problems I've been very interested in is early learning and the extent to which you have examples of these energetic relations contributing to what infants will show they learn. And I've also been interested in the--because I have an evolutionary point of view through my father and through my husband, which we'll discuss--I've also been interested in the notion that very young babies take so long to habituate, and is it that they are slow in learning? Well, babies who don't persist are babies who aren't "gonna get theirs." And so it may be that slow habituation in the very young is an adaptive mechanism. Also they cry, and they cry, and they cry, and they cry and ultimately the mother comes and gives them a bottle, and you'd see that the babies would be very quiet, but if the mother doesn't come in, they don't stop crying. If they didn't persist in crying the mother would say, "Oh, it must not be important," and she wouldn't come. And so persistence is something--learned persistence is something that's very important for the very young. They can't go get it themselves. If they're not able to ambulate or locomote on their own, then they need somebody to do it for them. And so I see habituation or learning differently--you have to ask what's the function of the behavior, and what does it buy you, and a lot of people don't ask those questions. And so I think that that's an important consideration, what does it buy you, what does--why is this behavior useful, and you have to ask not "why is it useful today," but "why was it useful in hunter-gatherer societies," because that's--all the behaviors infants exhibit are historical, and they were useful "back then." So you have to ask what was it, you know, the niche when the--when selection occurred--selection doesn't occur for behavior, but selection occurs for individuals who exhibited certain behaviors. And that's something that people don't realize and don't think about. I think about that all the time. I have a very functional view. So I think that in talking about the evolution of a career, one things lead to another led to another, I followed up on the conjugate reinforcement stuff, because I was so mad when John Watson wrote an MFQ article in 1972 called "Smiling, Cooing and the Game." It's a wonderful article. And he said babies--he said that babies didn't smile or coo until, like, the third or fourth day of contingency learning. He called the first three months a period of "natural deprivation." And so I was very upset by this, because I remembered that babies in our studies had laughed out loud, they'd belly-laughed, sometimes from the first day, when they were making this mobile move. I have videos of babies going, "Ah, hah, hah," before they'd kick to move it, and he said they didn't do this. And he said the reason they didn't do this in our studies was because they didn't really appreciate the contingency until after a while. But how did he know this? He interviewed mothers after a two-week study in which he had mobiles over their cribs, and the mothers said yeah, they started to smile and coo in about four days in. Well, this isn't really good, and they didn't have conjugate reinforcement set-up which seems to facilitate this. And he also said that there was--when babies kicked there wasn't--the mobile kept swinging in our studies and so there wasn't a good correlation between kicking and mobile movement. However--and we followed that up somewhat later, because I couldn't believe that either. But the cooing really--and the smiling really upset me. So we ran a study with Jeff Fagen my first Ph.D., and I was very fortunate to have people come my way at the right time, and we measured behavior and negative vocs, positive vocs, attention and kicking over minutes over four successive days, because Watson had said four--third or fourth day was when they first started it. And we did find evidence of this emotional beha--affective behavior from day one. But more importantly to me we found that from day to day their behavior increased. Not only did it increase their responding level, but it started off each day at the level it had ended up in the day before. And when we put the learning curves, day one, day two, day three, day four on top of each other you could see them going up, and you could draw a horizontal line from the end of one day to the beginning of the next day, and that was very interesting to us. And at that point in time we were using a fairly--I'd cut it down to nine minutes of acquisition--three minutes of baseline and three minutes of non-reinforcement at the end, and the idea of the non reinforcement at the end was not to extinguish behavior, but to find a period of time in which you could actually measure behavior when it wouldn't go down, because they have a response elbow when they detect the withdrawal of reinforcement they initially start an increase in response, that old college try, try to make it work, and then they'd decrease. And the mean of the three minutes, I wanted--it was very fortunate to select three minutes, because we knew total extinction took longer. Three minutes yielded the same rate as their last three minutes of acquisition. And so we could measure during a period when additional reinforcement wasn't influencing their response rate. In other words, it wasn't increasing or whatever, and that's something you learn studying animals. You don't measure behavior--you try not to measure behavior during--not during reinforcement periods and call--and have that add into the cumulative record. Skinner always had his animals have to stop, get the pellet and then go back, and they couldn't press and eat the pellet, because they had to hold the pellet with two hands, and so--two paws, excuse me--and so we wanted to be able to compare test performance with the baseline and ultimately we could compare it with the three minutes of non-reinforcement at the beginning of the next session and see that the performance at the end of one session and the performance at the beginning of the next one were exactly the same. So Fagen and I discovered long-term memory--what we published in 1976, this 24-hour retention, but started that study for a totally different reason than we ended up with. We started it to look at when cooing began: We--and that's why we picked four days, and we ended up looking at learning over four days. But then we had a little glitch in the--a little glitch, because I'm very conscious of experimental design from my history, and so to make sure that there was generality in the data and that it wasn't totally stimulus specific, that we used one mobile all the time, always used two mobiles and half the subjects got one and half, the other. And so we can counterbalance it and make sure that the same results come irrespective of the mobile. And that's something that a lot of people don't do. They only habituate to one stimulus and test with another one, and you really have to counterbalance--you really have to use two things to look at the generality, use two stimuli always. Anyway, because I'm not very clever sometimes, I was running subjects, and Fagen was running subjects, and there were two mobiles, and I had to run this baby, and as I was going to the house I picked up the wrong mobile, and so the fourth day, the "big day," instead of testing the baby with the mobile he'd been trained with, I tested him with a different mobile. I didn't know it. What do I know? I thought it was the one I'd had the day before. But the baby, instead of kicking like 25, 30 kicks a minute in that three-minute non-reinforcement period, lay like a rock, hardly moved at all, you know, basically looked dead. And I said to the mother, "Is this baby sick? Is there something wrong with the baby today?" And she said, "No, he likes your new mobile." And I said, "What?" And she said, "Well, see, he's looking at that little object over there--the bear holding the metal hoop." I said, "Wasn't that there yesterday?" She said, "No." I was thinking, "Oh my." And so that started a whole new--another new line of research, but maybe it was an error, but maybe it was advantageous, but we followed it up. You have one baby, get a group, and so we ran babies three days and then tested them with a novel mobile. And the babies didn't respond, and we discovered they just--they demonstrated that 24 hours after the last training day they could detect that the mobile was different. And what was interesting is that some of them did it right within, like, a minute or two, some of them took seven minutes. Can you imagine a baby lying absolutely quietly looking at a mobile for seven minutes? And then immediately they started kicking at the rate they had ended up with on the day before. And so they weren't relearning, they were simply--had a behavior that competed with the active kicking response. And what was interesting is that when you averaged across the group, it looked like the day-one curve of gradual learning, but what it was was different. Infants were coming in, as it were, showing total responding at 100% their level of the day before at different points along the way, and so by the end of the session, everybody was at a high level. And this is also something that I tried to--you know, that you have to appreciate and you have to understand--and you try to tell your students, is this a step function? Or is this a gradual function? Is it an averaging effect over individual step functions? I--you become acutely aware of these things when you've seen it operate. Well, at that point because, as I say, I'm not smart but I am sharp, but I do profit from my mistakes, we wrote up the paper about 24-hour retention, sent it in, and talked about them being able to discriminate--after 24 hours--the training mobile from a different one. Well, of two reviewers--one reviewer said, "Well, the learning stuff is interesting, but you should throw away those day-four data, because that's not very interesting." The other reviewer said, "The learning stuff is worthless. Who cares? But what's interesting is the day-four data." So this is an experience that every--all new investigators, all young scientists encounter constantly, and it goes on, I'm sorry to say, throughout a lifetime. And what you as an experimenter need to do is decide what you think is important and what you think is not, and go with your own instincts, you know. If they think it's not an important point, examine it. So ultimately it was published. We acknowledged both. But it started another question. There are two questions that we followed from this. One is how long a delay between training and testing do you have to have before they don't detect it--before they've forgotten what the original mobile looked like. And now they're not showing a discrimination response (behavior) in which attention and studying disrupts conditioned responding, and because three days seemed too long, we started our--we developed our standard paradigm of two sessions of training and a test session. Everything we do now is two training sessions in all of our paradigms. So we train them for two sessions with a mobile, test them one day later, test them two days later, test them three days later or test them four days later and so on with a novel mobile. And what we found was this incredibly interesting function that over time, their behavior *increased*. And forgetting as usually seen as a decrease, but here forgetting was an increase in behavior--such that by days three or four, they were responding as--just as if they were tested with the original mobile. And what were they (--but that was a forgetting function) and what were they forgetting? They were forgetting the specific details of their mobile so that the--after one day or two days, the specific details of the test mobile interfered with conditioned responding, because infants remembered that they were different from the specific details of the mobile they were trained with. But after three or four days, their really good responding--also incidentally on the original mobile too--was mediated by a different set of features, the gist or the general features. And people to this day don't appreciate the fact that you could have the same behavior at two points in time, and it could be mediated by totally different stimuli. And I think that's important to remember, just because it's--just because it walked like a duck and talks like a duck, it quacks like a duck doesn't mean it's a duck. It may be like a duck. And it may be something that's imitating a duck. You know? Or whatever. At any rate, there was lots of questions that flowed from this like how many objects have to be different in order to tell a mobile is different and so on, and this is a psychophysical question. And so all over--throughout my career I've asked questions that follow from the question before, and in the mid '90s, I had a wonderful lab, and we were no longer asking what infants learn, but what--of what they learned do they remember? How long do they remember it? And once it's forgotten, is it forgotten forever? And that's another thing that I--an idea we'll talk about later. But we also asked, well, are there developmental differences in how long they remember? And we had to develop a different apparatus to deal with older babies. And so my recent research has been incorporating new tasks, some of which are equivalent to old tasks, and it's interesting that they're equivalent, and then also--that's also altered my view of what is task-specific? And what is really the bottom line of what they learn? And more recently I've incorporated a deferred imitation task, and I've been very interested in perception and how infants perceive the stimulus, going back to my psychophysics days. And my success has come in large part from my funding, which we'll talk about, and having senior scientists in my lab who know about something I don't know about, and I try to select people who can teach me about something I don't know, like categorization or perception or whatever. And that's been a big boon to my education, which continues to this day.

