John S. Watson

- B.A. in Psychology (1958) Wesleyan University, M.A. (1961) and Ph.D. (1967) both in Child Development from Cornell University

Major Employment

- University of California, Berkeley
    - Professor Emeritus of Psychology: 2001-present
    - Professor in the Graduate School: 1994-2001
    - Assistant, Associate, then Full Professor of Psychology: 1967-1994

Major Areas of Work

- Infancy, learning, perception

SRCD Affiliation

- Past member

SRCD ORAL HISTORY INTERVIEW

John Watson

Interviewed by Lori Markson
Berkeley, California
July 29, 2004

Markson: Okay, John can you describe your family background, along with you child, adolescent life experiences that may be of interest, including things like your education, occupations of your parents, where you were born, grew up, what was your schooling like, were you ever in the military, early work experiences, things like that.

Watson: I was born in 1936 in Boston and lived there for the first five years of my life. Then, as the depression became personally very severe for my family, a little bit behind the wave of the nation, my father lost his job and we moved from Boston to Connecticut. But I would mention that my mother was a college graduate from Simmons College in Boston and my father was a graduate of Penn State. The somewhat unusual aspect was that my father was 20 years older than my mother. This was a second marriage for him. His first wife died, leaving him with two sons, 12 and 14 years old. They were 20 and 22 years older than I. The younger of them, Bob, lived with us. He was a cripple by the common classification of the day.

Markson: Was it polio?

Watson: He had had cerebral meningitis when he was eight years old. They called it spastic paralysis. So this family of five, my brother and my half-brother and my mother and father, we moved to Connecticut. And in Connecticut we were very poor, but my family was able to buy a house from the state that was pretty cheap even for back in those days. It cost 1500 dollars. It was on a bank of the Coginchaug River, out in the country, outside of town—Middletown, Connecticut. Each spring, it would flood into the basement. The house had no central heat, no running water, no bathroom but it did have electricity for lighting. All of these things are relevant to the statement that, as I look back and try to figure out why I became a psychologist, I think it was because I had this interesting childhood. I had a childhood in which I had a full brother and a half-brother, and my half-brother could not be understood by most people, even at times in the family by anybody but me. I was the translator at the table, for what he said. That was probably because I was the youngest and had just been able to pick up on what those sound patterns were. What they were like was a deaf person who has learned to speak. I had, later in life, when teaching at Berkeley, a deaf student in a seminar. I was the person, basically, who would translate what he would say to the class because his speech had a distorted sound pattern that was very much like my brother's. Another influential aspect of my family was that my whole-brother, who was four years older than I, was extremely shy. So when we would go out into the world, I would be the person that he would press forward to deal with people, buy the tickets at the movie house, et cetera. All of the preceding is meant to give background to the statement that I was almost forced to be a very introspective, reflective child, in the sense that I was empowered in my family by the fact that I was a translator for my half-brother and I was an important facilitator of social interaction for my whole-brother.

There was another aspect of my early experience, I think, that led to a great amount of introspection and a sense of wanting to understand how minds work. And that was that mine wasn't working that well in terms of school. I had a great deal of early schooling. I went two years to one of the first nursery schools in the country. I learned later in life that of the first three, one was Banks Street in New York and another was Merrill-Palmer in Detroit and one was Ruggles Street in Boston. I went to Ruggles Street. And so I had this early schooling, but the problem was I didn't learn to read. I had what was, in those days, called mirror-mindedness, which was just a term used early on for dyslexia of the classic god-dog garden variety. I also had two other problems that I hesitate to expose, but they're relevant. One was that I suffered from what we'll call extreme absent-mindedness and the other was a degree of aphasia that led to very difficult times coming up with words while speaking. Even though I knew the words, I'd have to substitute other words or long phrases to get around the lost words, and still do. The feature of absent-mindedness was so strong the teachers would have to come and shake me out of it. They might've been more like fugues. They were disconcerting in that I might be talking and then find that I would go into a self-referential loop that was difficult to get out of. I feared this all of my life, especially when I was teaching later on. But it pretty much went away by the time I was 12 years old. I did eventually learn to read somewhere after third or fourth grade. Each year prior to that it was a big decision as to whether or not I should be able to go to the next grade because I couldn't read. But I was verbal and was doing reasonably well in other things and so they would pass me on. And they eventually taught me to read with a special little card that had a slot in it so that I could move it down the page and follow the arrows left to right. The problem with that is that I learned to read in a fashion that was totally translating letters to sounds and I did not understand anything that I read aloud until I was in the eighth or ninth grade. All of this I think is relevant because it led to, early on, a desire to understand these things. I had terrific support from my mother. My father, however, was sort of dismayed because people in his family and my older brother, although very shy, had no such problems. So I was odd in this regard and my father was somewhat embarrassed by my shortcomings, I believe, and a bit self-righteous. I was kind of an anomaly that he didn't understand. But my mother really was very understanding and made excuses all the time for me. For example, she told me that my severe absent-mindedness meant that I should probably be a professor, because professors were absent-minded. And the fact that I couldn't spell was no problem, people in professions could get secretaries and they could spell. All these things were supportive. So I decided in fourth grade that I would go on to be a professor. I already had an idea about what my dissertation would be, and it was going to be titled "Memory, Learning, and Thinking." I held onto that idea until my senior year in high school when I had to do a senior term paper and I decided I had nothing else to talk about so I'd better use my dissertation. So that was the topic I used for my senior paper. But at the same time I had constantly been of the mind that I would become a physicist. Everybody was going to be a physicist or an engineer in those days. So that was the early portion of life as I recall it.

As to why I became a psychologist, I think because, in some sense, I was terribly interested in myself. Hopefully as time passed that became less a determining feature of my thought. However, there is a possible remnant of that self-reflective aspect of my professional career that came up in my dissertation. It was about orientation perception in infants. I only realized, probably five or six years after I finished, that what I had done was essentially gone back and tried to understand why I was a mirror-minded child and what was entailed there, and to sort of provide an excuse for myself. But the next part of the question is—let's see—

Markson: Early work experience and military experience.

Watson: Okay, yes. Let's see, early work experience. Because we were poor, I started working from the time I was eight delivering papers. I continuously had some job for the next 50 years. But at any rate, the jobs I had from age 8 to13 were delivering papers and caring for horses at a summer camp. When I became 14 I worked three summers on a local tobacco farm. Later I worked summers in various available jobs: a fruit stand, a diner, a box factory, and an open-pit feldspar mine. Throughout college I worked in a fraternity house as a housekeeper, a waiter, and a dishwasher.

Markson: Where did you go to college?

