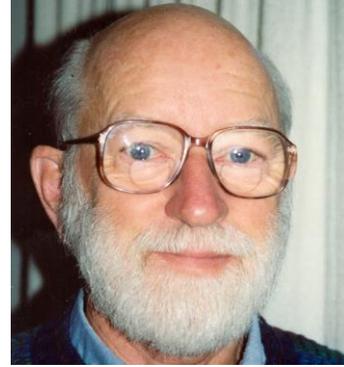


John H. Flavell

- Born 8/9/1928 in Rockland, Massachusetts
- A.B. (1951) Northeastern University, M.A. (1952) and Ph.D. (1955) both from Clark University

Major Employment:

- University of Minnesota, 1965 – 1976, Professor at the Institute of Child Development
- Stanford University, 1976 – Present, Professor in the Department of Psychology (Emeritus as of 1992)

**Major Areas of Work:**

- Theoretical and experimental work on cognitive growth in children
- The development of children's knowledge about the mind

SRCD Affiliation:

- Governing Council Member, 1975-1983
- President, 1979-1981

SRCD ORAL HISTORY PROJECT**John H. Flavell**

Interviewed by Tom Lyon

April 22, 1993

Lyon: This is an interview with Professor John H. Flavell for the history of SRCD project and my name is Tom Lyon. What I thought we'd do to start John, is I wanted to ask you some of the questions about your experiences with the society. First of all when did you first become involved in SRCD?

Flavell: Well I think it was 1963. I joined in about 1963.

Lyon: And who were the people that you came in contact with when you first became involved with the organization?

Flavell: I can't remember any specific people, I know Dorothy Eichorn I'm sure I met early on. I'm pretty sure that in that first SRCD which I think was in Berkeley actually. A small hotel, not a very big hotel in Berkeley. The organization was very small then. I can remember flying through the airport by helicopter. They had a helicopter service then, to Berkeley with Mary Ainsworth who I'd never met before. I didn't know anything about her and she didn't know very much about me either.

Lyon: So how many people, this would have been about '63? How many people would be at the conference?

Flavell: I would suppose there might have been, you know, like 600 or 500 or something. I really don't know, that would be a matter of fact, of course, they could look up. But it was much, much smaller then. May be one tenth of what it is now.

Lyon: And so '63 was this about the time that you had decided to think of yourself as a developmental psychologist?

Flavell: Yes it was actually, because I became progressively more enshrined as a developmental psychologist during my ten years at Rochester, from '55 to '65. So increasingly less a clinician and increasingly more identified,

self-identified, with developmental. So really I think probably joined division seven, of APA maybe even earlier than that. I don't remember exactly how I got on to SRCD.

Lyon: OK. Can you say something about how you became more and more involved in the activities of the society?

Flavell: Activities in the society. Actually I'm not an activist type, I'm not a political type, I'm not a good administrator, none of those things. So I never would have initiated anything. But in fact I was elected to the governing council of SRCD from '75 to '83. Then I was elected president from '79 to '81. And the way these things go was, you are president elect for 2 years, then you are president 2 years, then you are past president for 2 years. So you are involved in one way or another for a good 6 years and actually before that. So it was close to 10 years, I had something to do with the government.

Lyon: I see. Were there any major issues or controversies that you remember from that time?

Flavell: Yes I'm pretty sure I do. The main ones were how to increase minority membership, this was a perpetual issue, how to increase inter-disciplinary membership, that's to say, non-psychologist membership. People from sociology, anthropology, pediatrics, child psychiatry and so forth. So those were two of the big ones. And also the management of the society. Here is a society that's growing rapidly. The question is how should it manage itself should it keep doing it as a sort of Mom & Pop sort of operation. Dorothy Eichorn was really a genius at running this but she was sort of doing it all by herself and sort of on her own time and I can't remember all the details, but it finally had to become a professional organization with hired help. Also a convention manager, for example, as they currently have, rather than having it just sort of done by Dorothy Eichorn and sort of amateurs in a way. So, and organizing, taking care of the finances of the organization too. Making sure that we were financially stable and deciding about dues increases and other sorts of funds and how to invest the funds that we had. Those are the kinds of things that came up.