Gerhardstein: All right. So we'll move on to a second topic area talking about your personal research contributions. And so what were your primary interests in child development at the beginning of your career?

Rovee-Collier: Well, I didn't have any interests. I didn't even like children. I'm not--I've come to like children recently. I like individual children. I don't think, Oh, isn't that a cute child. I think the behavior of children are cute--the behaviors are cute. But I didn't have any brothers or sisters. I didn't have any opportunity to engage in any activities with other children. I lived a fairly isolated life. There weren't any other children much in my neighborhood that I could play with. And so I didn't come from a large family, and didn't think about child development. I knew about behavior from rats, from running rats, and from running puppies, and so--and I saw infants like Trygg Engen saw them, and like Byron Campbell sees them, a really good preparation. Well, may we say that people are appalled by that term "preparation." But I think that is a very good descriptor, because you can take an infant who doesn't have language, doesn't have social task demands, and parent expectation isn't influencing them, they're not trying to "put on" for anybody, and they just do what they do. And that's the ideal--once you asked the question right, they're ideal psychophysical observers. Later, you can take what they do and ask, why is this important in the lifetime or at this particular time in a child, why do they do this and not this at this particular time in the life of a child, what other forces interact to make this behavior important, like persistence and habituation, or like early learning as we see it now, where they just pick up information really fast or associations between objects that they see together, and they can use it months later. Why do they do this? And what is the adaptive advantage in their learned behavior? But early on I didn't have overarching interests in infants or in children. I thought the field was really interesting (in the age that I grew up in) and the experimental child psychology course taught by Lipsitt used a book called--by Lipsitt and Palermo called what—*Readings in Child Psychology*. Wonderful experiments about applying principles of what had been discovered with animals, socialization, learning, magnitude of reward, for example, deprivation, all about applying those principles to studies with children and infants. And up to that point in time (up till the mid 1950s) children were to be seen and not heard. But then Dale Harris gave a very important presidential address (I guess it was the Society for Research in Child Development) in which he argued that children should be of interest because they have different ways, and developmentalists needed to see if the general laws of behavior that have been found with adults and animals apply to children as well. And that started a whole intense movement to apply the principles of learning to children, and I got an earful. I got a very strong appreciation for experimental child psychology in that course with Lipsitt, and I still have my notes, and I still have my books, and I still read them and think that those were the beginnings of a very important age in terms of how we think about how to ask questions of children and infants.

Gerhardstein: So the next question is what continuities in your work are the most significant in your view, and what shifts occurred, and were there some kind of identifiable events that were responsible for any kinds of shifts that occurred? But really more focused on what's the--what are the major continuities in your work?

Rovee-Collier: Well, I've already elaborated that one thing leads to another, leads to another, leads to another, and I've tried to remember the golden rules of science in what I've learned from previous experiences, and applied them to designing future studies. And I guess the--I've already been through the mobile thing, and the mobile is what I guess I'm known for. I don't know. But I'd like to be known for more than something you can buy in a store. I'd like to be known for the principles that I discovered along the way. But of all the lessons of life learned through these early experiences with the chickens, and the mobiles, and trying to publish, and being rejected, and reviewers not liking this or that being aware of the timing of successive events has been really important and influenced my current thinking. And again, trying to apply principles, continuing Dale Harris's thing and trying to apply principles from--that have been totally ignored, but that I think may be important--from learning studies with adult animals and humans that seem to be important and applying them and seeing if they're important to infants or not, such as the role of context. Is infants' behavior context-dependent? Literature with adolescents talks about context dependence. A big literature on conditioned drug tolerance talks about context dependence. The session--you know, on the couch with a psychiatrist may be different when you're feeling good about talking or revealing your personal problems, and then you go out to the real world, and that's a different context. And so I've been very interested in the role of context in infant behavior, and asked those questions very early on. And it turns out that's a big deal. It turns out, it turns out that it makes a big difference where and how you test babies, and even who's there when you test them. If you have a novel experimenter doing the test, that is a novel social context, and you get different data than if you have the original experimenter or someone they've seen before. And I think that the questions that I'm asking now are basically the same questions I started asking, except they're much more sophisticated. We've asked questions, and are thinking about things that others, who study infants, or even children, haven't really thought about yet. For example, can you modify memories or distort memories? There's a big deal about eyewitness testimony, distorting memories. Can you distort an infant's memory? Their memories are so specific for what they learn. That's really bizarre, and why would a memory be so specific 24 hours later? Change more than one item on the mobile, and they don't recognize it. Test them at six months in a room in their home other than the room in which they were trained with the same train set in our apparatus that we developed with trains, pressing a lever and make it move around the track. Put them in another room in their home, totally familiar room and they don't seem to be able to do the train task. Why is that? That's really bizarre. You know? And so I became very interested in eyewitness testimony, and false memories, and I'm still interested in that. What is the basis for false memories? And so we had a paper in 1993 called *Infant's Eyewitness Testimony*, in which we teach them with one mobile in one context and introduce another mobile, like interrogating a child about what they saw before, and discovered that up to four days later, simply showing them the novel mobile will affect their recollection (recollection, that's the tainted word here)--their *retention* of what they had learned; they no longer will recognize the original mobile. They recognize the new one. And if you show it to them six days later, it doesn't have any effect. So you start thinking about when--the timing of--the so called interrogations occurred, and is the effect of the interpolated activity, in which something interferes or competes with information in the original memory--is that a permanent effect? And you discover that after they've forgotten the details of the original event it is permanent. Thereafter, they'll only recognize that new one and not recognize the original one. And once they've forgotten you can prime (which we'll talk about later) with the new cue and it works, and prime with the original--something from the original event, and it won't--it won't recover the memory. So that's an interesting thing about updating memory, and it must be a general phenomenon. That phenomenon is seen in all animals, and you can see up to a certain period of time it only interferes, and eventually the interfering stimulus is forgotten, and they recognize the original one again. Interference may be a recency effect, and over time a primacy effect wins the day. You could look at it like that. That's an interpretation that many people have used for other phenomena. But you need to study things over time, and people usually do one shot studies, because they bring babies into the lab, and it's hard to get babies to come in for many sessions. Mothers aren't responsive, babies get sick, babies sleep, blah, blah, blah. If you go to the homes, you switch the problem to your own *in*convenience, sit around and wait for the baby to wake up, and so forth and so on, and you have a much greater participation and response from the mothers, because they don't have to get the baby up, get it dressed, take the baby to the car, etc. In fact, they may do their laundry while you're there. So you serve the social life of the parent a little bit, but in the meantime, it's really important to do multiple sessions over an individual, and to try and track a phenomenon so that you totally understand it before you try manipulating it. Recently, there have been people who've started to do multiple interviews about an event children witnessed, either immediately after the event, a while after the event, very late after the event, etc., or they've given them different numbers of interviews and so on. They find all of these things make a difference in what the subject ultimately reports, and if they report the false information, if there's a distractor inserted into the information during the first test later on, they remember the distractor as part of the original event. And that's very interesting, but their memory may be very accurate if they don't have false information during the interview. So these all are very interesting questions. And I don't think that there've been any--I don't think there have been any strong deviations along the way. I'm very boring. I just pursue, pursue relentlessly what I think are important questions, and they may not be popular questions, and they may not be questions other people are looking at at the moment. But eventually they'll catch up.

Gerhardstein: All right.

Rovee-Collier: That's a very bad way of looking for an explanation for why I can't get my papers published: “Eventually they'll catch up, see that it's important.”

Gerhardstein: That's good. All right. So next, the--the next question's please reflect on the strengths and weaknesses of your research and theoretical contributions, the impact of your work and its current status.