Watson: Wesleyan University in Connecticut. I should probably mention how I got to go to college at all. We moved to a house with plumbing in Portland, Connecticut, as I entered seventh grade. I attended Portland High School. It was small and I was a large person. My size played a role in that I became an athlete. And being an athlete turned out, even in those days and even with respect to Ivy League colleges, an advantage in getting into college. I didn't have any money so I needed a scholarship and what got me into college was basically being a reasonably good student and a reasonably good athlete. As a combination for the Ivy League, this was really good, so I got—

Markson: You played football?

Watson: I played basketball and track. I threw the weights in track—shot, discus, javelin, and later the hammer in college—and I played center in basketball. Although we were a small high school, we were very good in those two sports. And so in applying to college I won a scholarship to Harvard and a scholarship to Wesleyan. Then I made a very smart decision, I believe, against the advice of the people that had worked hard to get me the scholarship to Harvard. Even though I felt a certain level of confidence, I was always aware of my deficits and my learning difficulties. I decided that the odds were better of making it all the way through Wesleyan than making it through Harvard. I would have to leave school if I lost my scholarship—these athletic scholarships got you in, but you had to maintain a B, 83.33, average in those days. If you didn't, you lost your scholarship. I thought that if I lost mine at Wesleyan it would be possible to finish while living at home, an option not possible with Harvard. It was a wise decision because, despite working very hard, I lost my scholarship at Wesleyan at the end of my sophomore year. An influential experience in my orientation towards psychology arose as I was preparing to leave Wesleyan. I had to see the Dean of Students to be counseled into a leave of absence because they wanted you to be able to come back eventually. But, at present, I couldn't afford even the option of living at home so I was leaving. The dean and I talked about my problems and my needing to go and all the rest. I shared my plan to eventually come back after earning some money. He said the plan sounded reasonable. But then, as I was about to leave his office, he asked me why did I think all of this had come to pass. And I said, looking for an excuse, maybe because I had been a mirror-minded child. And he was seemingly thunderstruck by this phrase. He asked that I tell him more, tell him more. It turned out that his daughter had been classified as a mirror-minded child. She too had had trouble in college. He sent her off to a special camp and now she was in graduate school in biology. He said don't decide to leave college, he would arrange for me to go to this camp. I said okay, and he did. And so they spent more money on sending me to this camp than a year's worth of tuition at Wesleyan. I went to this special camp in Maine run by Q. P. Sharp, a Yale clinical psychologist. It was important to me because everybody there was pretty smart because they had to pass a certain IQ test, but they were all messed up in one way or another. And a bunch of them were similar to myself, though they were all younger. They were all 14-year-olds or younger. There were about four of us that were college-age people. We had half of our way paid by working as the camp counselors. In my case, the university paid the other half. So this was an educating experience in the clinical sense of having a whole bunch of kids to look after and to experience a whole group of people who had very peculiar problems, many of them similar to my own.

So I did come back from that camp and went back to Wesleyan. The dean helped me borrow money from a source my father found and then I got my scholarship back for my senior year. So that was an influential experience. I decided toward the end of my junior year to be a psychologist. I was working at that time for a faculty member at Wesleyan, William R. Thompson, who had been a student of Hebb. He had just recently published in *Science* an article on prenatal influence in rats. I went to work as his research assistant. My first publications came out from working with him—with rats on prenatal influence.

Markson: So this was your undergraduate work?

Watson: My undergraduate work, yes. Two other undergraduate professors were very important. One was Stanley Coopersmith, who eventually became well known for his work on self-esteem. He had gone to Cornell—got his Ph.D. at Cornell. I don't think he was out very long before he came to Wesleyan. Another professor was Frank Barron, who was a personality psychologist. He was visiting Wesleyan from Berkeley at the time, but I think then shortly thereafter moved and spent the rest of his career at UC Santa Cruz. At that time, 1957, these professors and two graduate students, William Charlesworth and Arnie Mordkoff, were important as a set of people encouraging me and helping me apply to graduate school.

Markson: Did you go directly to graduate school?

Watson: Yes I went directly to graduate school at Cornell after graduation from Wesleyan. Cornell was the only place to give me financial support, so that was where I went. I was unsure of my ultimate goal and applied to both clinical schools and to research schools. I didn't get into any of the clinical schools, but I got into two of the developmental programs. One was to the alma mater of Thompson, Chicago—University of Chicago, but they didn't offer me any money, so I couldn't go. So Cornell, the alma mater of Coopersmith, is where I went. That was in the Department of Child Development, which was in the School of Home Economics. It was really excellent and yet a second-class citizen to the Psychology Department in those days. I think this becomes relevant to a historical view of child development and its relationship to psychology, in general, which young people like yourself may not fully appreciate. That is, the historical place of child development departments.

Markson: So this was general, that it was like this—

Watson: Yes.

Markson: —not just exclusive to Cornell.

Watson: There were child development departments that got started in a number of state universities through something I think that was called the Laura Spellman Rockefeller Foundation Grants. And they were initially undergraduate programs meant for women to gain a better, clearer understanding of children. I doubt that these departments had a development psychologist's goal initially, but I don't know that they did not, given that the foundation also funded the beginnings of IHD at Berkeley and helped SRCD get started. These departments attracted some excellent psychologists early on. For instance, Kurt Lewin became chairman of the Child Development Department at Cornell for a short time, but I think that was largely because he was escaping Europe in the late 1930s, and he eventually moved on, I believe, to either Michigan or Kansas or both. At any rate, the Child Development Department at Cornell became one of the better places for graduate training of developmental psychologists, I think. Minnesota's Institute of Child Development was coming along and probably shortly thereafter surpassed Cornell in breadth of academic power. But the Cornell department remained a really good one. The people there that were very influential in my life were Al Baldwin, who was chairing the department at the time, and Henry Riccuitti, who was the infancy person. Harry Levin and Uri Bronfenbrenner were also influential. These people were important, specifically in the training aspect of my education, but also by their support in terms of research assistantships and so on. Some of the things that were passed on as theoretical concepts by Baldwin have, in some sense, directed me ever since—e.g. dispositional properties and their perception. I think I'll probably get to that later.

Markson: Was Baldwin your main advisor?

Watson: No, he wasn't. He was for my friend Bill Charlesworth who also came to Cornell, and later taught at U. of Minnesota's Institute of Child Development. Charlesworth and I had both come to Cornell with interest in personality development. Indeed, I did a master's thesis project on objectivity as a personality variable. I had planned that Baldwin would be my dissertation advisor, but what happened subsequently was that I changed my direction of specific interests, in no small measure by the fact of having my first child. That child, my daughter Johanna, was inspirational. I was pretty much a half-time caretaker with my wife, Marilyn, who was a graduate student in philosophy at Cornell. We shared the duties pretty evenly, I think. That's why I had lots of time to observe my daughter. In those days, the theoretical frame that was central at Cornell's Department of Child Development was Piagetian. Baldwin was one of the early disseminators of Piagetian theory. The readings that we did led to my trying to do some observations of my infant daughter in the same light. What I thought I would do is a replication of his development of the concept of space. I shortly learned, however, that it takes a special mind to do intensive research on your own child. I found that I really couldn't do that. I couldn't objectify her constantly, taking notes. I felt that it was altering my life in terms of my relationship to her.

