Charles Brainerd

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Major Employment:

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SRCD ORAL HISTORY INTERVIEW

Charles Brainerd

Self-Interview
At the University of Arizona
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Hello, this is Chuck Brainerd speaking from the University of Arizona. This tape was made on September the 20th, 1998, and I'm responding to the Society for Research in Child Development's Oral History questionnaire.

With the general history part of the questionnaire, the first question concerns family background.

I was born in Lansing, Michigan on July 30th, 1944, in Sparo Hospital. Both of my parents were very strongly oriented toward education. My mother was a teacher. She had been trained in the old county normal system before colleges of education. My father worked for General Motor's, he was an engineer, a safety engineer specifically. However, he had gone to college during the Depression, early 1930's. He finished two years of college before he had to quit for financial reasons, and his objective all along had been a career in education. He'd hoped to be, for example, a high school mathematics teacher, and to perhaps coach. He was unable to do that, and certainly that affected his attitudes towards education, and my mother's, throughout their life.

I grew up in the Lansing, Michigan area. For the first ten years of my life I lived in a small town called Dewitt, which is just north of Lansing. When I was ten we purchased a farm, north of town. I went to high school in a town north of there that's called St. John's, for a farming community is was a very academically oriented high school; something like twenty-five percent of the kids in that high school went onto college, which was unusual for a rural high school at that time.

As far as college was concerned, that was a foregone conclusion throughout the time I was in high school. It was really only uncertainty at that point, whether I would go to Michigan State or to the University of

Michigan. Back at that time, at least in the area that I grew up in, it was extremely rare for high school students to go farther away to another university. We discussed whether I would go to University of Michigan or Michigan State, pretty much throughout my senior year, but my mother and father had both gone to Michigan State. And so had my grandfather for a short period of time. And some of my older cousins were going to Michigan State, so there really was not much of a choice there, and certainly my father made it clear that Michigan State was his preference. And I also won a scholarship to go there from the local Lions Club, and was given scholarships by Michigan State through their academic recruitment process, so the transition to Michigan State was fairly smooth. Also, since we lived not far from the campus, that meant that I would not have to be in a residence on campus and could drive back and forth, which was certainly not allowed for most freshman at that time, so it all seemed very convenient.
I had no military service during that period of time. And although ROTC was mandatory, so that most students of my age had some military experience, the mandatory ROTC requirement was lifted in my freshman year, so I did not participate in ROTC either.

As far as my education at Michigan State was concerned, I began in my first two years studying natural science; zoology, chemistry, organic chemistry and so on, and switched to psychology in my junior year.

My early works experience during this period of time, I had, of course, worked on the farm during my high school and junior high years and had also been a regular participant in Four-H. Essentially what my early work experience on the farm did was to convince me that that was not what I wanted to do with my life. Most of the sons and daughters of people that owned farms near to us were in the continuing inter-generational tradition of the farm being passed from the parents to the children. I found that I wasn't interested in that at all. I think, perhaps, I was simply confirmed in my work on the farm in something that John Kenneth Galbray said some years after, that I heard, namely that, 'when one grows up on a farm, nothing ever really seems like work again.'

I now want to move on to the second question under general intellectual history, that is, my early adult experiences that were important to my intellectual development, and my collegiate experiences.

Let me take the latter first. I had an experience as a sophomore in college in the spring term of my sophomore year, that was formative in encouraging me to move into psychology. I had had an undergraduate colleague of mine, with whom I was extremely competitive, as far as grades were concerned, and we were mainly taking chemistry, and zoology, and that sort of thing together. However, science majors were required to take what were called art's electives back then, and in his fall quarter of his sophomore year, he took as an art's elective introductory psychology. I took something else, anthropology, I believe. And in comparing our grades over Christmas break, I discovered that he had gotten a 'C' in psychology, and this was unheard of, and it lead me to conclude that there might be something very substantive and very interesting in psychology. So I decided to put it on my list of art's electives. I was unable to get it on for the winter quarter; however, I managed to get it on for the spring quarter.

And I was very fortunate in the person that I had as a professor. Normally, of course, introductory psychology courses are taught by beginning professors who are still trying to find themselves, but I happened to have a senior full professor, Ray Denny, who also happened to be a former student of Kenneth Spence's. So I was immediately exposed to the – what was then the dominant tradition in psychology, the Iowa experimental tradition. And, of course, this was very, very important in tweaking the interest of someone who was coming into psychology mainly from the natural sciences. If I had had the more typical introductory psychology course taught by a junior professor in social or personality or clinical, I might have been confirmed in the bias that all natural science students had at that time, that psychology was really not a natural science at all. But fortunately, I did not have that kind of professor, and I was exposed to someone who strongly emphasized experimental research; mathematical modeling and that sort of thing, and I thought that I was really in my element.

Let me move on to the first part of that question now, early adult experiences that were important to my intellectual development. What I'm going to do here is mention an experience that encouraged me to get into the specific field of research that I then got into, rather than the experience with Ray Denny that got me to move into psychology. After I had moved into psychology in my junior year, I was, of course, taking the usual array of undergraduate courses in psychology; learning, perception, personality, child development, social, that at that time defined a major in psychology. After I took introductory child development, I then took adolescence, which most people at that time took either adolescence or infancy after they took introductory child. And I had as my professor, a very senior child development person by the name of Harold Anderson, very well known at the time; had done some of the early studies of the authoritarian and democratic teaching styles in the late 1940's and early 1950's.

Now Harold at that time, he and his wife Gladis, had been concentrating on research in adolescence, and had been for about a decade; they'd published a very well known book in the area. Harold was, in the spirit of the 1960's, to say the least, this would have been 1964, a rather touchy-feely person, and that's how he taught his course. To give you an idea of what the orientation of the course was, I think it would suffice to say that the main textbook was A.S. Neil's, Summer Hill. This, of course, was not the sort of course that intellectually I found very congenial, and I was horrified by the prospect of having to complete the major assignment in that course for a grade, which was a term paper. And the material that had been assigned for a term paper was A.S. Neil's, Summer Hill, of course, and books like that, so you can imagine the sort of essay that Harold was expecting to get from everyone. I, of course, thought myself utterly incapable of writing such an essay, and I spoke to him about alternative possibilities. It turned out that although Harold found Piaget's work an anathema to his own, this very biologically oriented research, with the use of mathematical logic and abstract algebra to model human behavior, he thought was horrible. However, he admired Piaget as a genius. Indeed, Harold had taken his Ph.D. at the University of Geneva in 1929, and with other graduate students had often had dinner at Piaget's house, so he admired Piaget tremendously. And he thought that anyone who was willing to write on Piaget, that that indeed was a very noble thing to do, and he would tolerate that, even though it was utterly at variance with the aims of the course.