Rovee-Collier: One of the things, I guess, is the early learning--I'm seen as a learning person; I may be a memory person, because after all you have to--they (infants) need to have something to remember, so you have to teach them something, so later you can see what of it they remember, and what influences what they remember. It's not important to me if you can learn something, if you can't remember what you learned in the first place. So that's not a very useful activity. The problem is that a lot of people don't believe what we find. Like they didn't believe the mobile stuff because of Piaget. Then, he said it couldn't be done. And then, they didn't believe that infants could be classically conditioned in the paper that I coauthored with Arlene Little and Lipsitt, because we didn't have a 500-second interstimulus interval between the CS and the US. But babies take longer intervals; like what was appropriate for adults, 500 milliseconds, isn't appropriate for a baby. They do really well with 1,500 milliseconds, and by the time they are toddlers, maybe 500. But recently, studies of conditioning, for example, out of Mark Stanton's lab and Dragona Ivkovich have found that, you know, seven or eight hundred milliseconds is really optimal for five and six month olds. Well, this--there's a literature with this, but it's not that they can't be classically conditioned. It may be that the interval that people have used to study it isn't right. So there were a whole bunch of papers in the '40s that said--and '50s that said babies couldn't be classically conditioned. All these failures, and Kranogorski said babies don't have--the cortex isn't sufficiently mature till six months of age, so they can't be classically conditioned before that time--another big guy saying, "They can't." But in the late '70s, my husband--George Collier, who was chair at Rutgers (he was one of Estes' first Ph.D.s and a work-study student with Skinner, if you can imagine), had suggested to me that one of the people in the Rutgers College department who had left and gone to Binghamton, SUNY Binghamton, had discovered a phenomenon called "reactivation," which involved babies or young rats--baby rats--rat pups--being given a reminder after they weren't showing any evidence of learning after a really long delay, and the whole memory came back the next day. Oh, this was amazing, because we had been looking at forgetting functions, and people said, "Well, they don't," you know, and P.S., up to that time the people who studied visual recognition memory had said babies could remember only a few seconds, maybe minutes at most, and we were showing they were remembering up to like 14 days at six months, and about a week, a little less, at three months. And so, you know, all of these things are difficult to publish when you are using a different procedure and people say, "Oh, that's just action memory, you gotta understand that it's different from what we're studying, because we're studying real memory, and you're just studying something a cockroach could learn." You know? Or even--what was it--even a planarian could learn--something Jean Mandler said, "A lowly earthworm can learn to be conditioned." And so, obviously, that they could remember for so long made sense, because after all, elephants never forget. Skinner's pigeons on the project they had during World War II could remember for 14 years, so it makes a lot of sense that young infants could remember a conditioned response very long. But I mean now it's a big difference, minutes or seconds versus days or weeks, and so it turns out when a memory is forgotten, or they're not showing any evidence of their prior learning, that that memory may still be there somewhere. And Skip Spear--or Norman E. Spear, had found that giving a reminder, you could recover it, and so we go, "Well, maybe, you know, we have forgetting functions at six months after 14 days. We have forgetting functions at three months after like a week since they forgot." Maybe if after they are not showing any sign of retention we want to wait a good while, maybe wait a week to make sure they're not showing any (if there's a straggler comes in, he's in that group too, he's forgotten), then give a little reminder and maybe if you test them subsequently, you'll see that they remember again, as Spear did. The only thing was that he was doing fear conditioning with the rat pups, and he was giving them shocks, and looking at avoidance or escape, and it wasn't very likely that mothers would let us use shocks with their babies, and so we had to figure out an appetitive analogue with the mobile procedure that would mimic the procedure that Skip used. And it was total chance that my parameters--I picked my parameters because they basically matched Spear's parameters. For example, he was using an active avoidance procedure, and so he wanted to make sure that arousal induced by the shock wouldn't lead rats to run to the other side, the safe side of the shuttle box. So he waited 24 hours before he tested so that any arousal affects would have dissipated. Well, so we gave the little reminder, he'd used his shocks, and we said, "Well, what's a reinforcer, mobile movement?" So we used mobile movement and waited 24 hours and tested the memory. It was right back, they were kicking at the same rate they had kicked right after training, and I couldn't believe it, and they re-forgot at exactly the same rate they'd originally forgotten. So the memory a week later was recovered and reset in essence, and it turned out later that was published in *Science* in 1980, but I presented it at the Developmental Psychobiology meeting in 1978, and it created quite a stir. And then Byron Campbell at Princeton said, "But look, they go up after two or three days, and then they go down. Why are they suppressed 24 hours later?" I said, "They're not suppressed 24 hours later." He said, "Yes, they are. Their response is lower 24 hours later, then they show a peak the next day and then they go down." I couldn't stand it. So we ran out and--the answer for me is not arguing. I have never argued a point with a reviewer. Just go collect more data. That's the way to do it. That's the way to spend your time. If you--if someone else is challenging your data, like the *Science* reviewers, collect more data--it showed that they were wrong, and they still didn't believe it. But at any rate, that was really quite astounding to us, and we collected more data and found out--it wasn't really suppressed. So how could it be suppressed when they responded at 100% of where they were before? That's not suppressed. They're just higher. It turns out that the reactivation process is sort of time locked or time dependent, and maybe a few hours after reminding you see a little rise in responding, it goes up, and up, and up, and finally 24 hours later, they're at their peak, but then they go up a little more. That--but what happened was it was chance that we waited 24 hours because of Skip's shock and arousal in rat pups waiting 24 hours. If we'd have tested earlier than 24 hours, we would have never discovered reactivation. So a lot of decisions that you make are critical to what you ultimately find out. And reactivation has been one of my biggest contributions, and now everybody says, "Oh, reactivation, everybody knows about that." And later they were using reactivation which isn't a result of practice, it is renewed performance, it's just seeing something happen, but--and I emphasize that testing during a non-reinforcement period, but 15 years later very well known psychologists were still writing that I measure savings, and I never looked at savings. I always looked at behavior when they weren't reinforced--what they brought into the session with them, never savings. In fact, I've never found savings. And so people say, "Reactivation, reinstatement all the same thing. There isn't really any difference between them." Mark Howe was saying that in a paper with Mary Courage, and I guess--in 1993. So Byron Campbell of Princeton, who was the best man at our wedding and had written a lot with Norm Spear (they were collaborators on a number of early projects, but George Collier introduced them) had found the phenomenon of reinstatement early on, and reinstatement is procedurally different from reactivation. First, reinstatement is like throwing a log on dying embers and keeping the fire sort of burning, whereas reactivation is waiting till the fire dies out altogether, and then starting it up again. And so reinstatement is a periodic reminder while the memory's still active theoretically, and reactivation is a process in which you recover a memory that isn't ostensibly performed--a behavior that isn't ostensible or latent is still latent when making it active again. So we ran a lot of studies. There weren't studies of reactivation and reinstatement with babies, but Byron's idea was early on that early experiences are important later and if they keep getting this periodic reminder it can keep that memory going through long periods of behavioral development, and that is why early experiences are important. And I'm--my whole life has been--since I started studying infants, been interested in,, reporting the role of early experience for later behavior. Is early experience important? How is it important and so forth, that's the core question I'm interested in. Of course, we wondered about reinstatement, which keeps a memory going, versus reactivation, which everybody was saying was the same thing. So we did a number of studies, and it's not correct that they're the same thing. Reactivation works, but it is not nearly as effective as reinstatement, not nearly as effective--what do I mean not nearly as effective? Well, if you give a reactivation treatment, then the memory's remembered as long as it was originally. If you give a reinstatement it might be remembered--I don't know--ten times longer than it was originally. A three-month-old--or a six-month-old--who remembers for two weeks, after a single reinstatement may remember for five and a quarter or five and a half months as opposed to half a month again. It's really incredible--it's very powerful, even if it's given after the fire's burned out, even if they don't remember having had the experience originally, you give it after they've forgotten it, and you give them two or three minutes of practice where they could actually move the mobile or the train--it has the same effect. It's really quite remarkable, I--and so that says it all. And so I've been very interested in procedures that protract memory, and that brings me to the current interest, on which I'm back to the question of time windows, and if you have one session and the second session (we always give two sessions), and if you give those two sessions on successive days they remember for a certain length of time, but if you spread out those sessions, the question became when do they forget the first session, so they don't integrate the second session with it and treat that second session as if it were another first session? I mean, learning should have some advantages. A one-time event--when is something treated as a one-time event and when is it treated as something that's recurrent? If something recurs, pay attention to it. We found, for example, and I don't know if this is still--if this is true, that you can't reactivate a memory of an event that occurred only once. Now, maybe if it was a really strong event, or really enduring event and lasted a long time, maybe you could do that. I don't know. But within the parameters that we've used, if you have one session, you cannot reactivate the memory. You have to have two. But the first session doesn't have to be that long. It can be very brief, and when it occurs again, it can be very brief, as brief as 60 seconds in a different (imitation) task, for example. At any rate, this time window thing is very interesting, because two sessions could be integrated with each other, but how does that happen? If you think of the mechanisms, the first memory's retrieved, so you have to know how long the one event is remembered and when it can be retrieved. You retrieve it by presenting the stimulus again, it brings back the memory of the first event and puts the second event with it presumably in active or primary memory. And if you do it toward the end of the forgetting function and then at some point at which you can't retrieve it and they aren't integrated, and it turns out that if you retrieve the memory at the end of the time window, the last point at which you can just retrieve it, they remember it much, much longer than if they retrieved it earlier in the time window. So it doesn't have to do with retrieval versus non-retrieval. It has to do with <u>when</u> they retrieve it. And apparently the longer you wait--you know, there've been explanations for this in the adult literature. Bjork suggested that it was cognitively more difficult to retrieve a memory when more time had elapsed from the prior time it had been retrieved. And for whatever reason that may be the case, it certainly is true for infants as well as for adults. And it seems to be that the number of retrievals--every time you retrieve something they remember it longer the next time, but if you retrieve at the <u>end</u> of the time window, they remember it even longer, and this is a really major effect. And people, you know, they have repeated interviews with children who've experienced an event, somebody'll have a certain protocol and they'll re-interview them like one hour, or one day, two days, a week or whatever later, but you can't compare across studies, because they have different things they're remembering, you have different timing, you have different questions that are being asked. It's apples and oranges. You can't really compare. Is there some general principle that you can extract from all of this? The general principle is the longer the interval to the first interview, the longer they remember. The general principle is, if you don't introduce inaccurate information in the first interview, they'll remember accurate information later. If you introduce inaccurate or misinformation in the first interview, some of that misinformation will intrude later. But it's very difficult without a standard paradigm or a standard reference curve of how long they would remember at different ages, and particularly with language interfering. Kids can talk to their parents or rehearse it to themselves--it's very difficult to compare across studies. And I think that one of our major contributions has been my fortune to have some very good students and over the years we've looked at 3-, 6-, 9-, 12-, 15-, and 18-month-olds, and because we developed a second task that is performed exactly in the same way as the mobile task at six months, six-month-olds who kicked to move the mobile, six-month-olds will press the lever to move a train, there's absolutely no differences in any of the major factors or any of the variables, that affect retention in those two tasks, so we're measuring what they remember genuinely. It's a genuine retention measure. And we've, we've carved out what you'd expect them to do over 15 months of age. We've looked at their baseline changes over--excuse me--18 months of age, we've looked at acquisition curves over 18 months of age, and we have this standard reference curve down, which a large number of people--Tiki, Hartshorn and myself, Peter Gerhardstein, Ramesh Bhatt and a number of undergraduates contributed to. Peter Gerhardstein, and Ramesh Bhatt were my research associates at the time and greatly enhanced my knowledge. But those curves can now be compared with curves that are obtained using other procedures, changing variables and so forth, and you have something standard to compare against. You cannot do it otherwise. And the early--for example, the early insistence that six-month-olds are only remembering for two weeks because you're conditioning them, and because your procedures are so long, two days for a total of maybe 18 minutes over two days--but it turns out that a six-month-old can remember the two puppets that they saw paired with each other without explicit reinforcement. They can remember the association between two hand puppets that they simply saw paired for an hour a day for two days. They can--and then you ask at what point, do they forget that association. And they might have looked for maybe between two minutes and eleven minutes total at those two puppets if you measure their attention. And it turns out that they remember that association between those puppets for two weeks, even though they might have looked for a total of only two minutes. And so the--it doesn't have anything to do with conditioned responses that they remembered. It--seeing two puppets sitting side by side, there's no conditioned responses there; there's no reinforcement there, they're just there, those puppets are just side-by-side. Does this go with that? And they can remember that for two weeks at six months too. So that was sort of gratifying in the sense that six-month-olds' memory is about two weeks irrespective of task, and that's considered in our lab, that's considered a major finding. And we're exploring the theoretical contributions of early learning, the mobile task, the role of early experience, how reactivation and reinstatement can perpetuate the memory, the time window in which two events happen that can be integrated and protract retention, the notion that if you retrieve the memory at the end of the time window, it can be remembered much longer. We had a paper published in 2005 in the summer that showed that if they simply see a person demonstrating an action or a series of actions on a hand puppet that they normally remember that for one day, so one day would be the end of the time window. If they see it again (the demo again) for 30 seconds, which isn't long enough to produce 24-hour retention--60 seconds is necessary--they see it again the next day, they can remember it for a week. That's a big difference. Retrieving the memory at the end of the time window increases the duration of that memory for--seven times at least. So that was--that's interesting to me. And also, you can demo, you can demonstrate target actions on one of two paired hand puppets at three months of age, long before they can reach to pull the mitten off the puppet and shake it, which is one of the actions that's modeled. You can demonstrate that at three months of age and give them periodic reminders by just holding the demo puppet up stationary in front of them until they're old enough to actually pull the mitten off, and one day after the last reminder, they'll imitate on the other puppet even though they hadn't seen that puppet for three months, even though they hadn't seen what the adult did for three months. And so reactivation can be used as a tool to study what you're learning at an early age before you're old enough to show what you know, and it's kind of scary that information about what you saw for a brief period of time (60 seconds) is remembered for three months. That's scary. I mean, be careful, don't let them fall off the changing table, don't prick them with a pin. But--unless you do it a lot, and then they won't remember probably--but the idea is that they're like--babies are like sponges, and all that information comes in and is absorbed. And the question is, what happens to that information? That's why I'm interested in the problems of memory distortion and updating. Because information that may be appropriate when they're little may not be appropriate when they're older, but memories may not be forgotten. We don't know if they're ever lost, because--we only know you can't get them back, but we don't know where they are. We don't know if they're culled, or they're just under so many layers you can never get back to them, what. So the question is, how are those memories from early on kept going, and how do those early experiences affect later behavior, under what conditions can those memories be retrieved, and updated, and made useful for the future, and eventually who knows? You know, the infant may be remembering a lot of things from infancy. This whole thing of infantile amnesia may be a function of remembering things that have been modified so many times in between that they're not recognizable. But I think that the mobile task, and the door it opened for studying long-term memory, and reactivation, and the notion of the time windows have been really important. More recently, we've been interested in latent learning, what infants just pick up from the world around them by looking--spending so much time looking around when they can't do anything but look. Like with a four-month-old in the grocery line, and that kid stares at you incessantly, and you wonder, "Please, you know, what are you learning about me?" And it turns out that they're learning a lot, but they have to have an occasion and a means to express what they learn. And we've been involved with that recently.