Lyon: All right, well regarding the minority participation and multi-disciplinary participation did you think that progress was made in those periods?

Flavell: Yes, I think it was. I can't remember anything specific about it. But the committees, both of those were represented by committees, and one thing that I think was, yes I'm quite sure was initiated during my term in this period from '75 to '83 or so, was a change, the way of electing presidents changed. In the past it was just, you know, anybody who floated to the top. Then somewhere in there they decided to make every other president be a non-psychologist. So that continues to this present day.

Lyon: Oh, that's interesting.

Flavell: So you have the likes nowadays of Bob Emde, for example the recent president a non- psychologist and there has been a series of others prior to that. That's been a good thing by the way, I think that will encourage people to, encourage people outside of psychology to join. And again, how much success they had, I would rather leave to the people who really know about that, the people who do know all about that, including Dorothy Eichorn who would know everything about that. John Hagen similarly who is now running the society, both of whom have been excellent by the way. It's a wonderful society to belong to, I mean, I think it's unbeatable.

Lyon: OK. All right, well maybe could move to a different area and talk a little bit about, I have your vita here, it mentions that you were, we could way back to when you were born in Rockland; Massachusetts in 1928. Can you think back that far and think about any experiences as a child or as an adolescent that you think might have sort of presaged your eventual interest in developmental psychology?

Flavell: I just about can't, to tell you the truth. I never heard of psychology as a child. Or even as a high school student or even until my sophomore year at college. I knew nothing whatever about psychology. My family was not a particularly intellectual family. There were not a lot of great books around. There were not a lot of cultural events

in the small town I grew up in. The high school itself wasn't particularly... fostered anything much. So I wouldn't call myself an intellectual by any means.

Lyon: So what was it as a sophomore in college that got you interested in psychology?

Flavell: Well I think two things. This may sound curious to say, but when you're in college you like to find out what you can do and also what you can't do well. I began by wanting to be a chemistry major because chemistry in high school seemed really very interesting. It turns out chemistry in high school was a lousy course. Nothing like real chemistry. So when I encountered real chemistry in college it just about killed me. I was just no good at it, you know, I didn't have any particular talent for hard sciences. And I had very little math background and very little aptitude probably, and a good deal of math anxiety. So that didn't fly. I then was seriously for awhile a kind of a pre-med major and so I took a lot of biology and some more chemistry as well, especially biology. I think around my sophomore year I took my first course in psych, just a little introductory psych and liked it a lot and also was good at it. So the combination of liking it and being pretty good at it and it just sort of seemed to suit me. So I focused on becoming eventually a clinical psychologist. .

Lyon: Was psychology at that time, or was it primarily clinical psychology?

Flavell: No. No it was quite a variety of things. A heavy, a heavy amount of learning, obviously, learning theory orientation was right at its acme then just about and for some time after that. Also psychoanalytic theory of course and clinical things, those are the two theories that I best remember as being the outstanding ones. But there was also stuff on sensation perception, knowledge about perception, things like that.

Lyon: So in choosing, I mean in deciding you that wanted to become a psychologist what was it that made you think clinical would be more interesting than say becoming a learning theorist?

Flavell: Right. I don't know it just sort of seemed more interesting to me. I can't remember exactly why. I went to Northeastern University and the psych major there, the teachers although very nice people, were not great teachers. One of them was quite good the rest of them were really pretty awful. In fact I took a course in child psych as an undergraduate and although I didn't dislike the subject matter I certainly thought the course was dreadful. It was really one of the worse courses you could imagine. It was a wonder I ever came back to the field. Clinical was sort of in its heyday and if you weren't interested particularly in the hard end of things, and I sort of wasn't probably within any area I would have been in. Clinical would be a natural, developmental would not have been a very salient option.