Markson: She was like under the microscope, not being the daughter?

Watson: Yes, that seemed to be what was happening. And I, at the same time, began to see that there were other things I might do. I might do short things, mini-experiments as opposed to trying to work through a global transition in development. I would perhaps just look at a particular problem in space perception, see if I could zero in on that. So I started with depth perception and worked on that a little while. Then I moved on, by inspiration from my three-month-old daughter, to orientation perception because I found that she seemed to smile differently depending on the relative angle of my face when I was looking at her. I followed this up in samples of other babies between two and six months of age. That became my dissertation, the development of orientation perception, as I think I mentioned earlier on. Infant research as a topic was just beginning to grow, largely because of the work of Robert Fantz in Cleveland at Case Western Reserve University, I believe. Fantz had adopted a technique of preferential looking that had briefly been used by Staples in the '30s to study color perception in infants. His application of it was a breakthrough for people trying to understand what a baby's mind was doing in those days. It was shortly followed by the habituation recovery technique about five or six years later. But in this time period preferential fixation was the method. Many researchers started adopting it and I was one. There were other methods—e g. heart rate change—that were being pursued by notable people around that time—e g. Jerry Kagan and Michael Lewis at the Fells Institute. But there were not really very many infant researchers in the country. An important event for me was that, when finishing work at Cornell, a job possibility was offered at the Merrill-Palmer Institute in Detroit. What is interesting about that is that Irv Sigel was the head of research there and he had just started an annual infancy conference at the Institute. They had one conference or two already. He invited people in the country that were doing research with infants. Why he had this insight into the development of this field, I don't know, but he did even though it was not his area of research investment. The first series of conferences specifically on infancy research were at Merrill-Palmer. They lasted, I think, annually, for about seven years. During that span, *The Merrill-Palmer Quarterly* had one volume each year devoted to that conference. Those were where my early papers appeared. At this conference, researchers got to know other people who did infancy research. Shortly thereafter these people, under the leadership of Jerry Kagan and Michael Lewis started a group called—ended up calling itself the Committee for Research in Infants (CRI). And why that's notable is that, after about ten years of meeting every other year, that group sponsored the origins of the International Society for Infant Studies (ISIS) with the help of the Society for Research in Child Development. And then CRI disbanded with the origin of ISIS. So that was the gathering—early gathering of people doing infant research. Now, of course, ISIS has—gosh, there must be a thousand members, I don't know.

Markson: At least, right.

Watson: Right. So all of that was an attempt to say, in retrospect, in looking at one's life, that I was very lucky. I was very lucky for being in the right place at the right time.

Markson: And who ended up being your advisor in your thesis?

Watson: Henry Riccuitti because he was the infancy person in the department.

Markson: Okay.

Watson: And also an extraordinarily supportive person. I was not having an easy time of getting finished. I was having great difficulty in writing. And my reading was always with great effort. A little aside to that—I'll get back to the main point, but a little aside is that a wonderful experience of my old age is discovering in the last two years, basically, that I can now read for the first time in my life for pleasure. I actually can read without a sense of constant effort. And I've actually read—I believe it's true—I've read more in the last two years than in all of my life preceding it.

Markson: What would account for that?

Watson: I have no idea. Perhaps some level of brain functioning changed. Who knows, but whether it's deteriorating biological tissue, getting rid of some nasty neural circuits, or simply by virtue of time—getting enough reading done to develop good circuits—suddenly it became possible to read without effort. I can't say I've put it to good use professionally, though I have read professional things more readily than ever before. But I've done tons more of just pleasure reading, which I almost never did.

Okay, back to the point. The point was—what was the point?

Markson: The people at Cornell that you worked with.

Watson: Yes, that's right. So those people at Cornell, basically, fortunately got me to Merrill-Palmer Institute where Irv Sigel was an extraordinarily supportive leader.

Markson: So was that like a faculty position or like a postdoc?

Watson: It was a research associate position on the faculty. And so I was there for four years, struggling to pass my German exam at Cornell. I was still working at it, something like my eighth year of trying to learn German.

Markson: So it was required to have foreign languages?

Watson: Cornell required one to pass three out of four languages: English, French, German, and Russian. You got credit if you got a higher degree in a university teaching in the language. So my B.A. at Wesleyan meant English was a pass. I did study and pass my French, but I struggled and could not pass German. Fortunately, in 1966 they changed the rules at Cornell to two languages at a higher standard and I was grandfathered through on the French. I had been about to submit a petition to Cornell because Harvard had just let someone have a Ph.D. with a special deficiency in German. I was composing a letter to ask for similar treatment at the time that the news came through that Cornell had changed. So it all worked out. I finished up and then, I think largely by the help of my friends, I ended up getting an offer from Berkeley where I came and stayed ever since.

Markson: So when did you come to Berkeley?

Watson: I came to the Psychology Department at Berkeley in 1967. I started doing infancy research once we got a lab worked up in the Institute of Human Development. I think it took about a year or a little more. As you may know it takes a lot to get started. I think the one thing I didn't touch on, though it has no great bearing, is military service. I was in the Naval Reserve. I did learn some things that were useful.

Markson: How long were you in that?

Watson: I was in the Naval Reserve for eight years.

Markson: So this is like on the weekend or once a month kind of thing?

Watson: Every Monday all the way through college and all the way through graduate school.

Markson: All day? Every Monday?

Watson: No, just in the evening, every Monday evening and then two weeks every summer. I earned some money and I actually did learn some technical things like typing while going to Wesleyan. I was in training to be a yeoman—i.e. a secretary. When I went to Cornell, I had to switch to electronics technician because they didn't have a yeoman training facility there. I had to be an electronics technician and that was good because I learned about electronics that way.

Markson: But you never got called to war or anything?

Watson: No, thank goodness, the closest I came was the Bay of Pigs. We were all on alert, we had to be ready to leave at any moment, but luckily we didn't have to leave.

Markson: Okay. So were there political or social events that influenced your research in writing and teaching, in particular?