Gerhardstein: So the next question--you really sort of covered it in talking about some of the other topics, but the question specifically is what published or unpublished work best represents your thinking about child development, which of your studies seem most significant, and which

contributions the most wrong-headed, if any? It might be useful maybe to actually just mention a couple of specific studies that pertain to some of the things you've been talking about to give some sort of timeline of actual publications of--

Rovee-Collier: Without my chicken studies, right? We'll leave them out-- But the very first study that I finally got published was by Rovee and Rovee, my first husband, in 1969 in JECP, and that was called *Conjugate Reinforcement of Infant Exploratory Behavior*. And before that time I'd been primary doing olfaction, and I did it a few times thereafter and you see the--you see the chicken stuff intervening in there. But then we did the study with Rovee and Fagen in 1976, also published in JECP on extended conditioning and 24-hour retention in infants. And that was the beginning of that. Then I guess there was a major chapter on the review of conjugate reinforcement with Marcy Gekoski, who was my second Ph.D., and that was published in Reese and Lipsitt in 1979, *Advances in Child Development and Behavior*, and I think that that--I interviewed Ogden Lindsley over the phone, or rather my husband, George Collier did, because he knew him personally. We interviewed him over the phone and got a lot of information about how he found conjugate reinforcement. And I don't think that any of that information may be published anywhere except there. And that has--was used in a lot of graduate courses. I know it was used by Eleanor Gibson at Cornell, and a lot of people--the next generation of psychologists had to read that. And that has a review of all of the literature, but it also asks questions about parameters, and preference, and so on. I thought that was probably one of the best chapters I've ever written. Then there was a paper by Sullivan, Rovee-Collier, and Tynes which my grant is named after--titled “A Conditioning Analysis of Infant Long-Term Memory,” and that was in 1979 in *Child Development*.

Gerhardstein: And that's a reference to a current ongoing grant that began?