Lyon: Was it even really a clearly demarcated field; like could you have gone to a program in developmental psychology back then?

Flavell: Yes you could have, you could have gone, particularly you could have gone to places like University of Minnesota and studied child development, or the institute of child welfare at Iowa. I mean there were several places around the country that had, and Berkeley, had for a long time a tradition of doing research on children of one kind or another. Much of that didn't much interest me, much of it didn't, didn't then and doesn't now seem very interesting. The Dorothy McCarthy type stuff counting numbers of sentences kids have at different ages. Then a lot of replication of old Piaget's, some of that didn't seem terribly exciting then.

Lyon: How well known, well maybe we should talk about this a little later when we get into your work on Piaget. So you decided to go to grad school and I know you went to Clark. You were mentioning to me yesterday how you had decided between Clark and Harvard

Flavell: Right. At this point here I'm a senior at Northeastern University in Boston and I applied to about 4 or 5 schools, you know like most people do, the good ones and some fall back ones. And the best offers I got from my point of view were Harvard and Clark, except in those days it wasn't a sure thing that you were going to have any kind of financial support. Harvard offered some admittees with fellowship money of one kind or other and some without. In my case it was without that doesn't mean I wouldn't have got some, I mean, I might not have got some.

But I didn't know that and I didn't have much money anyway. If I remember, I think Clark offered me a VA traineeship, that's what it was, a veterans administration trainee ship in clinical. So right from my very first year I was making some money. And so that's what helped keep me going, my family didn't have any money.

Lyon: Was it a coincidence that both schools were near where you had grown up?

Flavell: Probably not a coincidence entirely. I think I applied to five schools or so all together. Well I think I also got into Illinois, but I didn't know much about Illinois. And in those days you never, never, I mean, you would never travel out to these places as people would do if they wanted to find out what they were like. And I knew Harvard was a good school and Clark I had heard good things about. So I decided to go there.

Lyon: So you had mentioned, do you remember speaking to anyone at Harvard, like who your advisor might have been if you had gone there?

Flavell: I'm trying to remember who was in clinical at that time at Harvard. And I haven't had time to look it up. So I'm not really quite sure who it would've been. Gardner Lindsey who is the recently retired director of the center of advanced study lives right here. Who is going with, probably not married to, but going with Lynn Carlsmith, the wife of a former staff teacher who died several years ago? But I mean I see them around all the time, and he was I think even at that time, he was probably a young faculty member in clinical at Harvard. I don't know who else was there, I just don't remember right now exactly. Maybe David McClelland would have been someone I would have been involved with. I think Gordon Alport was still alive then, but he was just working in personality and I probably wouldn't have taken classes from him.

Lyon: Right. So once you decided to go to Clark was it right at the beginning of the outset that you started to do things with Heinz Werner?

Flavell: I never actually did any work with Heinz Wemer at all. Contrary to what you might think.

Lyon: But he was your advisor right?

Flavell: No he wasn't actually. He wasn't actually my advisor. A woman named Thelma Alper was my advisor. She was a . . . I don't know whether she is even still alive now. I haven't heard from her in a very long time. She was in clinical I think. But I don't remember really having any kind of real advisor in a sense. I also, this must sound really weird, I didn't really have any mentor, I didn't have anybody that played the same role towards me that say I play with you, somebody who's working closely with their research. I don't know why, some people did, but I sort of didn't. I didn't work as a clinician I wasn't obliged to do a whole lot of research, as you would be here. I did do a master's project, which is that return of the repressed thing there. Then I kind of followed more or less by myself with a bit of help from Alper. She actually kind of served as an experimental stooge. I mean she actually served as a deception experiment of a certain sort. And she actually served as the professor who sort of came in and evaluated some of these responses as not being quite right, in order to cause repression. I don't mean that she provided no help at all and I'm not trying to be boastful but, in fact I went all the way through graduate school kind of really devising my own research. What research I did as a masters and a Ph.D.