Watson: I can't think of any political events, with the possible exception of Head Start. Head Start had interest to me in relation to my desire to participate in helping children, particularly those of economic or mental handicap. That led to some work that I pursued through the years with a colleague, Richard Umansky, Head of the Child Development Department of Children's Hospital, Oakland, California. I think Head Start bears on this. The times made it possible to think that if you invented a procedure of some kind that would help a child, the government would probably kick in and enter into that support. I wrote a paper in the *Merrill-Palmer Quarterly* in the early '70s, "Infant Research: Setting for the Seventies," in which I talked about Head Start, and also discussed some ideas that we should be cautious about as well as some we should be energetic about. I think I was representative of a time of infant researchers in thinking that we were now coming upon discoveries that could possibly be very significant for intervention. I think that that exuberance has pretty much faded away in the present-day view of infancy. People still view early experience as very important, but as one increases the sense of innate endowment in infancy one, I think, naturally decreases the sense of how much nurture is going to do. Of course, that need not be the case. One could hold an interactionist view wherein an infant is endowed but needs environmental support for optimizing that endowment. That idea is pretty common nowadays, but back in the days when both learning theory and Piagetian constructivist theory were closer to the blank slate assumption of the infant's initial state of knowledge, advantageous manipulation of the environment seemed a realizable goal. In this spirit, I proposed a theoretical concept I called "natural deprivation." I thought that it was quite possible that human infants, in the present state of their evolved form, possessed a potential developmental handicap. I proposed that they were looking at the world with a brain that could analyze their control in the world but with a behavior system that was incapable of taking advantage of their analytic capacity. Further, I thought that this barrier to gaining control could potentially lead to a loss of overall cognitive capacity and initiative. This may now seem like a very dumb idea, but it was one that seemed worth thinking about at the time. I thought a lot about it and tried to work out ways of intervening. I thought about the kinds of machinery that might be put into homes. I wanted to make objects that could go into homes and help kids avoid natural deprivation—i.e. compensate for the mismatch of their behavioral prowess and their cognitive capacity. In fact, as I was being interviewed for the job at Berkeley in the spring of

1967, I was also offered a job at a toy company in Princeton, New Jersey, called Creative Playthings. It was kind of an upscale toy company, but headed by a man, Kaplan I believe, who had a degree in social work. He was pretty thoughtful and was putting out good stuff. I thought if I could get control of product development, I could perhaps do Crest-like research—i.e. sell toys by virtue of their proven cognitive effects. Following the recent success of the toothpaste company, I wanted to go into homes with toys and make sure that they would, in fact, help. I had some ideas. However, I could not get Kaplan to agree to give me money to do the validity research on the toys. He said that I could possibly do that after the fact, but not before the fact. Well, that decided it for me and I didn't go into that career. That was the temptation of those days. People like myself, possibly a few others, heady on the idea that we could really do something significant in the way of changing the beginnings of human life that avoid problems and get passed handicaps, things like that. Burton White was motivated along these lines, I believe.

Markson: Okay, I just want to ask about—I noticed one colleague that I know we've talked about and that was on your list, that you didn't mention yet. I'm thinking of Gyorgy Gergely and if you wanted to add anything about him. All the other ones I think you mentioned.

Watson: Oh, that's right. In terms of people coming along that were important in my professional development.

Markson: Important colleagues in your career.

Watson: I think I probably didn't mention some in sufficient form. Two were important over the whole of my career: Michael Lewis and Tom Bower. I never published with either of them. Never did direct research with them. But I have always been very impressed with their minds and they've been inspirational in many ways. I mentioned Richard Umansky. We have collaborated off and on over the past 35 years. We've only published about four or so things in that period of time, but it's been fun and they've all been in the area of abnormal development with some recent theorizing about brain functions in Rett Syndrome. I think most important in my last ten years, since I retired, has been the interaction and collaboration with Gergely. I met him about ten years ago as he was summer teaching at Berkeley. We've just had a wonderful meeting of minds. We published a few things and interacted over many things through the years. I am quite proud of our social biofeedback theory that we published in the *International Journal of Psychoanalysis* (1996). In addition, he and his colleague, Peter Fonagy, put together a conference on contingency perception and attachment theory that was published in the *Menninger Bulletin* (2001). That gave me a chance to update some ideas on how infants may perceive and misperceive contingencies in their environment and specifically with their caretaker. I will visit Gergely in Hungary in about a month.

Markson: Because you're going to Hungary for the summer?

Watson: I'm going to a little conference there on the development of intentionality.

Markson: Okay, so would you characterize the development of your ideas in child development as evolving in a rather straightforward way or were there sharp turns in your theoretical thinking and research style?

Watson: I think that it was a kind of a slow swoop, changing direction serendipitously. That is, I would be researching one thing and then something would pop out of the data. However, in the case of my early work on orientation perception, there I would say there was a sharp change from orientation perception to learning. But, as I mentioned earlier, learning and thinking and memory were kind of the original issues for my interest in psychology. Orientation perception was probably just my trying to look back and explain myself. But in working on orientation perception I found within the data very clear evidence that what the infants were doing was learning about object placement. In my dissertation, I had used two methods to assess infants' sensitivity to object orientation. One was elaboration of the method of eliciting smiling from various facial angles. The other was an adaptation of the preferential looking procedure that Fantz had popularized. In the standard form of this procedure, I placed two pictures on a display card: a right-side-up facial drawing on the right side and the same drawing upside-down on the left side of the card. Another card was made with the two drawings in reverse positions. A hole in the center of the card allowed me to record the infants' fixations over the 30 second exposure time. By the commonly held assumptions behind use of this technique, if the infants found one of the pictures more attractive than the other, they would be drawn to look at it longer. But my data did not look like what the tropistic assumption would predict. Rather than simply being drawn to the right-side-up face, which they significantly preferred given the sum time they spent looking at the two pictures over the presentation of the two cards, the pattern of their fixations implied they were learning the spatial positions over time. They weren't being drawn to their preferred stimulus by a kind of a tropism to attractive places. Rather it looked more like fixation was an instrumental act, or operant. That is, when one looked at their selection to look back and forth through time, it seemed as if they were struggling to remember where that place was that was interesting. Their probability of looking back to the more interesting place increased over time. I could look at this through time over the course of exposure of the two cards. Moreover, the procedural shift from one card to the other provided an illuminating test of the tropism versus learning notions. If they had learned the position of the preferred right-side-up face on the first card, they should now initially make a mistake by looking in the wrong spatial position for the preferred picture on the second card. But if fixation were simply guided by a tropistic sensitivity to the attractive picture, they should be guided to the attractive picture on the second card just as they had been on the first. It was just very, very clear they had to unlearn the position that they had been looking at on the first card. I followed that up with fixation games with my two-month-old son, Sean, that I published in the *Merrill-Palmer Quarterly*. Then I built devices that could just project pictures on two white circles out in front of infants. Pictures or sounds were made contingent on where the infants looked. Learning was rapid. That work on the conditioning of visual fixation is in the first volume of *Developmental Psychology*. So that was a significant shift and most of my work since then has been about early learning and how that learning goes on. I'd actually rather frame it in terms of how a child discovers that it has control in the world. I also prefer putting it in terms of the jargon that I came to early on, referring to this as perception of contingency in the world. That was what might be called a dramatic step. Everything after that, I think, has been pretty much a slow turn in going off in one direction or another that might seem more clinical or speculative; the way in which contingency perception might affect early self perception, or how it might illuminate autism or Rett Syndrome, for instance. But those are really not sharp boundaries or sharp changes. In the last six or seven years I have made a significant move into the area of simulation of artificial life. Still, the topic is about how systems come to have control and evolve purposiveness and intention. My research with infants was largely about how they come into the state of mind we describe as an executive state, the child looking at the world as a place of opportunity and a place to pursue goals. I have been, in recent time, pursuing a very similar question from an evolutionary point of view. I want to understand how this can come about, how this very fancy aspect of mind states can arise from virtually nothing, from the primitive states of complex systems. That's a funny phrase: primitive states of complex systems. What I mean is primitive states of systems that can behave but have no goals. My first publication of that work is to appear in an e-journal called *Evolutionary Psychology*. So I guess that would be the second major turn in my professional career.