So I went and spoke to another professor, Charles Hanley, who was an expert in that particular area. He put me on to John Flable's book, The Developmental Psychology of John Piaget, which had been published about a year and a half before that. I studied the book carefully and was particularly struck by the sections that dealt with the use of abstract algebra and mathematical logic as modeling techniques. And I wrote a term paper for Harold Anderson's course that was concerned with the use of algebraic models in cognitive development, and he gave me an A+. Whether or not he ever had read the paper, I don't know. I have my doubts, because there was not a single comment written on the paper, though the other papers that some of my colleagues in the class were written upon profusely.

Let's move on to the third question under general intellectual history, first of all, the origins of my interests in child development.

Well, I would say that the main one was a content orientation. This is a rather complex answer. It wasn't so much that I was interested in child development per se, as that I found child development to be a potentially interesting venue for the kind of research that I wanted to do. Let me explain that a bit.

At the time I entered graduate school, fall of 1966, and at the time I was an undergraduate in the four preceding years, this was still the hay-day of learning theory in mainstream adult experimental psychology. There was no cognitive psychology, as we know the term today. Indeed, there was no psychology of memory. In that era, memory was still called verbal learning. If you go to one of the standard textbooks in conditioning and learning, a real bible of that era, Gregory Kimball's revision, '61 revision of Hillguard and Markesis, *Conditioning and Learning*, you with find that there is neither the term memory, nor the term cognition in the index.

Now, I was very interested in studying complex cognition, and let alone – that is to say, something even higher cognitive than memory. And in mainstream adult experimental psychology there was basically none of that except for a small tradition associated with Gestalt's psychology, people like Karl Dunker and Wolfgang Curler, for example. Curler had dabbled a bit in it, but Karl Dunker, and also the Luchen's phenomena, such as Einslung, for example, in cognition. The problem was that this was clearly viewed as a minority viewpoint, and it was also viewed as rather spooky and unscientific work. And from my own part, when I reviewed the Gestalt oriented work, I found it to be not very interesting theoretically, that is to say it was mainly, I thought at that time, a kind of elucidation of – I wouldn't say an elucidation of – an illustration of Gestalt perceptual principle's in higher cognition. That is to say, cognition equals perception in some sense. Whether that be true or false, I didn't find it an orientation that was likely to open outward into major new theoretical ideas, so that didn't seem an interesting way to go.

In child development, on the other hand, by the time I entered graduate school – between the time I had been an undergraduate, and the time I entered graduate school, Piaget had basically come to dominate the landscape. And Piagettian theory, as opposed to learning theory, although the learning theory tradition remained extremely vibrant in the work of people like Howard and Tracy Cander, Albert Vandura, much of the work applying Skinner's ideas within atypical children – 'a token economies' for mentally retarded children, for example. Although learning theory was extremely popular within child development, and it was a strong intellectual force, Piaget's work had really come to the fore, and that's what people were doing. Now, Piaget's orientation, of course, was high cognitive. So I thought that child development would be a very attractive venue to study my own interest in cognition, which came about primarily because of my interest in mathematics. I had been very interested in branches of mathematics, such as abstract algebra, and mathematical logic. It was sort of a hobby with me as an undergraduate, and in my first year in graduate school. And I really wanted to study mathematical cognition at that time, so that, of course, is what Piaget was doing, so child development seemed to be very interesting to me.

The next question under number three, it concerns the individuals that were important to my intellectual development.

The main ones were, as far as faculty at Michigan State were concerned: Ray Denny, who had really been an intellectual mentor since the time I was an undergraduate. Hiram Fitzgerald, Gordon Wood. Gordon Wood is now chair of the department at Michigan State and has been for some years, and Hiram Fitzgerald is associate chair and director of graduate studies, and has been for some years. Another one was Ellen Strommen, who was ultimately the director of my Ph.D. dissertation. Charles Hanley, who had become Dean by the time I was in graduate school, but whom I had taken many courses from. And then finally Abraham Barg, he was the resident expert in verbal learning, and had been a student of Spence's. I must say that as far as all of these people were concerned, what was important about them to my intellectual development was really style rather than content. I had a clear idea of what I wanted to do as far as content was concerned, and what I wound up doing was nothing that was closely related to any of their research interests. However, of what they gave to me was a style of doing science, and an orientation toward doing science that proved to be very influential.

The next part of this question asks, who my research mentors were?

Well this is a rather difficult question. I take this question to mean something different than people who were generally important to my intellectual development; people who specifically influenced the nature of my research. I would say two things about that: The three most important people in this category would be Piaget, Piaget and Piaget. I steeped myself thoroughly in his work while a graduate student, because his literature was the main literature in the development of complex cognition at that time. So his work, his theoretical ideas were my – functioned as my main research mentorship, which is not to say that Piaget had any direct influence on me, other than through his writings. I never met Piaget, nor have I ever been to Geneva.

I would add one thing under research mentors though; I was considerably more independent and was not seeking of research mentors the way most graduate students were at that time. I think there were a couple of reasons for that, one financial, and one more social. On the financial side, I had my own independent research support, so it wasn't necessary for me to be an RA in someone's laboratory, and sort of get into their lab and run their research to get familiar with a particular area. National Science Foundation had been generous enough to give me a pre-doctoral fellowship in my first year. Pre-doctoral fellowships at that time paid for tuition, paid a stipend, and also provided a not insubstantial budget for research, so it allowed graduate students, at least in psychology to be functionally independent of faculty members for financial support. So I was able to buy my own research equipment and run my own experiments on my own. I also had in my second year in graduate school; I applied for, and received, an NIH grant to conduct research on cognitive development. So in my last year in graduate school I had my own research grant the way any other faculty might have, the only complication arose there in that I was budgeted as a research associate in the grant and I was still pre-PhD., so I actually took a research associateship at Michigan State for a period of a few months after getting my PhD to complete that grant.