Lyon: Was the program such that a lot of people who were going the program were really looking forward to being clinicians primarily and not so much researchers?

Flavell: Yes yes, and I also assumed I would probably be in clinical practice to a large degree, anyway. I don't remember whether I thought, well actually I think both would have been attractive. Both an academic clinical job and a clinical, clinical job. When I graduated there weren't any academic clinical jobs that paid anything at all. So I took a clinical, clinical job.

Lyon: That was with whom?

Flavell: That was in Fort Lyon, Colorado. That was my first job.

Lyon: Oh, OK. Right. So maybe we could talk a little about your dissertation. I know you did it on schizophrenic thought.

Flavell: By the way when I say that I didn't have a mentor, I mean didn't have somebody kind of directly supervising, and sort of giving me ideas about my thesis. I had some people on the committee. But an important figure and kind of looming over everything was Werner, at Clark, was Werner. So he was the one who first provided me with an image of something like developmental psychology. That was other than, you know, what they were doing the Iowa research welfare station. It was a view of developmental theory involving differentiation and higher kind of a conception of what it was for things to develop. And that was, all of us sort of soaked that all up as being part of our conception of things no matter what we did. Because his theory strangely, would apply to anything. I mean anything was development that changed basically. So it could be applied to clinical things. A number of people did do thesis's involving research responses and so forth. Looking at the developmental level of patients and people with different kind of diagnostic categories. That was really kind of a common kind of thing to do. And mine was a little bit like that. I can't remember the details very well, I didn't look it up, but it did have to do with evaluating their communicative abilities. And also their, I think something about I the meaning of certain words.

Lyon: How abstract their meanings were. Yeah I was looking at that. I suppose, I mean I looked at the paper you gave me that he wrote in 1957 on the nature of development. And so there seems to be a link between, schizophrenic thought was presumably immature thought. And so that does, in trying to figure out how it is that you were doing work with schizophrenics and at the same time you were becoming interested in developmental. It's an interesting question.

Flavell: Somebody else might have done it with depressives though. You know with anxiety patients and so on. It actually, it was more though than just a study. I got some kind of a cockamamie theory in there that occupied... the thesis actually was pretty long and a lot of it-- which I think actually ended up being this microgenetic stuff. It's kind of vague in my mind, without looking at it again. But it was a kind of a theory, I ended up with a kind of theory of schizophrenic thinking. Which in fact, I think actually got published, in the Canadian Journal of Psychology. That must about the third item there. Some observations on schizophrenic thinking. I was influenced by a guy named Arieti who had some notions about some types of schizophrenic thinking. Somehow all that got into the thesis. And the thesis was the in fact the second publication. The doctoral thesis is the second publication.

Lyon: The abstract thinking and social behavior? OK.

Flavell: That 's a short place that you can see it's three pages or something. It's a short account of whatever that was about.

Lyon: But you were mentioning, then you published a microgenetic approach to perception and thought. And that of course, has strong ties to... Werner talks a lot about micro-genesis.

Flavell: Absolutely. I never would have done any of those things probably, without Werner being there. So even though he didn't supervise the work, I would never have gotten a foot into anything like developmental theory or developmental psychology. Most likely, no matter where I go all the country, except there, that's my guess. I'm sure that this is an important, a really important part of my career that I happened to go that far with Werner's influence. Even though I didn't become, I didn't come out as it were as a developmental psychologist until later, this sort of laid the ground work for it, for sure. Even, actually from my first days at Rochester, all my first job I was teaching child psych. It seemed logical to everybody that I should teach it. They needed somebody to teach it. Because after all, rd had courses all at Clark University and with Werner and so forth and so on.

Lyon: So that was when you first taught developmental psychology?