Markson: So turns, but not very sharp turns. They always followed related topics.

Watson: Yes. I should mention one other thing that I developed and I think is a contribution. I was trying to understand how the infant was gaining this perception of control. I proposed relatively early on a model in which the infant was conceived to be examining the conditional probabilities of the contingency space that was in its surrounding. That model for contingency perception was, really, basically a simple conditional probability model with one addition, that being a placement of a time zone in the probabilities. So my conditional probabilities have an explicit inclusion of a time window. There has been a move into use of conditional probability conceptions of the infant's mind states in recent time by Alison Gopnik and her colleagues, Clark Glymour and their students. I think they're doing a wonderful job and developing a new kind of look at the analysis of causal understanding. They are using a Bayes-net system of analysis, much like Tom Bower used logic in his "The Rational Infant."

In such logical frames, single pieces of information can be taken to form causal inferences. The leverage, in my mind, derives from the use of negation of conjunctive and disjunctive propositions, but that is a speculation on my part. I think there is a distinction between the kind of probability analysis that Gopnik and Glymour have recently introduced and the probability analysis that I proposed in the '70s. Mine was a conception that involved an assumption that the infant was gathering a reasonably large database and then analyzing the conditional probabilities of that database. If there is anything risky about my proposition it is the idea that somehow the baby is able to keep track of a temporal span of its past action. But under that risky assumption, one can then move forward to ideas about the way in which this conditional probability analysis proceeds. One novel idea, unlike anything I see in the Bayes-net approach, is the idea that conditional probabilities can increase information by being framed both in forward time and in reverse time. I've called these the analysis of sufficiency and the analysis of necessity. I proposed that the infant was using both of these pieces of information when making decisions about whether it had control of something in the world. There's not a great deal of empirical support for it, yet I still hold onto that as a conjecture worth following. Forming conditionals on the effect as well as on the cause is not a unique proposition in the way of general science. The people in the area of epidemiology analyze data both ways. Scientists have long known that you can look at the effect and calculate what the causal options are and frame this as a conditional, versus looking at the cause and calculating the subsequent probability of the effect. So my proposal is not of any dramatic uniqueness, but as a frame of how the infant's mind is working in combining these two conditional probabilities it is possibly unique at this time.

Markson: I have one last question. You were thinking and were always trying to be very deeply theoretical, but then I also have been hearing throughout all of this application in various ways. And I just wanted to ask you about, you know, the boundary of the application versus theory. Did you try to apply your theoretical views in your intervention work?

Watson: Yes, I did apply my notion of natural deprivation in one early study of trying to help a case of developmental failure. I thought perhaps her hypotonic motor functions and/or a diminished short-term memory could have prohibited her successful perception and engagement of sensory-motor contingencies. I put a mechanical mobile in the home of this nine-month-old baby and I got very invested in how she changed. She changed dramatically from a very passive, incapable infant into a very active infant looking much like normal babies in transition at around three or four months. She became very different in the way you would describe her, to the point that the parents, in fact, started treating her very differently and putting ribbons in her hair and et cetera. But it was a sad experience in the sense that it did not cure her. She became a more capable, yet still a very, very retarded child. I followed her for about a year and then she was placed in a foster home for retarded children.

A second venture, similar to the first, was an attempt with a case of Rett Syndrome. I was still working on my belief that one might be able to go in and, even in dramatic cases of early brain state confusion, straighten them out by getting the contingency structure straightened out. So despite my lack of success with the developmental failure child, I optimistically took on this case of Rett Syndrome. Rett Syndrome was a relatively new category of early retardation—something like autism. It was actually differentiated out from autism. It was a disorder that seemed to occur only in girls. It was dramatic in the sense that a child that looked like she was developing normally for about six months to eighteen months would, all of a sudden, collapse cognitively to a baby that looked like a brain state of about three months. The child we worked with was classic in form. She was about two and a half years old when we first saw her. In the early days cases were often identified rather late. There are earlier identifications now, particularly now with genetic markers that are helpful, but in those days it was a behavior classification and most parents and even pediatricians thought the children were suffering from something else. At any rate, this child we began working with was classic in being highly stereotypic in hand wringing, loss of speech, teeth grinding. Theoretically interesting to me was the way in this classic form of Rett the instrumental functions that have developed, the normal executive functions, seem to have been overtaken and replaced by the stereotypes. My view of what possibly was happening was that the very early stages of contingency analysis were disrupted. Normally, by my view, infants began with a pursuit of perfect contingencies which was later replaced somewhere around three months with a pursuit of imperfect contingencies—that is, high but imperfect. That presumably leads them from investment in mapping the perfect contingencies of efferent-afferent relations in their body to mapping the imperfect behavior effect relations in their surrounding world, particularly their social world. That social world would give them high but imperfect feedback. For the Rett child, however, the thought was that this switch that had occurred in the contingency module—the switch that normally occurred around three months in normal children—had fallen back to its pursuit of perfect contingencies again. Theoretically, this would lead to a great deal of investment in the perfect contingencies of self stimulation and, thus, a loss of contact with the imperfect contingencies that were going to be so essential for understanding the higher order contingencies of the social world. So that was the theory. I worked for over a year with a strong investment in this child. I had dreams of her coming to talk, et cetera. I thought I was doing very well for quite a while, but in the end it failed. She was left as a perhaps better functioning Rett child, but that's it.

Markson: So you were very theoretical about it.

Watson: Very theoretical.