Rovee-Collier: Yes, that began--that was my first grant on the conjugate reinforcement problem, although I had other grants before that, some with chickens. By the same token, this is the first one which is still going on, my grant is still called “A conditioning analysis of infant memory,” and I guess it's in its 38th year. And reviewers might well question if she hasn't figured it out in 38 years, why should we fund her again? But then there was “Reactivation of Infant Memory” in *Science* in 1980; Fagen was an author on that--Rovee-Collier, Sullivan, and Enright, Lucas, and Fagen. And then the “Organization of Infant Memory” by Sullivan and Rovee-Collier; and finally, the 1983 *Science* paper entitled--let's see if I can find it--entitled, “Memory Retrieval: A Time-Locked Process in Infancy,” with Fagen. And basically, those papers summarize my--the thrust of what I'd done until Hayne and I had a chapter in '87, again, in “Advances in Child Development and Behavior,” reviewing all of the work on reactivation. And then there are a couple of papers that I consider seminal, and one of them is a '95 paper entitled “Time Windows in Cognitive Development,” which was published in *Developmental Psychology*. It was a theoretical paper, and it basically introduced the construct of the time window, and my current grant is funded based on that construct. And it turns out that the--there are data on that from all species, and all tasks in all kinds of problem areas, and it's a fairly ubiquitous concept. And I consider it to be absolutely critical. The ISIS presidential address that I gave in 1996, which was published in *Infant Behavior and Development*, “Shifting the Focus From What to Why,” I consider to be another major theoretical paper. I think that was the best talk I've ever given. At least my former graduate student, Harlene Hayne, said, "Oh, that was really good," probably because I was hoarse, and she could hardly hear me. And then recently there's a paper that's in press right now but, well, there was a *Psychological Review* paper in 1997, which I also considered really important, another theoretical paper. I took all the data from the last 25 years in my lab and asked if infants showed the same functional associations as amnesiacs in priming and recognition. We'd test them for recognition with a mobile or a train a certain number of days after they were trained or we'd reactivate the memory after infants forgot it and the use the same variables for the reactivation test that affect the infant's recognition, and this was inspired by Skip Spear again, who called the reactivation effect a priming affect. And priming is a phenomenon used that's considered an implicit memory phenomenon in studies with adults. If you look over the literature in adults, you can find a number of variables that influence recall or recognition and priming differently. And those dissociations are modeled by the very same variable, like interference, which has massive effects on delayed recognition, but has no effect whatsoever on priming. With 13 different variables, one gets the same dissociations with infants that you do with adults. And because I did so much work, published so many studies, I had 25-plus years of data, and I had it on a variety of these variables.

Gerhardstein: This citation was that paper that's the '97--

Rovee-Collier: It's the '97 *Psychology Review* paper--*Psychological Review*, "Dissociations in Infant Memory: Rethinking the Development of Implicit and Explicit Memory," and that has shaken a lot of people. It subsequently led to the--a book that I wrote with Harlene Hayne and Mike Colombo of the same title. It was published in 2001, and is used in a lot of graduate seminars on the problem of implicit and explicit memory, except that Mike Colombo has developed chapters on the neurophysiological basis of implicit and explicit memory in adults--in human adults, and monkeys, and rats. It has a whole chapter on the developmental psychobiology literature from Mishkin and Bachevalier and it's quite detailed. I think it's--I think it was pretty good. It's used quite widely in Europe. But the most recent paper that I consider my best current paper is in *Psychological Science* with Kimberly Cuevas as the first author. She received a Sandra Weiner Award for Graduate Research from the International Society of Developmental Psychobiology, and she shows--it's a very contrived study, but she shows that infants can form associations between the memory of one thing, which isn't even present, they just have a memory of it, and a memory of something else that isn't even present, but if those memories are simultaneously activated in the physical absence of the stimuli so that they're simultaneously active, presumably in primary memory, then they are associated. And later one memory is usually remembered for a long time, and one's memory's remembered very briefly (the puppet imitation task--one day, and kicking the mobile to move it at six months, two weeks). Now, once they're associated, they can remember the second event as long as they remember the other one, the first one. They remember the short-lived event as long as they remember the long-lived event. And what's important about that is that classical theories of learning, every theory of learning currently, except for Gallistel's new theory, assumes that two stimuli have to be physically present in order to be associated, and that's true in all theories of memory for all various memory theorists as well. For example, Roediger has done a beautiful review in which he says associations are the core data of all theories of learning and memory, cuz memory is of what you've learned. The Kent-Rosanoff norms that are used in constructing study lists of words are a perfect example. They assume that salt and pepper, for example, which are high frequency associates, are associates because at some point people saw--always saw salt and pepper together. There isn't any challenge to this, and associations--well, Aristotle told how associations could be formed, and that was certainly long ago--but the notion of association and association between two things as being central to interpretations of all memory phenomena and having to be true for all learning phenomena is really, is really long lasting. And Kimberly Cuevas showed that for two stimuli that weren't even present, so you can have association between things that aren't even present and that guide future behavior. We've also got a study that shows that if they just see the puppet demonstration, which they can remember for one day, in the mere presence of the train, which they learned to move a week or two earlier and remember that association for very long--how long do they remember the puppet demonstration under those circumstances, when it's at the end of the time window for the train task, which is 14 days? Oh, maybe six weeks. I mean, *six weeks*! And if they--that's really quite incredible, and I have a comment on that later. But what information babies pick up when they're merely seeing things together, we have a--currently a dissertation that has found if they see two puppets paired one day, puppets A and B, puppets B and C paired the next day, puppets C and D the next day, and they see the demonstration (the modeling of the target actions) on puppet D and infants are tested with A, which was seen three days earlier--which they haven't seen for three days, and which they never saw in the presence of the demonstration, they will perform the actions on A. But if A and B early in the chain were not paired so that the link between A and B is broken, you see A and B equally long, but at different times of the day, and then B is paired with C and the demo is on C, then they don't imitate on A. So it doesn't have anything to do with generalization, it doesn't have anything to do with familiarity. It has to do with a knowledge base that has been built up. And they can go, you know, it's like reading road maps, it's like when I worked in Maine at Jax Lab, the big joke was "you can't get there from here." A driver asks a gas station attendant how to get to some other place, and he says, "You can't get there from here."

But you know, babies have all of these associations linked in their whatever, and they can exploit them, you know--and they can remember them days later and use them to solve a problem, even though the problem is way down in the chain. Now, that's really interesting.

Gerhardstein: All right. The next question is pertaining to research funding and the research funding apparatus. Please reflect on your experiences with the research funding apparatus over the years, and please comment on your participation in shaping research funding policy and implementation while securing support for your own work, and related issues.