Finally, as far as my significant colleagues were concerned in graduate school, I take that to mean other graduate students as well as opposed to faculty members. It might include faculty members if I had published with any faculty members, but I did not. All of my publications while in graduate school were either by myself or in collaboration with other graduate students. The main one, the only one with whom I published, was Terry Allen. Terry Allen was a student who came to Michigan State as an undergraduate from Oklahoma, then went on to graduate school there. He studied infancy, particularly, perception and attention, and he and I did some early work together. We wrote the first review of the Piaget learning literature in our middle year – summer of our – after our middle year in graduate school, and we conducted a series of experiments together, and we also wrote part of a book together.

I should also add under this question, that the fact that I had a NSF pre-doctoral fellowship, and then later an NIH grant, freed me up from teaching responsibilities as well. So another way in which graduate students become closely involved with faculty members, who then turned out to be their research mentors is, of course, serving under them as teaching assistants, which I did not do either in graduate school.

The fourth question asks what political and social events have influenced your research and writing?

Well, I couldn't truthfully say that there are any political or social events that have directly influenced my research in writing; directly I say. In fact, I think that would be bad science. Science should be by it's nature, impersonal, and as much as possible, if it's going to be valid, should not be colored by political or social events, though it may certainly reflect them. And though the choice of research topics inevitably with the availability of grant funds and so on, may reflect current social and political emphases, but I don't think they should directly influence the research you're writing.

As far as indirectly is concerned, I think there were definite influences of this sort; of course, the 1960's were the 1960's. It was an era of amazing expansion of individual human rights; first the civil rights movement on campus as well as off campus. And a time in which we discovered, America as a whole discovered, the fundamental weaknesses of authorities upon whom we had depended. I think that one cannot live through an era like that and not be influenced. It develops a certain level, of shall we say, independence from the views of others. If we think of Julian Roders old internal, external control scale, it nudges people toward the internal control end of the scale, and therefore, creates in them an attitude where they are perhaps not as highly dependent, or think they should be as highly dependent, on the attitudes of other people in choosing their research topics, and in choosing what their going to write about it. It generates a certain fearlessness to take the opposite view, or to say that a dominant theoretical view is wrong. Certainly I did a great deal of that with respect to Piaget's theory, and I think that the social and political context of the 1960's probably encouraged that.

This question also asks whether or not social and political events, which ones had influenced my teaching?

I would say much the same kind of thing, teaching in child development, whether in graduate school or undergraduate school, should precisely and correctly reflect the state of our science, of course. But, I do think that at a more general level, the social and political currents of the 1960's affected my style of teaching, and that, in particular, it is a more student-oriented kind of style than a style that's much more accepting of the opinions of students. A style that attempts to take time to educate students rather than to make them simply memorize what I'm saying, and that is certainly a very different approach to teaching than I was exposed to in the 1950's and the early 1960's. Whether I had – if I had not come through the 1960's, whether I would have adopted that style of teaching, nevertheless, I think is perhaps doubtful.

Let's move on to the first question under personal research contributions, namely what my primary interests were in child development at the beginning of my career?

They were primarily in three areas: higher cognitive development at all age levels; measurement, that is to say, a question such as reliability and validity and the how's and the why's of measurement in development psychology, particularly in the infants and young children where it's difficult; and then finally in development theory as well. Within all of this, I would say, that a major interest of mine was how the learning theory tradition in mainstream experimental psychology might interact with, how it might be related to, how it might interface with, the then dominant developmental tradition, which was Piaget's theory with it's stages and it's emphasis on readiness and so on. And many of the early papers that I wrote revolved around this particular topic.

The second question under personal research contributions, namely what continuities are most important, or most significant in my work?

Let me mention just two that are at a very general level, and this constitutes very strong continuity. Throughout my career, my emphasis has been on experimental research, and on the evidence that results from experimental research as a basis for the science of child development. One could argue that I am basically an experimental psychologist, or what in the 1960's was called an experimental child psychologist. This distinction has to a greater or lesser extent, disappeared from the lexicon, but in the 1960's people used to argue vociferously about whether they were a developmental psychologist or a child psychologist, or an experimental psychologist. A child psychologist was viewed as someone who primarily studied children, or the uniqueness of childhood, usually with a clinical or social bent. A developmental psychologist was viewed as someone who studied age change, but perhaps from a more biological perspective; really reflecting the tradition that began with G. Stanley Hall of Darwinian thinking being the basis for science of behavioral development. And finally, an experimental child psychologist was viewed as someone who applied the techniques of experimental psychology to the analysis of age change. An experimental child psychologist might be someone, who like a child psychologist, focused on a particular age level as, for example, Bandura's work did, or work on token economies with retardates in that era. Or a person who was an experimental psychologist might be someone, who like myself, was extremely interested in age change, but approached it from the point of view of the design and techniques of rigorous experimental child psychology. I may say that, although these distinctions have largely disappeared I think today, certain fields within child development have become the home of experimental child psychology, and the main area that I would cite would be infants, and the study of infancy. The people who are senior people in the infancy field at this time were people who were trained in the experimental child tradition in the 1960's. Certainly I was one of these. Hi Fitzgerald was a leading young experimental child psychologist at that era. I myself, I think, would call myself primarily an experimental psychologist, who studies development. If it had not been for a fact that I mentioned earlier, namely that mainstream adult experimental psychology in the 1960's had no interest to speak of in the study of higher cognitive processes, if it had I probably would have wound up as what nowadays we call a cognitive psychologist, but it didn't, so I studied cognitive development experimentally.

Another part of question number two is what shifts have occurred in my personal research contributions?