Markson: Okay.

Watson: I think I am supposed to comment on what work I think went nowhere versus where I am satisfied. Well, I think I just mentioned some work that went nowhere. I'm still holding out hope for some of the theoretical notions and, in particular, in relation to possible relevance to autism. I'm still trying to follow that up.

Markson: So can you reflect on the strengths, and perhaps weaknesses, of your research and theoretical contributions, the impact of your work, and its current status?

Watson: Well, I think that the strength of my work is that it's basically theoretical, and its weakness is that it's basically theoretical. Through time I have not really done a lot of good empirical research. I think I've done a little bit of good research and a fair amount of passable research. In reflecting on my career I would say that my contribution to the field has been largely theoretical. In particular, the theoretical constructs that I think may have some future are, on the one hand, that contingency is a feature of the environment that can operate like a stimulus. The novel aspect of the idea is that, prior to my introducing it, one generally conceived of contingencies as being important because of the stimulus value of whatever stimulus was in them, and possibly some added value from the fact that you could control that important stimulus. But the idea I was floating was that the contingency, in and of itself, might be a signal to the infant or, as I speculated, to many other animals. The stimuli that were involved might gain value, might become powerful stimuli, simply because they were imbedded in a contingency. That idea was, I think, taking shape in other minds as well. When I first proposed it I was able to cite other people that I believe were thinking along the same lines. Latane was one such person who briefly spent some time working on how rats seem to change their social orientation towards human hands when they were handled early in their life. They began treating these hands as if they were social objects, treating them like other rats. That idea struck me as being very close to the idea I was developing to the effect that infants would decide that an object was important if they could control it. More specifically, I thought that initial contingency experience could be an ethological signal. For the infant the contingently responsive object signaled the condition of its being a social object, a special object towards which you would orient and to which you release social behavior—e.g. smiles, coos. The other contribution is a related one, I think. It is the framing of the contingency analytic process as a conditional probabilistic analysis, inclusive of a time window, as explicated in a paper in 1979. This is a different view, as I think I mentioned earlier, than very recent uses of Bayesian analytic conceptions of infants' minds. As to the current status of these ideas, I'm not exactly sure. I think they may still have some future to go before they lose out to better ideas. There's some contention, at the moment I'd say, about the importance of contingency perception in the identification of fellow members of species, especially of my proposal of the role of perceiving high but imperfect contingency. This is a refinement of the so-called “Game Hypothesis” of a paper I wrote in 1972. That hypothesis implicitly denied that the baby, at birth, was given sufficient identification information for aligning itself with fellow members of our species and that there was this special role being provided by the infant’s contingency analytic capacity. But there seems to be a fair amount of recent data that infants are capable of making direct sensory—e.g. visual, auditory—identification of humanoid objects in the world. So, the Game theory is, to some degree, wrong if those data are sustained with future research. Still, however, in my mind it is possible that there is a low order orienting power to humanoid stimuli for babies that needs to be—underline *needs to be*—augmented, supported, and confirmed by the perception of appropriate contingency with that object. Some work that seems to support that notion is the work on agency perception by Lisa Johnson.

Markson: Susan Johnson?

Watson: Susan Johnson. Thank you. Susan Johnson. And so I would hold out hope that that theoretical notion has a few more years to live.

Markson: Okay. So building on that, I guess, what work of yours do you think best represents your thinking about child development? Which of your studies are most significant or which contributions do you think might be the most wrong-headed then?

Watson: Well, in terms of long-term future, I think the view that infants are using lots of information in their analysis of contingencies in the environment is a reasonably good bet for holding true and producing future significant findings. There is the recent work in language perception by Aslin at Rochester that has moved in that direction, framing syntax learning in Bayesian terms. So I do think that giving the baby credit for using high order information about conditional probability is probably a good idea and will be for some years to come.

Ideas that I was thinking were possibly wrong-headed in my experience and not likely to be worth somebody venturing forward into are those good works aimed at accomplishing dramatic rehabilitation. These things are such long shots. The odds are so small that it's probably not worth wagering one’s career trying to cure dramatic capacity loss. However, I don't feel bad about having done what I did in that regard. But I would not recommend it for another person as a rational choice.

Markson: So can you reflect a little bit on your experiences with research funding agencies or apparatus over the years, such as your participation in shaping research funding policy, implementation, study sessions, getting support for your own work, and other related matters.

Watson: I don't have much to say here. I was not a very successful fundraiser for my research, though I did get as much as I think I probably deserved. And I did participate on an NSF panel, back in the ’80s. I found that very interesting and it provided some insight into how government works and how people can work as professionals on such panels in support of what they see as good research. I found that these panels worked better than I had imagined in the way in which people gave very thoughtful consideration to proposals and the way in which they tried to be fair in distribution of money and tried to not be over generous to their own bailiwick of research. So the people that I met and formed my opinion by were altruistic in the good sense of science for science.

Markson: Okay. So you've already said quite a bit about the institutions that you've worked in. But maybe we could get the specific dates and capacities of your work at Merrill-Palmer Institute and also at UC Berkeley.

Watson: Okay. I worked at Merrill-Palmer from 1963 through 1967. And then I came to Berkeley and was there from 1967 until I retired in 1994.

Markson: But you were still very active, actually, after your retirement. That’s when you began collaborating with Gergely.

Watson: Yes.

Markson: And you still are.

Watson: Yes. The major reason for taking early retirement was in fact to avoid large class teaching and to escape some of the residue of what I have mentioned of my personal deficits. I knew that the teaching load was on the verge of rising and large class teaching was going to be extended. I was suffering from the large class teaching that I was already doing and it seemed to be getting harder rather than easier. So that was the inspiration, but I have tried to continue doing intellectual work since then. The University is very supportive in allowing one to do that. It even gives you a small amount of research funds to help support it.

Markson: Okay, now for institutional connections. Were you connected with a research site? Describe your role with that facility and please also describe the changes in this unit that occurred during your time there. What objectives were being pursued, what achievements and frustrations were encountered, and what role do you believe you played. Also, what role do you believe was played by that unit in the history of child development research?

Watson: Well, I'll start with the Merrill-Palmer Institute, which was a very interesting and, I think I mentioned, important place for me. As I also mentioned, it was on the forefront of two things: Piaget and infancy research. Irv Sigel was the research director. Irv was an early promulgator of Piaget in this era that Piaget was making his return as a theoretical influence in the field of development psychology. That was the period of the late '50s and early '60s. Irv was doing research of that kind. He even managed to get Piaget to visit Merrill-Palmer Institute. This was before my day. I arrived in 1963. I mentioned earlier that Irv had started the infancy conference, which was a really extremely important contribution, I think. The Merrill-Palmer Institute, as a whole, was diversified in many ways and overextended financially in many ways. And shortly after I left in 1967, it collapsed financially, having been around for—I think at that time for 45 years or more. It collapsed and was taken over by Wayne State University, but most of the programs were dissolved or reduced considerably. They had lived for many years on monies from the Ford family and that apparently just wasn't sufficient for them to carry forward past that time. Carolyn Shantz, with whom I collaborated on a couple of papers while at Merrill-Palmer, moved to a position in the Psychology Department at Wayne State.