Rovee-Collier: Research funding is increasingly important these days. The mobile is really cheap. I didn't have any funding for ten years. I didn't ask for any to do the mobile studies. I just bought mobiles, they were cheap, my apparatus is a ribbon, it's not automated. All I had to pay for was the mobiles, because it turns out you can't take a mobile away from a baby after they've had it for three months. It's cruel and unusual punishment. And so I had to buy those, and those got increasingly expensive, but I could handle it. And gasoline, that's all I basically--it basically cost me, and that was good for undergraduates, it's still good for undergraduates, except that gasoline has gotten really expensive if you run as much--many babies as I run a year, 700 to 1,000, each baby a minimum of two or three visits (depending on what protocol you're using) up to as many as six if you're using multiple reminders. It can be quite expensive to drive all of that distance. We work within a 50-mile radius of Rutgers, and I have to reimburse students for that. The apparatus is still pretty cheap. The problem is that a lot of people need research funds to get started up, particularly people in animal research, people doing more technologically advanced things than I do need touch screens, need eye trackers, need laboratory setups that are much more sophisticated, that require space, they have to bring parents in, blah, blah, blah. It's a tough row to hoe, which is an expression from when people used to hoe onions and potatoes in rows. But the research funding has become increasingly critical, and I've served on a number of study sections. For four years I served on the small grants--NIMH small grants panel, and for two years I chaired it, and that was really--I saw that as really important, because when I started, having to give $5,000 a year for two years got people going and gave them an opportunity to get the pilot data that they could then use to apply for their ROIs, which are much more expensive. Usually they require pilot data for those studies, those applications. But then I thought that was a really important thing to do, and we would have like 20 or 30 grants apiece to review, but I never regretted a moment of it, because I thought it was a really important activity. I also thought that--when I subsequently have been on a variety of other sections like the psychobiology study section, or I don't know what--it's changed names like five or six times--attention, perception, and cognition or whatever--all of those have been for ROIs. I've been on review panels for senior scientist awards. I think that the apparatus for the availability of funding is absolutely critical to today's researcher. You can't publish or perish without money to publish--to do the research. If you don't have the money, you can't do the research; if you don't do the research, you can't publish; if you don't publish, you're not gonna maintain a job in a major research university. And funding is getting very tight. It's unfortunate because there are more, and more, and more researchers out there doing important to think using new technical knowledge, asking really interesting questions. And the funds are getting smaller, and smaller, and smaller, and it's sort of the luck--these days it's sort of the luck of the draw. It's a very small number of people reviewing your grant. It's the luck of the draw, who's reviewing your grant. Do they think what you're doing is important by their own personal philosophy, with their own personal science with what they do, are you a threat to them or not? Usually, they try to have someone with expertise, but if you're doing something that is counter to that person--what that person is advocating, maybe despite the expertise, they're not gonna like it, and you'll get a low score. The low scores today, you have to have a priority score, they give priority scores of one to five now. It used to be the--it used to be that you could be funded pretty much with a score of two to a two point five. Now, you can't be funded much with more than a one point six, and then they don't even bother to review half of the grants that we had to review. And I think that's really sad, because the input from the reviewers to someone who's naïve or has really great ideas, but didn't quite have the sophistication for method, or doesn't quite understand the controls, for the disapproved grants or the two point sixes, two point sevens--in those days they profited a lot from our comments, and came back many times with highly funded grants. So I feel bad about that. But when I say "luck of the draw," study sections change from time to time, and I can remember--I've always had sort of a lock on the field, because nobody else was idiot enough to go out and test babies multiple times in their own homes. It was so much more convenient, and Al Caron told me this, it was so much more convenient to test babies in the lab. But I thought that you could get the best behavior in a familiar environment. Too many times, if babies come to Rutgers in the late '70s in the heat, in a car, in traffic jams, they're totally zonked by the time they get to the lab or are crying and in a horrible mood. So I think it's best to go to the homes where you get--it's sort of like a power test--get the best behavior. We're not asking what can they do under adverse circumstances. We're asking what can they do under conditions that are optimal. And what's really interesting is that I was funded with a really high priority score for my first grant, really high. It was the highest priority score, right, in the whole National Institute of Mental Health, and next time I came through they wanted me--that study committee said, "Fine, fine. But you should get rid of that old mobile stuff. Try something new. She's still sticking with that mobile stuff?" These were the actual comments. "She should throw that away. She should be encouraged to try another procedure, because this is not interesting." And I thought to myself, "Would Skinner throw away his Skinner boxes? Would Thorndike throw out his repeating problem boxes? I can't throw away my mobiles--they provide wonderful data and could be adapted to answering a lot of questions." And so another thing that they said was--so basically they slapped my hand, and gave me two years of reduced funding, not three years, hoping that I would get rid of that horrible mobile thing--and they also said, "And when you come back you'd better have,"--study sections aren't supposed to do this--"You better have a comprehensive theory of memory development." So I thought, Oh my g**, we don't have enough data to have a theory. So I came up with the one, 70 pages, single-spaced, had it all together. My graduate student at the time, Harlene Hayne, was waiting for me to print it out and she was gonna duplicate it and drive it to Washington the next day. But my computer crashed. I lost my whole theory of memory development. I called Harlene at 3:00 or 4:00 in the morning in tears. There's nothing I could do. My whole computer crashed, and that was it. And I called the next day--I called the secretary of the section, and she gave me 'til August to get it in, so I rewrote. It's not bad. I don't think I--I know more now, but I have new ideas about it. But at any rate, that was very traumatic, and the next time it went to that section in August, they said, "What do you have this stupid theory of memory development for? Whoever said you needed that? Send it back to her. Why does she need to have that?" And yet at that time, NIMH gave me a merit grant, a ten-year merit award. Two percent of researchers in the country have one, and I got one for having--I don't know. So they slapped my hand one time, and they give me a merit award for ten years the next time. And then the next year after that, I asked and was shifted into the Psychobiology Study Section, where I've remained ever since, because my work is basically a psychobiologist's. I consider myself a developmental psychobiologist because I use an animal model, although not exclusively now, to study preverbal infants, and preverbal infants—like animals—don't say a lot (except my dog, who definitely talks). And so babies are sort of like them—it's how you ask the question; the instructions are in the environment set up for them, not in the--or in the task--not in verbal instruction. But it just goes to show that the luck of the draw, what section you're in, who your current reviewers are, can make such a difference. And I know many, many people whose grants have gotten lower priority scores simply because they probably had somebody unknowledgeable or with a chip on the shoulder giving some totally insane, inappropriate review, and I feel good research is not being funded. And I'm not opposed to bad research being funded, because I think that eventually it'll be culled out, and people will recognize bad research for what it is. It won't get published. But I don't like it being funded by an old crony system at the expense of really good young researchers. And I don't like young people being dissuaded from keeping at it and persevering. That's what my husband told me, "Keep gathering the data and showing them. At some point they'll have to account for it." If you don't have the money, you can't do that, and you have to go through obstacles and be really resistant to criticism and persist. When I was editor of *Infant Behavior and Development* for 18 years, my one thing was if the methods or the procedure is clean, there's no confounds, and the data clear-cut, then within reason you can have any ideas you want. If it's an important problem, a new problem, go for it. And so many young people send out articles, and they get these crushing reviews, and they just give up, and they have to learn to persist. Young people can have new ideas, and they look at things in new ways, and it's important for us to read about those and to think about what those new ways may be.

Gerhardstein: So we'll turn now to the general topic of institutional contributions. First, in what institutions have you worked, including dates and capacities?

Rovee-Collier: Well, I guess the--you would consider postgraduate work, and I've always been associated with some state institution or the other. And originally, before I got my PhD, I got my PhD in 1966, I came to New Jersey in 1965 in my third year, and--in May, and that September I started as an Assistant Professor II at Trenton State College, and I taught there. They were looking for five people to teach courses. I was--I taught the Experimental Psychology lab, the conditioning and learning labs, and a variety of different courses, mainly Experimental Child Psychology and Development. I taught everything. I taught Psychology of Mental Health of all things. And I taught Graduate Statistics and Undergraduate Statistics. I taught five courses a semester, and that was a really hard load, but I loved the students. And at that time, Trenton State was an educational institution. They only gave education degrees. While I was there, they converted to psychology, you know, a liberal arts college. And the first--there were undergraduates--there were two undergraduates there that went on to good graduate schools because they worked with me, and I still correspond with them. In fact, one is really well-known, and it--that made me realize the importance of undergraduate research. One went to LSU in clinical, and the other went to University of Virginia and worked for Doug Mook. Eventually, after six years there, I--at Trenton State, I moved to Rutgers, because I resigned as a matter of principle from an action that the department took and changed the grade of an adjunct professor, and I thought that was wrong. It was during the Vietnam War time, and they were a very liberal department. And this was a student who never came to class and had been marching and supporting the war and so forth, so the department voted to give her an A in a course that she had actually only taken one exam in and had flunked it and hadn't reappeared in class since, but she was the darling of the department. And so I resigned just on principle. I didn’t want to report it to the AAUP, because they'd just gotten off the blacklist, and I didn't want to get them back on again. And so it occurred to me at some point that maybe I should get a job. So I applied at Rutgers, and I was very fortunate to have gotten--you know, to be hired. Two of the people I'd studied with at Brown were applying for the same job I was, and they hired me. So I had gone--I had become an Associate Professor at Trenton State, and I started all over again as an Assistant Professor at Rutgers, and they say I was an Assistant Professor for longer than anybody else except a fellow in English. I was Assistant Professor in my life for around like 11 to 12 years. That's a long time. And I've just been at Rutgers ever since. And I started out at out Douglass College, which was the women's college at Rutgers, the women's division, and my--some of my very best undergraduates came from there. And I've been very fortunate and blessed with almost all of my undergraduate students; many undergraduates work in my lab. They have fueled the research in my lab. As the grant reviewer said, she's got cheap labor--well, let's say *devoted* labor, and they've gone on to graduate schools in some really good places with really good people, and I still correspond with them regularly. They still come to my Christmas parties, and we're really close. I think that’s the other thing about the teaching, which is a portion of--I did teach child development research and experimental child, but I basically taught Conditioning and Learning or Motivation and used examples from my research and examples from my chicken research in those classes. And those people were the ones who'd taken Conditioning and Learning, and who especially were attracted to my lab. And one of the things that I had learned at LSU when I was an undergraduate, as I mentioned earlier, was that you always give recognition to the undergraduates who contributed to your research, give them recognition in the publications. And so I've tried to make a point of making sure that those undergraduates who've contributed to the research went to the professional meetings, presented posters or at that time gave talks at professional meetings, met people in the field, knew them by sight, oh, that's Jerry Kagan, oh, that's Lew Lipsitt, “oh, hi Lew, this is so and so,” and also when they ultimately did the research, many of them honor students and many not, that they would be authors on those publications, sometimes first authors, depending on how much they wrote up themselves. And that was really, really critical. I would like to list some of those students, because many are really well-known in child development. Among those students are Barbara Morrongiello, Pam Griesler, Eve Perris, Wendy Hill, Janis Kupersmidt, Rosemarie Truglio, who is the Vice President of Children's Television Workshop or Sesame Street, and John Worobey. I am very, very proud of the affiliation I've had with these students. There are a number of people who went on to wonderful schools and got clinical or professional degrees, and

I consider them my own. And they all think that the experience in the lab contributed to their--to begetting their success, even those who worked with chickens initially have appreciated that, even people who've worked with chickens who've gone into clinical. And they still think that, wow, that was really important. It's cognitive dissonance, I think. But in terms of graduate students--I was very fortunate to have this funding from the Merit Award to fund my research, and I could then fund graduate students. And I've had wonderful graduate students who've gone on and become stalwarts in the field of child development, very active in SRCD and other societies. And my rule has always been, do not appear as an author on a student's dissertation, because I feel that this is their first foray into the life as an independent scientist, and I will not be an author on that foray. So people like Kim Boller, who is very well-known right now in early childhood education, she had an SRCD congressional fellowship at one point, and worked at NIMH in Washington on the early cay care project and Head Start, and still does that work with a company locally, Mathematica, which writes grants and takes subcontracts and continues to look at that. I was very interested in working with Fagen, who studied interproblem learning for his dissertation. Boller and Fagen published alone. Who else? Harlene Hayne. Her dissertation was on multiple reminders, and that's become a major aspect (multiple reminders) in our lab, but her publication won a dissertation award. Like Karen Hildreth and Scott Adler. Karen published her thesis with--she insisted on putting Debbie Hill, my lab tech at the time, on her thesis, because Debbie helped her run babies. Publications are like boy scout badges. You accumulate them, and you accumulate them, and you accumulate. And at this point in my career I don't need first authored publications. In fact, it's better for students if they--maybe they'd do better if I weren't even on the publication! But it's really important to students, and I love students, and that's why I keep doing my research. I teach--still teach conditioning and learning. There are usually three or four people that come into my lab and do honors theses with me from that course, and in graduate classes, graduate seminars I teach are usually on the development of learning and memory, or the <u>comparative</u> development of learning and memory, or human infancy, or the development of implicit and explicit memory.