Here, let me mention two: one substantive and one technical. Or, let's say one theoretical, and one as far as research content is concerned, on the theoretical side. A major shift that occurred is that I have gone more and more away from qualitative theories, or verbal theories, which dominated the theoretical aspects of my research, immediate post-graduate school up until the late 1970's. And I've gone more and more in the direction of mathematical theories, or let's say verbal theories that are precisely implemented as mathematical models. There are a number of reasons for that, technical, you can't really have a precise theory in psychology unless it's implemented as a mathematical model of court. But one reason was simply partly historical, or I wouldn't say motivational, but it has to do with time. If you look at the work of leading theorists who emphasize mathematical modeling, you'll see that they didn't do it very soon after they got out of graduate school. The fact of the matter is, that unless one happens to be trained in one of the leading mathematical psychology programs, of which there are not very many, it simply takes time to do your own personal post-graduate studies, to develop the necessary techniques, and the necessary history of the field and so on. And that's what I did in the 1970's; I read and did my homework in the evolution of modern mathematical psychology until I reached the point where I could do this work on my own. And the first two official mathematically oriented theoretical papers appeared in 1979. One, a paper in *Psychological Review* entitled “Marcovian Interpretations of Conservation Learning,” which applied the technique of Finite Marcov changed to the learning of Piaget’s conservation concepts by children. And a paper on the same topic that appeared in French Journal in the same year.

The other major shift in my work is that I have moved more and more in the direction of the study of memory and memory development, as opposed to higher reasoning abilities. It’s interesting because when Piaget’s work was dominant in the late 1960’s and 1970’s, memory and memory development was treated almost as a given. It was treated as a rather uninteresting phenomenon that did really not contribute directly to cognitive development. The idea was that on most kinds of reasoning problems children had to have a certain level of memory capacity in order to be able retain the information in the problem, long enough to solve it, of course. But that, beyond that, memory really did not contribute much to higher reasoning itself. I became more and more convinced in the early 1980’s that that view was incorrect. And that understanding memory, understanding memory development, and understanding the nature of the representations that children retrieve while they’re solving problems, and the nature of the retrieval operations was the real key to understanding even the most complex forms of higher reasoning. And that’s what I’ve emphasized since that time. And the development of fuzzy trace theory, for example, in collaboration with Valerie Reyna, is my attempt to translate what I’ve learned into a workable development theory. That theory is mainly concerned with the interface between memory and cognitive development. It attempts to say, “What’s going on on the memory side that’s relevant to reasoning, and how those developments on the reasoning side interface with specific kinds of reasoning skills.”

The final question under number two is what events were responsible for this shift?

Well, I really have explained those in the mathematical modeling area. I wanted to do that all along. I thought that was important work to do, but it simply took me time to build up the necessary skills to do the work. And of course, my shift toward research on memory development has been motivated by conclusion from my prior research program on complex cognitive development.

The next question, number three, under personal research contribution, is concerned with reflecting upon the strengths and weaknesses of my research, and theoretical contributions also on it’s impact and it’s current status?

Let me talk about strengths and weaknesses of both my research and my theoretical contributions, but to do it separately, and do it in very general terms. As far as the strengths of the research are concerned, I think there are two. First of all, since my research tends to be experimental and conducted with a strong emphasis on questions or reliability, I think that my data and the trends that I have reported are reliable; they’re data that can be accepted by other investigators who can then move on to other research of their own, in the belief that those data are trustworthy. I cannot think of a major finding that I’ve reported during my career, which has proved to be unreplicable, because I’ve always emphasized replication before publication. That’s a technical matter, a social matter for the science as a whole.

As far as the findings themselves are concerned, I think the main strength is that during the course of my career I’ve produced a number of counter-intuitive findings that have stimulated people to think about new explanations. Certainly, early in my career I demonstrated that, it not only wasn’t difficult, but in fact, it was rather easy with a number of techniques to train Piaget’s conservation concepts, which people thought were largely impossible to train, and in John Flable’s words in 1963, “The training did not cut very deep, even when it was modestly effective.” I demonstrated in my research in the 1970’s, that that simply was not true. I also demonstrated at the same time that the kinds of sequences, or so-called invariant sequences in cognitive development, were much more variable than people had thought and were very sensitive to measurement kinds of considerations.

Later on in my career I would mention phenomena such as the memory independence affect in cognitive development, a highly counter-intuitive finding, in which it turns out that in simple reasoning tasks, simple problem-solving tasks where you give the child some background information and then let them solve a problem, that children’s ability to accurately remember the background facts tends to be statistically, completely independent of their ability to solve the problem, and in some cases the two are mutually interfering. A finding that violates both Piaget's and the neo-Piagettian view of the relation between memory and cognitive development, and also the corresponding view of the information processing tradition.

Another highly counter-intuitive finding is the cognitive triage effect, in which people actually read information out of memory in reverse order of it's strength; an ability that develops markedly with age. I could give other examples, but as I've said, across the course of my career, the strength of my research, I think, is that I've been able to produce a number of counter-intuitive findings. I think that there's a very good reason for this, and that is, as I said before, my emphasis is on experimentation and on evidence as a basis for conclusions. I try as much as possible to maintain no commitment whatsoever to beliefs about what the data should show, including my own. Nothing makes me happier than being able to write a "Brainerd is wrong," paper about something that I have reported previously. And if you take that kind of attitude toward experimentation, then you will be more readily able to recognize counter-intuitive findings when they jump up and bite you in the laboratory.

I think the weakness of that sort of research, and I've seen it again and again, is that, of course, if you publish a highly counter-intuitive finding, it takes a very, very long time for people to digest it and believe it. I became aware of this very early in my career when I began reporting findings on conservation learning and variability of invariant sequences that upset some people to the point of apoplexy. I couldn't really understand why until I understood that people were having great difficulty digesting those findings and fitting them with the gist that they already of that particular research topic. So it does take a long time for counter-intuitive findings to be even believed. And then it takes an even longer time to figure out what their theoretical implications are, especially if they fly in the face of some very comfortable, intuitively reasonable sort of theory.

On the theoretical side I would say that my main contributions are first, to encourage development people who are interested in doing development theory, to think mathematically, to do mathematical modeling as a basis for theoretical development. And also to exemplify that to them, that – and this may be the more useful aspect of this emphasis, that to provide examples through my own work of workable, theoretical ideas that are implemented in mathematical models that can then be used to go on and measure in a precise kind of way, developmental changes in the processes that underlie behavioral development, that we're all, of course, most interested in.