When I came to the Psychology Department at Berkeley I very soon became associated with the Institute of Human Development. That institute has a long and grand history, from the early '20s again. The longitudinal studies it initiated were probably the most important longitudinal studies in the world for many, many years. There are other very significant longitudinal studies now, but none longer and none that had, I think, as much influence for so long. The influence was with respect to physical growth and also with respect to psychological development. Initially, I believe, the physical data were more influential than the psychological.

Many changes occurred to IHD during the time I was associated, between 1967 and about 2000. For many years, up through the '70s or early '80s, I was on the executive committee and so I did have an inside view of how things were going. Through time, I would claim two things have happened to the Institute. One is that it has shifted its emphasis from the longitudinal study and the sort of naturalistic, observational frame to a more experimental frame, with a number of different laboratories. This remained pretty steady during the 10 years that Paul Mussen was director. In recent years, it has shifted to a commitment in the clinical and potentially applied frame of activity. This shift began under the influence of Joe Campos and accelerated under the influence of Phil Cowan and Steve Hinshaw. I think that it probably will be carried forward as Jonas Langer now takes on the directorship.

So the character of the Institute, I think, has changed, much as the field has changed. I think the field of developmental has shifted from primarily theoretical issues to more practical issues, and from pursuit of primarily psychological explanations to more biologically based explanations. I don't mean these are now dominant perspectives, only that they are the direction in which the field is moving.

Markson: Let's change gears a little bit and talk about your teaching. If you could describe your experience as a teacher of child development research and/or a trainer of researcher workers, any graduate students, and also course—what kind of courses you have taught. And you might also, if there's any tension, want to comment on that tension between teaching and research.

Watson: Well, I think that I'll deal with the tension to start with.

Markson: Okay.

Watson: There is classically, of course, tension between the time you have to teach and the time you have to do research. And so that certainly was there, as it is for everybody teaching. For myself, there was additional tension, as I think I also alluded to earlier, regarding the teaching of a large class. The infancy course that I taught for many years, every other year, turned out to be made a requirement in the Physical Ed Department and so it drew many students, it always had a class size of a hundred plus or minus twenty and sometimes more than that. And this size of class, for my personal, psychological disposition, was very tension inducing. While I didn't mind teaching small classes, and didn't get very tense in teaching small classes, I always became very tense in the large lecture course, even if I knew everybody out there was thinking about something else and/or had a major in physical education. So I can't say that I enjoyed my large-class teaching at Berkeley. But I did enjoy topical seminars. Through the years I taught a variety to undergraduates and graduate students. I enjoyed a great deal a particular undergraduate course, a freshman seminar on the development of personal identity or self. I tried to bring a kind of enlightening experience to these freshmen that I had had upon arrival in college. When I was a freshman, I had a humanities course wherein I was exposed to things that I had never thought about before philosophically. So I taught this freshman seminar for a number of years to my great satisfaction, introducing a philosophical background to the problem of identity and the psychological process of developing a self-concept. It was well received.

Other courses I taught over the years were primarily about early cognition. They would be infancy courses inspired often by the works of Tom Bower or by the works of Michael Lewis. I took particular inspiration, I think I mentioned, from these people. I would design courses around topics that they were exploring. I did this for graduate students as well. I enjoyed, I think, virtually all the graduate training courses. Possibly an exception was early on, when the department had a different structure. It had been divided into three subparts, and the subpart that I was in was called Group One. We had to provide a first-year graduate seminar that would span all the topics that were relevant to Group One. There was about no topic that wasn't relevant; we had to cover clinical work and biological work and developmental work and so forth. That was a bit tedious. When the department changed, after about my first ten years there, we shifted to having subgroups in the more standard style of psychology departments. I was in the developmental subgroup. We had seminars for our graduate students that were much more closely constrained in topical content to our own specialization. Most of us find that easier to teach and enjoy.

Markson: When did you join SRCD? What were your earliest contacts with the Society and with whom? Describe the first biennial meeting that you attended, if you can recall.

Watson: Yeah—well, actually I'm not sure I can recall directly. But I believe I went to my first SRCD meeting back in 1963 or 1964. The Society was meeting biennially, so I probably could figure that out. I believe the president at the time was Al Baldwin who was the chair of the Department of Child Development at Cornell, and so I thought that was kind of special. I met a variety of people there, one of whom I would, later, have as a colleague in the Institute of Human Development. That was Dorothy Eichorn. And I'm sure I met many other people, like Jack Gewirtz for one, and the Carons.

Markson: Al?

Watson: Yes, Al and Rose Caron.

Markson: Oh.

Watson: And so that group was important as an early contact in the field, but it was not as important to me as the early infancy meetings.

Markson: John, can you describe the history of your participation in the scientific meetings and publications of the society?

Watson: Well, I participated in various meetings in the form of presenting papers and participating in symposia from year to year. And I served as reviewer for the journals *Child Development* and *SRCD Monographs*. I think that's about it.

Markson: Okay. Can you describe the history of your participation in SCRD government and if you—if there were any major problems or issues that confronted you during that time, if at all.

Watson: Actually, I never participated in its governance, so I guess I have nothing to say about that.

Markson: Okay. We'll just move on then. What do you think are the most important changes to occur in SRCD and its activities during your association with it?

Watson: Well, I think one thing that everybody's noticed who's been around this long is that it's gotten way bigger than it was back then. For some of us it's less fun, therefore, but not necessarily to its own detriment. I believe that it's doing a good job and has changed over time in the right direction, at least by my biases. That is, it has become a more rigorous and experimental research oriented organization than it was. And so for a developmental psychologist, I think, all those moves were in the right direction. At the same time, the journal *Child Development* matured. By my biased view, it became a more rigorous journal and, therefore, became more respected in psychology departments. In earlier days young faculty in psychology departments would've been discouraged from publishing developmental work in *Child Development* as opposed to looking for other places. And that was in the days before APA had a *Journal of Developmental Psychology*. *Child Development* was viewed as a soft journal, so-called. It is no longer a soft journal. I can certainly testify to that from the number of my rejected articles. But at any rate, I think that as a professional society it has grown. It has become, as I mentioned just a moment ago, like many aspects of the field of psychology, more biologically oriented. Another important thing that it did was to help spawn and support the development of ISIS, which is functioning sort of as its child attending to the enormous expansion of infancy research.