Gerhardstein: So this might be a good point to go to this question. It seems to fit in here, the third question here about describing your experiences as a teacher--

Rovee-Collier: That's what I was doing.

Gerhardstein: Okay. And then there's this specific sub question there that I don't know if you want--

Rovee-Collier: --between teaching and research?

Gerhardstein: Yeah, yeah.

Rovee-Collier: Well, you know, I don't know of tension between teaching and research in small colleges, but with the demand for funding in large research institutes and universities, the emphasis on publishing or perish has gotten to the point that teaching is reduced by buying-out of a course, so people have to come in basically with their own grants, and that's not always easy. They may get those as post docs, and take them to the institute where they're gonna work, but it's very difficult for people just starting out to--and it depends on whatever the-whether it's a "buyer's market" that year or not, whether there are a lot of people coming out in social or whether there are a lot of people coming out in cognitive, or developmental or whatever, and who knows who--the "old boy's school" is not dead, and a lot of institutions want to truly advertise, but have somebody in mind for that position, and it's very difficult on young people who are very idealistic and think that hard work'll get you anywhere. It doesn't always work that way. And so the problem is that they can end up teaching like seven courses a year and spend all their time in preparation and have very little time to do research, or the institution will give them hardly any money to fund it, although it'll pay them to go to professional meetings, they'll have a research budget for travel and that makes it very difficult to satisfy all of your bosses and all of--or meet all of your expectations. So if you attend to students, then you might not do as well on the research part, or if you help students do research, which is important for them to learn how to do, and I think that's really critical, then you might not have the time to write the grants and get enough pilot data for yourself. So there is a squeeze there, and again, it depends on--it's almost as if you have to make decisions about whether you're primarily gonna do teaching or whether you're gonna do research, and a lot of these smaller schools are now calling themselves universities, whereas they used to be colleges, and in order to call themselves a university, they have to have two or three active approved graduate programs. This makes it tough. So a student may go into a job with a lot of publications, but needs starter funds, and three years later they come up for reappointment and they're let go and they're told teaching isn't important, you have to have a publication a year while you're here. And how could they do that? They're not gonna have one the first year, because they're just getting started. They're not--probably not gonna have one the second year. The publication process may take eighteen months to two years before you--between when you do submit something and when it gets out. My *Science* paper came out in 1980, but I had reported that in 1978. That'll give you an idea of how much has changed in that line recently. And so I think that there is an unnecessary competition between these things, but it's very hard on new people.

Gerhardstein: Just one more question in this category. Describe your experiences in applied child development research?

Rovee-Collier: Oh, thank you. I wanted to do that, because people always--they ask, well, so what does what you have to do got with--to do with--what--what are you doing that has to do with the real world? Like, who cares? And my--I mentioned that my first, my first grant, or my first paper on modifying memories had to do with children's eyewitness testimony. And I--that was not a random choice. That was a choice that was made because I wanted to make sure that people understood that memory distortions in babies worked exactly the same way as memory distortions in children and adults. And I also have been very interested in problems of updating and I wanted to make that clear. When can a memory be updated? When can it not? And I think those are general principles. The other thing is that I think that there is a relation between baby learning, what babies are able to pick up on their own without being reinforced, and their attentional capacities to learn in the real world. And it's conceivable, as Joe Fagan has done with the Fagan Intelligence Test, it's conceivable that you could look at this basically as what can they pick up from the world, can they put two and two together and make four as opposed to two plus two making three, what can they do when very young. And if we know the expectations for normally developing kids, like the learning curve for the mobile over the first 18 months, then you can start seeing when kids start falling off that curve, you can start seeing when kids can't put two and two together, and if you can have a task that can measure that very early you might be able to do some screening and detect that. And the key is, we study individuals. We don't study large groups. I have very small groups of subjects. And that allows individual prediction. We can look at a baby's score and say whether that baby probably remembered or not, because even though we do group--we do--we have six in a group, even though we do an analysis over groups, the measures are individual measures. And we know that when you retrieve, if you can learn it, are the problems in remembering it? What's the most efficient way to remember? How can you facilitate retention? And using the time window notions you can program information to be repeated after certain intervals, and that has proven to be a very useful thing in studies with Alzheimer's patients and studies with institutionalized individuals. Programming repetitions of events so that they occur near the end of the time window, even if the person can't remember it very long as an Alzheimer's patient, if you get them to reproduce it just before they forget, then they can remember it may be up to 45 minutes instead of not even two. So these--there are potential applications that'll come out of this research, and I feel very good about that.

Gerhardstein: --your experiences with the Society for Research and Child Development, when did you first join SRCD, and what are your earliest contacts with the Society, and with whom?