Another aspect of my theoretical contributions that are important, I think, is to encourage, and exemplify again to developmentalists, the importance of looking at basic processes; interference, sensitivity, the nature of memory representations and so on, basic processes, call them unconscious processes, automatic processes if you will, but to look at the connection between those things and the development of higher cognition, that that's a very productive way to go in understanding the development of higher cognitive ability. And I mean that in a very specific sense. I don't mean it in the information processing sense where attential skills and short-term memory capacity and so on, sort of serve as a, if you will, 'brain grease,' that is not specific to the development of abstract reasoning abilities, rather I mean, interference sensitivity, attential processes, representation and retrieval processes as being something that's very specifically connected content wise, to the development of, shall we say, mathematical reasoning, logical reasoning, spatial reasoning and so on.

The final part of this question asks about the impact of my work and its current status?

Well, I would say that the impact of my work, at least measured by normative indexes, such as citation levels and discussion of the work in major handbooks and so forth, has been substantial. I should say pleasingly substantial for someone who grew up on a little farm in northern Michigan, and went to Michigan State to graduate school. As far as the current status of the work, I am heavily invested intellectually at the moment in continuing to develop F—Tray's theory, I think really the work that's been done to this point is only prolog for what really needs to be done, and my research program in memory development, and the relation between memory development and higher cognitive development is continuing.

Since this side of the tape is about exhausted, I'm going to move onto the second side of the tape now for question number four under personal research contributions.

Let's move on to question number four under personal research contributions. What published or unpublished manuscripts best represent your thinking about child development?

Now I'm only going to mention published papers, and I'll indicate where they are published because I think it would be unfair to the listener to mention manuscripts, to which they would not have ready access. I'm also going to apologize for the fact that the list I'm going to give is somewhat long, however, I hope the listener will indulge me a bit. The length of the list, in fact, reflects the fact that I have published, I don't know, something like a hundred and sixty scientific articles and twenty books, and it will only represent a small fraction of that work.

The first paper that I think is significant is a paper that Terry Allen and I published in Psychological Bulletin in 1971, which was the first review of the Piaget Learning Literature, and was widely cited at the time.

The next paper also appeared in Psychological Bulletin in 1973, it was the first paper discussing the relative merits of the two sides of a very hot methodological controversy at the time concerning the use of judgments versus explanations in the study of children's cognitive development.

The next paper of significance also appeared in 1973, in the Journal of Cognition, and it was a literature review that dealt with the question of whether the stage explanation of children's learning of Piaget's concrete operational concepts had, in fact, been supported by the data.

The next paper of significance also appeared in Psychological Bulletin in 1975, it was coauthored with Frank Hooper, and it was an exploration of some of the methodological ins and out's of studies of invariant sequences in children's cognitive development.

The next paper also appeared in Psychological Bulletin, it was a review, another review of a certain area of the Piaget learning literature, conservation concepts in this case, that specifically focused on a subset of studies that had reported confirmation of the stage hypothesis. The stage hypothesis of children's learning is a readiness hypothesis that says essentially that at the level of data, children can learn a concept to the extent that they already possess it. What I showed in that literature review was, in fact, the data that had been used to support this conclusion were faulty, and contained an important artifact, in fact, they were infected by reliability considerations.

The next paper of significance is the paper that appeared in the *Behavioral and Brain Sciences* in 1978. It was concerned with the stage hypothesis in cognitive development, and discussed the behavioral criteria and the validity of behavioral criteria, and the ambiguity of the behavioral criteria that were then being used as a basis for inferring that there were stages of cognitive development.

The next paper is one that appeared in *Psychological Review* in 1979. The significance of this paper is, it is the first one that I know of in which modern techniques of mathematical psychology, particularly mathematical modeling were used in cognitive development.

The next paper of significance appeared in 1981, it was also published in the journal, *Psychological Review*. This paper dealt with the development of probability judgment in children, but it was also another paper in the tradition of using modern mathematical modeling techniques in the study of cognitive development.

The next paper is one that I co-authored with Mark Howe and Alenda Roche in Psychological Bulletin in 1992. It was a major review of mathematical techniques for modeling memory development. At that time, although I had published some work on mathematical modeling techniques in the development of higher cognition, I had not published anything, nor had anyone else, on the use of mathematical modeling techniques in the study of memory development, so this review sort of was intended to launch that area.

The next paper of significance is one that I published on mental arithmetic in the Journal of Child Development in 1983. This is also a mathematical model's of development piece. It's significance is mainly that it showed how people could implement precise measurement of the concepts of working memory that were currently being used to explain the development of higher cognition, how they could break the components of that concept apart and measure age variability.

The next paper of significance is a 1984 paper on the development of transitive inference that I coauthored with Yohnus Kingma, that appeared in Developmental Review. The significance of this paper is it is the first one in which the memory independence effect in children's reasoning was reported. This was an effect that was ultimately to lead to a number of other research programs, and also to the development of fuzzy trace theory. The memory independence effect is essentially one in which age variability and children's ability to remember the key determinative background information on a reasoning problem does not affect the development of their ability to solve those problems.

The next paper of significance appeared in the Journal of Child Development in 1985, it was coauthored with Yohnus Kingma and Mark Howe. It dealt with the development of forgetting. The significance of this paper is it was the first one to show that about a decades worth of research, which had found that there was no age variability in rates of forgetting from long-term memory between early childhood and young adulthood was, in fact, wrong, and that that finding of age invariance had been a consequence of three artifacts in the designs of those experiments.

The next paper of significance appeared in the Journal of Cognitive Psychology, also in 1985, also coauthored with Yohnus Kingma. This was the second major paper in the memory independence sequence in cognitive development, whereas the 1984 Brainerd and Kingma paper had reported eight experiments dealing exclusively with memory independence and the development of children's transitive inference. This paper then generalized that finding to other major important reasoning paradigms of the day, specifically to class inclusion, to probability judgment and to conservation. And in each case showed statistical independence of age changes in memory for background facts on these reasoning problems and in the accuracy of reasoning. But also showed experimentally what are called "singled associations," that is to say experimental manipulations that dramatically improve children's memory for determinative background facts, having no affect at all on the accuracy of their reasoning. This was also the first paper in which we began to explore the fundamental hypothesis of fuzzy trace theory. That is to say that the representations of background information that are stored are of fundamentally different sorts, and only one of those sorts of representations, or one of those sorts of representations, primarily, namely just kinds of representations, stripped downed, pattern-like representations, are what are actually used by children in reasoning.