Markson: Okay. So we'll move into some questions about the field of psychology. Can you comment on the history of the field during the years that you were participating in it—major continuities, discontinuities.

Watson: Yes, I think that in the period of time when I arrived as a student and a faculty member behaviorism and its associated learning theories were losing their grip on psychology overall. The cognitive phase of modern cognitive developmental was beginning to catch hold. The early deliverers of support for this were Al Baldwin's influential textbook and John Flavell's book that summarized the extensive work of Jean Piaget. And the fields surrounding developmental psychology—e.g. information processing, linguistics, and ethology—were challenging the theoretical assumptions about learning and innate endowment. This surely had a contextual influence on the field. So developmental psychology has moved from a broad learning frame to a cognitive, developmental frame in my time.

A different sub-theme, but important in my mind in the area of infancy, is a shift that occurred early on in my career. Piaget had provided a major worldview for conceiving of infants and their early cognitive growth. Pretty much everybody was beginning to ride that bandwagon. And then, even though early in the growing wave of research in infancy, Tom Bower began introducing notions that were seemingly quite at odds, though seemingly also quite related, to Piaget. Bower later told me that Piaget loved the new findings. He had a very good relationship with Piaget and in fact visited him and had students in common. Tom could speak French. But his ideas that arrived on the cover of *Scientific American* and *Science*, back in the ’60s were mind bending for most of us. His creative research was showing how the infant might be construing the world and implied that they might have much greater endowment than Piaget seemed to imply. His additional notions thereafter, in terms of an idea of abstract endowment, also were of considerable influence. I see his work today being continuously mined. The fields that he opened up are the primary fields of infant research today. There is a lot of conflict, historically, around his role. Yet, clearly, he began a transition, in my day, from a cognitive frame that was largely Piagetian to the neo-Piagetian frame that now includes many people: Meltzoff, Spelke, Carey, Gopnik, Baillargeon, Gergely, and on and on it goes.

So that's one way in which the field changed. Another way the field changed was a product of a conceptual duel—a productive duel that was going on in the ’40s between Learning Theory and Freudian Theory. And so, if you were in developmental psychology or child development, you would be tossed back and forth by claims from the Learning camp and from the Freudian camp. People were straining at trying to figure out who had the best frame here because they seemed so different and yet both seemed so relevant. Influential books by Whiting and Child and by Dollard and Miller and later by Sears, Maccoby, and Levin highlighted the issues. Well, as I just mentioned, the learning view transformed in my day to a view of cognitive learning and cognitive development, largely influenced by Piaget. In the area of socio-emotional development there was a transition from Freud to Bowlby. Bowlby, in the late ’50s, brought a new frame, an evolutionary frame, that would alter the kinds of questions one would ask about how an infant was adjusting to the world as a social creature. The work of Mary Ainsworth, Mary Main, Alan Sroufe, and many others are continuations and extensions of this Bowlbian perspective.

Markson: Okay. And what are your hopes and fears for the future of the field?

Watson: I guess my fear is that we will become over biologized—if I can make up a word—and that we will spend too much effort on scanning brains. We might lose track of some historical interests in the nature of the development of individual difference, the nature of conceptual forms of thought, and spend our time mapping the pathways of the neurostructure and the subunits of neurostructure as our major effort. My guess is that this will not last terribly long, but then I'm probably wrong and they'll invent a new machine that gets even better for online mapping and we will continue with our interest in mapping. I do not say this with any degree of assuredness or even with malice. I do believe that these are important pieces of the pie and that this work should be done, but that this work should take center stage and that the good new thinkers be enticed into that one section of the field would be a serious risk factor and therefore my fear.

My hope is that the field will continue in a united, progressive transformation of what it's been invested in in the past, going through, perhaps, an easy structural transformation of cognitive reorganization in a Piagetian-like move forward rather than a radical shift to the side in this biological frame. My hope is that it will move forward with transitions that keep the momentum of the interesting ideas about cognitive structure, about social organization, in particular, about evolutionary influence on development. So if I have a hope it's that something like the Bowlbian transition of the Freudian perspective take place; integrating the new biological findings with cognitive and socio-emotional perspectives, awakening our minds to the idea of a newborn being an evolved creature that's organized as a member of a system, a creature with certain predilections and propensities given to it through evolution. These are the directions in which I think major productive findings may occur.

Markson: Okay. So, in closing, I know you've already mentioned a lot about your family and personal experiences and interests, but I just want to see if there's any other things that came to mind that you'd like to share, especially ways in which they might have bearing on your scientific interests and contributions.

Watson: Well, let's see.

Markson: Is there any additional background?

Watson: I think that I should mention the fact that my wife has played a significant role in my development through time and, I think, largely because of her own diversified path, having been a graduate student at Cornell in philosophy, sort of a bedrock place in the '60s for linguistic philosophy. Her interests as a philosopher were in the explanation of behavior, that's what she was intending to write her dissertation on. She did change after having our first child into a person who wanted to do more practical work in the world. So she went on and got a Ph.D. instead in educational psychology at Berkeley and has worked ever since in the area of prosocial development. She taught for a while at Mills College and then joined the Child Development Project as Director of Program Development. Her recent book, *Learning to Trust*, is a distillation of her theoretical perspective on classroom management. We always try out our ideas on each other first. It's been like having a bright colleague at hand whenever I needed one.

I should also like to return to the influence of my father. I gave him rather short shrift by only noting his dismay at my early learning difficulties. He had suffered great misfortunes in life, with the loss of his first wife, the crippling of his son, the loss of his career, and the poverty that followed. He had been head of the foreign student section of the Boston YMCA during the 1920s. He lost his job in the Depression shortly after marrying my mother. They eventually moved to Connecticut where my mother got a job as county club agent for the 4H club and he found a job as a timekeeper in a brake factory that left him smelling of burnt rubber. When the Second World War broke out, he got a job in an airplane engine factory, again as timekeeper. Amazingly, he never became seriously depressed. He and my mother sang in the church choir. He thanked God for our blessings at each dinner. Although he was a bit self-righteous, he was a truly kind person and did many things to support my education, from early teaching of vocabulary and mathematical short cuts to eventually seeking loans for me when I lost my scholarship in college. And he was always a non-pushy big fan of my athletic endeavors. While my mother provided the self-concept support I needed, my father set standards of duty, integrity, and perseverance. These standards were reinforced by their display in my crippled brother Bob who went on to be editor of a newsletter for handicapped people on the west coast called *The Rebounders*. I know this sounds sappy, but I am sure these familial inputs were formative in both initially orienting and ultimately sustaining my career in developmental psychology.

[End of interview]