Rovee-Collier: Frankly I can't remember when I joined SRCD. It seems like I was in SRCD forever. But I'm sure that *Child Development*, its publication, was always a critical publication to me. And I think that my interactions with Lew Lipsitt from Brown were probably critical in my participation in the SRCD meetings. He founded the International Society of Infant Studies, and I started going to those on alternate years. But then in the early '70s I happened to go to a Merrill-Palmer meeting in which Les Cohen gave a talk, and Joe Fagan happened to be in the audience, and nobody knew who Joe Fagan was at that point. And I happened to sit next to him at breakfast the next day, and we struck up a conversation and became fairly good friends. Anyway, he stood up, and he challenged Les Cohen, oh, you can't believe, over, and over, and over again, it was a really interesting meeting. So I think that was a very stimulating exchange, and I think thereafter I started going to SRCD if I didn't have to travel to the meeting. In the '70s things on infants started appearing in the *Child Development* journal, and I became very interested then. And I have all the old copies of *Child Development*, yellowed pages on my shelf, and it was just article, after article, after article and so forth that were important. The--my history of the--going to the meetings was affected by, as I say, my travel funding, and you can't really go to an actual meeting so easily, especially these days--when it's so expensive to stay in a room overnight. But my recollection is that SRCD was--has grown, and grown, and grown, and I'd go there, and I'd see my friends and people who disagreed with me--almost no one agreed with me it seemed, but I knew everybody because I was editor of this other journal, and I enjoyed immensely those papers, those posters and so forth. It was a very important stimulus over the years. I was very fortunate to receive an award for research contributions from SRCD, and all my present and past students were there, and I was in--I was very grateful that they came and--because they were responsible for my research contributions, not me. But the meeting got bigger, and bigger, and bigger, and it's--it would--it's almost as though it was overwhelming. There were too many choices, too many conflicting papers and so forth, and so I've taken a step back and decided whether or not it's--whether or not it would be more profita--whether or not I might more profitably attend smaller meetings where you can have greater interaction over papers than something so gigantic as it's become. It's a testimony to the field of child development that there are so many people all over the world that are now involved in it, because when I started, believe me, there were a handful. And this is what happens with organizations I guess. Anyway, it was kind of a shock to get a--in 2003 to get an award from the Society for Research in Child Development for Distinguished Scientific Contributions from Fran Horowitz, who I've known for years. She was the President at the time. And I'd think, Oh my g**, have I been around that long? You know, now I'm a “goldie oldie,” or a “tarnished oldie” let's say. At any rate, the changes in SRCD over the years have been primarily in its outreach, primarily in its involvement with public policy, all of which I think are incredibly important, and as I say, my--one of my students got a--Kim Boller got a Congressional Science Fellowship from them, and that's been very important and it's affected her whole career. And so I applaud the activities of SRCD, and it's really too bad that we're being crowded in our houses, and crowded in our traffic and everything else, and now I find the meetings too crowded. It's almost like the Society of Neuroscience, which now lasts--their meetings now last seven to ten days, and people stagger out after the first couple of days when they present their poster saying, "It's too much. I can't do it anymore," and they're holding their heads. I'll comment on changes in the field over the years that I participated in it. You know, being in infancy, I was there at the very beginning. I could be considered the second generation of infancy researchers in this field, and that's kind of depressing! But Lipsitt was in the first wave, and I'm in the second wave, and now my students--original graduate students have students who have students who have students, and that's kind of depressing! But I'm glad that infancy has grown up so much. The questions that are being asked with children, and I include the infant as a child, are much more sweeping and much more interesting, and much more, much more focused, I notice, on the child itself than on questions that grew out of other fields. Now, with preverbal infants this is not the case, but language development and autobiographical memory, and sense of self and a lot of social-personality issues, those are--seem to be dominating the field right now. When I was first starting there was a debate about Mary Ainsworth and the question of attachment. Should you pick up a crying baby or is that reinforcing crying, and all of those things were the top burning issues of the day. And now those may be still asked in some ways, but now the field has moved on, and they're asking about the learning context and so on. I think that it's a very exciting time. A field will go along for a while without changing much, and then there seems to be a large leap in the questions that they're asking. I see that probably as a result of funding, number one, or it's a result of students who take a variety of courses, and then go out, and then they start with what they're doing, but then they see something else, and they leap forward, they're retooled in another area or whatever, and they apply their knowledge from basic perception or whatever in another area. Now the questions are no longer, “Is early experience important, and how can it be if they can't remember their early experiences?" Now we know the mechanisms for remembering early experiences. Some people ask about infantile amnesia, what's the basis of it, and that's an interesting question, possibly unresolveable. But by the same token there is an appreciation of the fact that things that happen early on may be persistent, may be lingering, and if they're not remembering, the question is why. Instead of accepting a label (like Piaget had a lot of labels, stages, things happen because you're in a certain stage), now we're more into mechanism and the process instead of just labels. Now we're--some things--maturation affects a lot of things, but it doesn't affect everything, but it does affect some things, like Watson said, you don't start with a completely blank slate. Now we're into modules and new ways of thinking about things. Now Gallistel's saying there's no such thing as association, but it has to do with mathematical probabilities of events predicting other events, and that's an interesting idea, and the idea that the animal or the baby or humans have sort of internal calculators or, or neural mechanisms that address two kinds of questions, frequency and place, and that's--those are all interesting and should spur people on. Today the big thing is neuro-imaging, and fMRI's, and you have to have "neuro" in your title to be respectable anymore. It used to be you'd wear a white coat to sell toothpaste--that made you respectable. Well, now you have to be a developmental cognitive neuroscientist or a developmental behavioral neuroscientist. Well, I think you could just be a scientist, and it's certainly interesting to see what brain areas are active at what points in--you know, are correlated with different kinds of ongoing behaviors. But correlations don't necessarily mean that that's the controlling neural center. And I've looked at some of these pictures, and it's a mess. It makes you actually believe Gallistel-- that there's a probability distribution of dense activity, which are the denser number of areas lighted up. The question of false memories is very interesting. Schacter found that when people are reporting false memories, one area lights up and another area, when it's an actual memory, doesn't light up. So that's all interesting. So I still think that they're--that the neuroscientists or the people who are studying recall and recognition, and implicit and explicit memory, or declarative and nondeclarative, I think they've got it wrong. They're--it doesn't really matter what the label is, I mean, call it declarative, call it nondeclarative, maybe different parts of the brain are affecting one thing, then another, but until you can really know how the brain tracts work, it doesn't really do much good to give it a label. The question from the organism's standpoint is what can they do, and how can they use that memory in doing something else. And I'm very, very concerned that people have bad databases that they're working with when they assume that something first happens late in the first year of life, and you have shown that it happens by three months. Well, what does that mean? There are two possibilities. One, you could just reject it and just pretend like those data weren't there, and that's what many people tend to do. They tend to report data that are consistent with their ideas and just ignore what isn't. But the other alternative would be to say, "Okay, is this different, or is this qualitatively different, or quantitatively different, and how does this lead to this, lead to this, lead to that?" and try and develop an understanding of mechanism and process, and maybe you could use these data as a way of looking into the nervous system to ask what really is going on instead of building walls around your data. So you know, a perfect example of this is correlated attributes. There was a point in time in which Younger and Cohen said that babies couldn't detect correlated attributes until seven or ten months of age. First they were sensitive to them at seven months, and then they could use them to categorize at ten months or older. Well, Ramesh Bhatt found that babies (using mobiles with correlated attributes) could detect them at three months and could use them to categorize at six months. So are we going to reject all of the conflicting data on correlated attributes from three and six months and say that's no good and just--and develop our age-related theories? Or are we gonna say, "Whenever they start to do it, first they're sensitive to it, then they can categorize with it." That's the broader statement that holds across both sets of data. What are the data showing? The data show that what you get is specific to the task you use, and if you use a different task or different parameters you might get different data. I think that we're wedded to the same task, and so we need to see how our concepts extend and how our principles extend across tasks. That's why we started including a deferred imitation task in our laboratory, and we use that also in conjunction with the conditioning task. And that's--that--I think that has made our research broader. Now, the question has to do with also, "what are your hopes and fears for the future of the field?" I have very strong concerns about the future of the field. And my concerns are that people's research will go where the money is. It has to, because research is getting more costly, it requires money to start up, the machines are getting fancier, and so you'll have schools that don't offer money for fMRI's to persons doing that kind of research, and so only the wealthy places, the people who have a lot of money, will be doing that work--and a lot of very talented people won't be able to do it. So I worry that money available for research will drive the kinds of questions that are being asked. That's one of my first fears. My second fear has to do with whether or not we'll--we're heading towards psychology of the study of abnormal behavior, that is, if risk infants are basically what's in the hospital in the neonatal and intensive care unit, they're mainly what one has access to, or in clinics or--so maybe we're studying populations and looking at abnormal behavior before we even know what normal behavior would be. And that concerns me, that there's not enough basic research. We haven't begun to detail the course of normal development. And the National Institute of Mental Health doesn't care about normal development anymore apparently, according to their new guidelines. They're only interested in psychiatric types of problems, they're not interested in normal development. And so I worry that there won't be a place for that in the future. And my other fear is that--well, I have two more--one--another fear is that the field will become so specialized, that is, students will study perceptual development of something or other, that they will lose sense of the big picture--they will not have courses--basic courses in learning, or social psych or aspects of developmental, they'll only have this very narrow little problem, some subfield of a subfield of a field, and they won't know--they'll be so specialized that they won't be able to place what they're doing in the broader context. If you're a psychologist you should be able to read psychology. If you're a psychologist, you should be able to read about a lot of interesting problems in psychology. We behave in a context, a social context. How do we use what we've learned? What we perceive? How does it affect us and so forth in that context? And I've seen students--I really hate to say it--they're so narrow in their interests that they won't take courses in anything other than that little narrow area that they study. Even at Rutgers, I hate to say, where they have a variety--a lot of very well-known developmental psychologists, when I teach "The Comparative Development of Learning and Memory" I think one student in the last ten years has ever taken them from anyplace other than my lab. For example, the Behavioral Neuroscience program people don't take it. They're studying electrophysiology and things like that, and that's not the way that psychologists--that's not the way I was trained. You should be broadly trained, appreciate problems broadly, and then within that breadth have some specialties, and then in those specialties have some more specialized knowledge, obviously. But one doesn't become, you know, the master of one or two neurons, you can give a ten-word phrase that narrows down what you do so that you're studying like one millisecond of behavior between two stimuli, and what brain pathways are activated and so forth, but what does that mean? And the final fear I have is related to this, and that is that in order for students to succeed in science as teachers themselves, as researchers, as productive scientists, they must be able to do research and publish it. And my fear of what's happening in the field affects what is able to get published just like it affects what can be accepted by grant review committees, although in grant review committees at least they have--may have more senior people who are broader. But the--there's been such a disconnect between the fields and so much specialization that people will not know what is important or novel. And I'll use myself as an example, because almost every manuscript I send in is a fight. It's a fight to get it published, and that's why I say, maybe my students would be better off I weren't an author, it might be--you know, because I carry a lot of conceptual baggage with my name. People assume things about my research which may or may not be correct, but they just assume it because blah, blah, blah, because they haven't read anything for 20 years, and they know what I did a long time ago. Well, if a new person in the profession sends in a manuscript, and it's--and we touched on this before--and it's received really bad rejection letters, they have to be stalwart enough to resist giving up. But also, they have to recognize that some of the people who are reviewing that manuscript may not have the background that they have, maybe some of those people that are reviewing it are not up to the task of seeing the big picture. And so--and it's interesting, and so, for example, what I consider my most important recent paper, which appeared in *Psychological Science* in July 2006, in which associations were formed between two things that were physically absent—I got raving reviews from--mostly from people I suspect were learning theorists and perception theorists, and they were--I know who some of the reviewers were, and they were major authorities in the field of learning, conditioning and memory. And so when I send a related paper to *Journal of Experimental Child Psychology* they rejected it because the reviewers say, "This is unimportant, associations aren't important in child development. Nobody cares about associations. Everybody knows that children associate memories of things in the past with things they see now. Everybody knows that. Associations aren't important. Reject." Well, this is very devastating, because the field of child development does depend on them. Memory is associative according to memory theorists, associations are learning, learning and memory are important to children. So I feel bad if developmentalists don't know what's going on in psychology more broadly, and they only have a little, narrow perspective, and they think new ideas are old ideas. That's sad. They've over-generalized, and that's sad for the field of child development, and I feel bad for its future. Thank you.

End of Interview