The next paper on the list is interesting for a number of reasons, but I will simply point out that from this point on every paper of significance that I shall mention is coauthored with Valerie Reyna. And this is the first paper of significance that I coauthored with her, although I had coauthored other papers with her in the past. It's a 1989 paper that appeared in the Journal of Experimental Psychology. It's concerned with output inference explanations of dual task deficits in memory development. The significance of this paper is that, I think as most people look back on what we now think of as the basic processes movement in cognitive development, this is one of the early papers in that that sort of kicked that movement off. The basic demonstration in this paper was that the dual task deficits, which were then being studied extensively in memory development, under the hypothesis that they were measures of executive processing, or some sort of higher cognitive ability, that the basic findings could be explained by the use of the concept of output interference, a very non-cognitive basic process kind of idea.

The next paper of significance, coauthored with Valerie Reyna, appeared in the Annals of Operations Research in 1990. This paper is an extensive review of research going back some twenty years, of the relation between children's memory for premises in deductive reasoning problems, particularly transitive inference problems, and the development of their ability to solve these problems.

The next paper of significance also appeared in 1990, also coauthored with Valerie Reyna, but also with Mark Howe and Juliana Kevershan, it appeared in The Journal of Psychological Science, this is the first journal to be published by the, then fledgling American Psychological Society. This paper's significance is that it is the first paper in which the cognitive triage effect was reported, both in adults and developmentally. The cognitive triage is one in which our common sense tells us that when we are asked to recall a bunch of items from our long-term memory, that the ones that have the greatest memory strength are bound to come out first. In fact, what happens is, we tend to begin by recalling some of the weakest information, then we switch to stronger information, and then we basically return to weak information again. That effect, which appears to be due to the interplay between semantic memory and output interference was first reported in the 1990 paper.

The next paper of significance appeared in 1990 as well. It was also coauthored with Valerie Reyna, and it appeared in the Journal of Developmental Review. This is the first formal paper stating fuzzy trace theory. Although the term fuzzy trace theory had been used six years before in a paper that Kingma and I had published in Developmental Review, this was the first systematic exposition of the theories assumption.

The next paper of significance is coauthored with Valerie Reyna, appeared in the Journal of Psychological Science in 1992. The significance of this paper is it reviewed and set forth in very general terms, for a general readership in psychology, the great pervasiveness of the memory interference in reasoning.

Next paper of significance appeared in a book, coauthored with Valerie Reyna. The paper was on fuzzy trace theory in the development of children's mathematical thinking. The name of the book was *Everyday Memory*, it was published by Logey and Davies. Basically this paper showed in some detail how memory processes and memory representations that are fusion in character, and therefore, might be reviewed as rather sloppy and imprecise by most people, nevertheless, can lead to very precise forms of reasoning of the sort we associate with mathematics.

The next paper of significance appeared in the *Journal of Psychological Review* in 1993, also coauthored with Valerie Reyna. This paper was the first to demonstrate that in addition to the memory independence effect in cognitive development, you sometimes got an interference effect as well, and this was predicted by the theory. And the memory interference effect is specifically cases in which the child's ability to remember background information that is absolutely determinative in solving a problem actually harms their ability to solve the problem.

Next paper of significance appeared in the journal, *Learning and Individual Differences* in 1995, also coauthored with Valerie Reyna. This is a synthesis or summing up of all of the work on fuzzy trace theory that had been done during the first half of the decade of the 1990's.

Next paper of significance also coauthored with Valerie Reyna and with Ryan Kanier, was published in the *Journal of Memory and Language* in 1995. This paper was the first to show that the two fundamental ways of representing information; children's two fundamental ways of representing the information that they encode can have opposite implications for how well they remember that information.

Next paper of significance, coauthored with Valerie Reyna and Ester Brands, also appeared in 1995. It appeared in the *Journal of Psychological Science*. This is a paper, the first paper applying fuzzy trace theory to a very important problem at the present time, namely the nature and the origins of children's false memories. Fuzzy trace theory has a particular analysis of children's false memories, namely that they arise from just memories and from misinterpretation of just memories. This leads to a very interesting prediction that was tested in this particular paper, namely that false memories, once they're stored, can, in fact, be more persistent across long-term retention intervals than true memories of the same information.

Next paper of significance, also coauthored with Valerie Reyna, appeared in *Developmental Psychology* in 1996. This paper is a further application of fuzzy trace theory to the area of children's false memories, and it shows that simple memory tests, tests that neither are designed, nor, in fact, mislead children or suggest anything to them, can in the long-term falsify their memories in very significant ways. So in other words, just tapping, or just diagnosing or just tempting to find out the contents of children's memories can alter them in falsifying ways.

The next paper of significance, the final developmental paper that I will mention, is one that is forthcoming. It will appear in the *Journal of Experimental Psychology* in this November, accompanied by commentaries from several senior investigators. In this paper we present the first formal mathematical modeling implications of fuzzy trace theories, assumptions about memory development and about the connection of those assumptions to children's false memories.

Now finally, this question, question number four also asks which of my contributions were the most wrong-headed?

I would say the most wrong-headed contributions were some ones that I made in the early 1970's in the area of developmental sequences in cognitive development, so called invariant sequences. I noticed at that time, and published data to the effect, that these sequences were highly variable and were different than the ones that people had been predicting, and that Piaget's theory had been predicting. However, at that time I published some proposals, which in retrospect proved to be far too optimistic. I had thought at the time that we might, in fact, find stable developmental sequences that were not subject to criticisms of diagnostic error if we took due account of measurement considerations. That did not prove to be the case, and there proved to be a fundamental undecidability in that area having to do with whether a sequence was, in fact, real, or was, in fact, due to false negative or false positive diagnostic error. And one reason for that is, that without fundamental measurement of our theoretical concepts, these ideas, having a concept, making a false negative error, making a false positive error, are really all the same thing and I failed to spot that.

The fifth question under personal research contributions has to do with my experience with research funding apparatus over the years and my participation in it?

Let me say a few things about that. First of all, since I happened to be fortunate enough to go through graduate school at a time when we were flush with funding for research in psychology, so by comparison with people who took their Ph.D.'s ten or fifteen years later, I consider myself to have been very generously treated as far as research funding is concerned. My participation in the research funding apparatus in the United States has not been extensive, primarily because I spent the first fifteen years of my career in Canada, where although the nature of the research supporting apparatus was really not as pure-driven as it is here in the United States, it was much more centralized. And this mechanism of in-house study sections and so on, in which investigators participated, it really did not exist in Canada. Basically senior investigators that were submitting, as I did, research proposals to the Natural Sciences and Engineering Research Council, would have them funded without any difficulty, perhaps not at the level they wish, but nevertheless they would be funded. So there was not a lot of participation or shaping of research funding policy in Canada. And so therefore, I have not had extensive experience in that area. Though as I said, the support that I have received, both in Canada and the United States, I think has been excellent.

The next question falls under the new heading of institutional contributions. It asks what institutions I've worked at, what dates and in what capacities?

My first post-PhD. appointment was as a research associate at Michigan State University in the fall of 1970. My next appointment was as an assistant professor at the University of Windsor, where I stayed for one year, leaving in 1971. From 1971 to 1973, I was an assistant professor at the University of Alberta, in Edmonton in Canada. From 1973 to 1976 I was associate and then full professor at the University of Alberta. From 1976 to 1983 I was a professor at the University of Western Ontario, in London, Ontario. During 1980 to 1981 I was on sabbatical leave, and during that time I was a visiting professor at the Institute of Child Development at the University of Minnesota. I then returned to the University of Alberta in 1983. And from 1983 to 1986, I was Henry Marshall Torrey, professor of social science. I was also the director of the Center for Research and Child Development in the University of Alberta during that period. This was a new research center, which I'd helped develop, along with Dr. Frederick Morrison, who is now at Loyola University in Chicago. From 1986, in the academic year 1986 to 1987, I was on leave as a visiting professor at Southern Methodist University. From 1987 to the present, I have been professor of Educational Psychology at the University of Arizona in Tucson. Since 1997 rather, I have also been serving as director of the University's division of Learning Technology and Assessment.

I'm going to skip question two because it does not apply to my career.

The next question asks me to describe my experience as a teacher of child development research, and/or as a trainer of research workers, and asks me what courses I have taught?

Well, first of all, my course teaching experiences have been rather extensive and varied. I'll comment upon why in a minute. At the undergraduate level I taught such courses as abnormal psychology, assessment and evaluation, cognitive development, educational psychology, exceptional children, infancy and early childhood, language development, psychology of adolescence, psychology of aging, statistics and measurement, theories of learning, and, of course, the usual introductory child course.

At the graduate level I've taught advanced child development, advanced statistics in research design, cognitive development, infancy and early childhood. I've taught a laboratory in child development research. I've taught learning disabilities and retardation. I've taught memory and language development. I've taught test measurements and measurement and assessment. I've taught theories of development. I've taught theories of learning too.

My experiences as a teacher have been so varied for a couple of reasons. First of all, my role in the different departments I have been in has been varied. I have not simply been a part of the developmental group, or the development psychologist who teaches the development stuff. I had been in some cases early in my career at the University of Windsor, and then at the University of Alberta, that was exactly the function I filled, and at those times I focused primarily on the traditional forms of developmental teaching.

In the early 1980's, however, when I became Henry Marshall Torrey, professor, I was expected to do somewhat broader teaching. Since that time, I have also done somewhat broader teacher since I have been at the University of Arizona, for a couple of reasons. First of all, my appointment has been in the College of Education, rather than in a Department of Psychology, which is emphasized courses that are more applied in nature.

The second nature reason for my rather varied teaching has to do with the role that I had traditionally played in most of the departments I had been in with respect to graduate students. More often than not, I have been in the position of being – providing, if you will, a service component to an applied child program of some sort. That is to say, directing graduate thesis and teaching graduate students who were in some program other than mainstream child development. Usually that has been at one of two sorts. In psychology departments, there's usually been a clinical child program. Since I have been in the College of Education at the University of Arizona, it has been a school psychology program. But in both of these cases the phenomenon has been basically the same, you have a faculty who are directing a licensed program of some sort that is oriented primarily toward training graduate students to achieve licensure and do full-time practice in a particular applied field, and that's what the faculty devote most of their time to. However, as part of that program, students need basic training in theoretical areas of psychology that are going to make contact with their work, and they need to do doctoral dissertations. So my teaching, and also my research direction at the graduate level, has been structured to fulfill that particular role, and naturally that means a lot of breath and a lot of variability in teaching.

Finally the same question asked me to comment on the tension between teaching and research in the field of child development?

I must say that I have really not experienced the tension that is sort of implied here. I have found in most of the programs that I have been in, that I have been strongly encouraged to do research as well as to teach. And I have also, although I thought in graduate school that there might be some dichotomy between these two, I found that they reinforce each other tremendously. I don't think that in a field as new as psychology is, and a field as new within psychology as child development is, that it is possible to be a good teacher unless one is also a good researcher, at least to the point of being up on the primary literature because things have changed so fast and fundamentally in terms of our knowledge of children since I have been in the field.

By the same token, I don't think it's good to be completely isolated as a researcher. This may not be true for other people, but as for myself, I find that my research problems are sharpened – certainly my ability to explain my own ideas, and therefore do better research is increased by having to explain these ideas to students in courses. So I would replace the word tension, I think, by interaction and mutual facilitation. Now that is not to say, of course, that there are not many, many places that load up child development people with far too much teaching so that they cannot do their research, so that that's largely precluded. And I don't just mean small colleges where people are expected to teach three, four, five courses, but I also mean in the child development area where we are more than say a physiological psychologist, or a social psychologist, expected to do a lot of service teaching; the traditional mid-week three hour introductory child psychology course for teachers and for expectant parents and so on. So, but I wouldn't say that's a tension. And certainly to have a tension, things must be operating on both sides, and I don't know of too many places, or certainly not a comparable number of places that simply relieve people of all teaching in the child development area and throw them forcible into research. So I don't think that that's exactly a tension, it's just a problem that we have to live with.

The fourth question under this heading asks about my experiences in so-called applied developmental research, and to comment on the role of putting theory into practice?

Although I have tended to do research throughout my career that I suppose is pretty basic, my experiences in applied child development research have been fairly extensive. I'll mention three in particular. Early in my career I focused on the development of mathematical and scientific reasoning, and I was very concerned to translate that work into new instructional initiatives in these areas, and I served on committees and attended conferences of the National Council of Teachers of Mathematics, American Educational Research Association, and so on. Contributed publications to educational journals and educational books, attempting to translate the major findings of basic developmental research on mathematical and scientific reasoning, into the likely ways in which it might be implemented in instruction.

Shortly thereafter, I became interested in preschool education, and in the late 1970's, early 1980's, was actively involved in what was then called the cognitive curriculum movement. At that time more and more people in the United States and Canada were sending their preschoolers, their toddlers to preschool, and there was very much concern with developing specific curricula for these preschools. And I played the roles in committees and workshops of helping translate basic research on cognitive development into appropriate kinds of experiences for preschoolers.

Then finally, in recent years, the past decade, I've been very much interested in the legal's sphere and in the child abuse sphere, and the application of developmental research, especially memory development research to this sphere. This interest has arisen because of some changes in the law that began to occur a little over a decade ago, where in various venues throughout the United States, the traditional laws governing admissibility of testimony from children, the kind of evidence that is required to convict someone of a crime like abuse, began to be liberalized in various ways. The accused traditional right to accuse the accuser was weakened in these cases. In some states, hearsay testimony was permitted where a child might make a disclosure of abuse and then be unwilling to repeat it. The adult who heard that disclosure would be allowed to testify, and so on. And after a number of years of people being prosecuted and imprisoned, some cases came to light, which appeared to involve very serious abuses of this situation; certainly the McMartin Preschool case in California would be example, or the Kelly Michaels' case in New Jersey would be another example; the Little Rascals case in North Carolina; the Country Walk case in Florida, and also the Snowden case in Florida would also be examples of this. So as someone who, at that time, had spent a few years studying children's memory development and having developed a theory, fuzzy trace theory, that has some things to say about how children's memory might become falsified. I became interested in providing whatever advice I could provide from the basic research literature to the courts and to attorneys, and I've subsequently become a member of – a Board Certified member of a forensic college, and I'm also Board Certified in Forensic medicine.

The next question asks when did I join SRCD?

This is under my experiences with SRCD that was in 1968; however, I did not have many early contacts with the Society. I did not attend a biannual meeting until 1975 in Denver, preferring to spend time working in the laboratory until I had established myself there.

The next question asks about my history of participation in scientific meetings and publications of the Society?

I've, as far as publications are concerned, I've published an SRCD monograph. I've published commentaries in SRCD monographs. I've published probably oh, fifteen or twenty articles in Child Development, and I think I participated in every biannual meeting since 1975. And I think I have contributed a paper or more to each of those biannual meetings. As far as my other contributions to the Society, I served a term as associate editor of Child Development in the late 1970's and early 1980's.

As far as question number three under SRCD experiences --?

I have not formally participated in SRCD governance, except at far removed, when I was an associate editor of Child Development, but, of course, the editor is the one that serves on council.

The fourth question asks, what are the most important changes that have occurred in SRCD in its activity?

Well, obviously the most important change is size. At the time I entered graduate school, one of my advisors used to say that SRCD was a small society that met around tables in places like Columbus, Ohio. Now it's huge. It is by far the largest conference that I attend. And certainly all aspects of the field of child development are now represented, whereas, at the time that I originally started attending SRCD, the representation of content of the field was much narrower.

The next topic is called the field, and the first question under that asks me to comment on the history of the field during the years that I participated in it?

I suppose what I would say there is the major change in the field has been it's growth, like the Society as a whole. And I think it's very, very positive. I think we should be happy about the field. We are of the, I think, excluding clinical psychology, we are probably the largest of the six or seven basic fields of psychological research, developmental is probably the largest of all of them, surely much larger than mainstream experimental psychology; social, physiological, quantitative and so on. And we now reflect -- really all aspects of psychological research are now reflected within SRCD, which is wonderful.

Certainly as far as my own views are concerned, I have not changed my views about the importance of child development research for psychology as a whole. I think they've strengthened if anything. When I came into the field, with exception of Piaget's theory, theoretical development was from mainstream psychology to developmental – we adopted theories from mainstream experimental psychology and tried to test them out. Developmentally now, since my time in the field, a very positive development has been that a number of theories have been developed within developmental that are unique to developmental, which have then been translated into adult work.

Then the last question is on personal notes, and it asks to tell us something about my personal interests, my family and especially ways in which they may have a bearing on your scientific interests and contributions?

Well, it's easy to say what the main thing there is, and that is my wife, Valerie Reyan. Valeria and I met in 1980 at Boston, and we were married in 1985. And as you can tell from her work and from mine, we have collaborated in almost everything since that time. She has deepened and enriched my scientific work enormously. There is no boundary anymore between my scientific work and my family life. She's challenged me and she has improved my work tremendously through her own ideas. And my work, certainly during the last ten or fifteen years would not be what it is without her guidance, without her collaboration and without her ideas.

Perhaps the most interesting or one very interesting aspect of her influence has been on my applied contributions. Valerie herself grew up in rather poor circumstances, in Miami Beach. She would not have had a college education, let alone a PhD if it had not been for the generous financial support that was provided to National Merit Scholars that enabled her to leave Miami, attend undergraduate studies at Clark University in Whorster, and she has always been very conscious of the need for good application of scientific work. And I would say, two emphases that she has are tremendously important: Number one that applied work needs to be scientific in character. A basic failure of application of psychological work to applied consideration, she believes, and I certainly agree with her, is that it is the applied work is typically the weakest science that we have. I would cite in this connection something I mentioned earlier, namely the changes in the laws that were made governing child abuse cases, which were made in the 1980's and result in a number of miscarriages of justice, simply because they were based on bad science, appropriate safe guards based on our scientific knowledge were not included. So one point that Valerie has is that applied work psychologists do should be based on the best science. But secondly, that the best scientists should also do applied work, that part of good scientific citizenship is taking a portion of one's time to contribute to important applied areas where one's science can make a difference. And certainly my own work in the forensics' sphere in the last ten years, the work that I have been doing recently in the learning technology area, are a result of the stimulation from Valerie on this.