Flavell: Yes, at the University of Rochester. I began, I got my degree all around let me see when was it exactly, effectively all '54. It was awarded all '55. So I was sort of all finished all December of '54. And then Ellie and I and

Beth drove out to Colorado to this God forsaken place in the east, if you've ever been to eastern Colorado, it's like western Kansas gone downhill. It's drier and more barren. Anyway that's where Fort Lyon was. I worked there for the rest of year for '54 and '55. And then got an academic job at the University of Rochester. Simply by the chairman there asking around, call the guy at Clark, and said do you know anybody? And he said, yes Flavell did pretty well in school and he said OK, so we'll call him up and hire him. I never interviewed for the job, and I got the job, just like I went to school sight unseen, I got the job sight unseen. And that was not uncommon in those days.

Lyon: So we're talking about first getting the job at the VA 's or at Rochester?

Flavell: No, I was at the VA, and here I am at the VA out in Colorado, and I got a letter from one of my teachers at Clark saying would you like a job at Rochester. Or else I got a, either that or it was a letter from the chair at Rochester, saying would you like a job. Since I was looking for... I began to realize I really wanted to do something academic, rather than being a clinician the rest of my life. Anyway that's how I got the job at Rochester.

Lyon: What was the, do you remember the thinking and when you started to decide that you wanted to be a researcher rather than a clinician?

Flavell: That wasn't all of a sudden. I think I was interested in doing research even while being a clinician. I thought I would be doing research. I kind of had a, not a flair, but certainly a bent towards doing that kind of thing. So I probably, even if I'd stayed in clinical work, I would probably have done some kind of research. Curiously once I got to Rochester, the work I thought of doing was almost never really clinical. That's why all these early publications have to do with cognition and some of it has to do with children, some of it doesn't.

Lyon: Was that, can you think, was there any point at which you had decided you were more interested in say, normal cognition rather than abnormal or?

Flavell: I'm trying to think, that's a good question. I don't really know. Some how or other I just sort of got interested in what passed for cognitive psychology in those days. And also to some extent in cognitive development. And teaching developmental psych all the time as well as teaching clinical courses at the same time. Actually during my whole 10 years at Rochester, I think that's true, the whole 10 years I was functioning as a clinical professor. Because even though none of my research was clinical in the least, everybody was quite happy accepting that, and the Piaget book was well along, in fact finished. Although I was still teaching diagnostic testing, supervising people in therapy and doing a little therapy, you know all those things are part of the clinical program at Rochester.

Lyon: Right. There are a couple of interesting things towards the end of the 50's. There is one thing, I see one of your papers was written with Ellie, your wife. When did you get married?

Flavell: Ah '54.

Lyon: So that was shortly before you got your degree. I met her in graduate school. Was she also a student then?

Flavell: No she wasn't. She was living in Boston. She had been tragically widowed at a very young age. Her husband was killed and she had a two year old child, now Beth is almost 42. We're celebrating our 40th anniversary next Sunday. I simply met her on a blind date from some people I knew, mutual friends and so on. This was in my next to last year.

Lyon: So you were able to convince her to drive out to Fort Lyon in 1955. That's pretty impressive.

Flavell: Well they did have highways then. And the car was fine and we just drove out there.

Lyon: So I know now she participates in all your research. What was it that led to her doing something with you in the 1959 paper?

Flavell: I think probably simply asked her to help out. She wasn't particularly interested in doing research. I mean even today, doing research is not anything like at the center of her self- system, or whatever you want to call it. And actually most of her research collaboration came about since I came to Stanford and the structural arrangement sort of made it more feasible to do it that way rather than hiring, having graduate students or RA's do it the way we usually did it prior to that.

Lyon: About when was it that you started work on the Piaget book that came out in 1963.

Flavell: That I think I can tell you, I think it was probably the summer or the late spring of '56. Really towards the end of I think my first year at Rochester. Very early.

Lyon: So what was it, I mean Piaget was.. .you know, you're often credited with basically, bringing Piaget to the United States a lot of people didn't know about it. How is it that you first learned about his work?

Flavell: Well, I was teaching I think four courses at Rochester, two of them were clinical courses, they were on the semester system. And one of them was an undergraduate child course the other was a graduate developmental course. And that one was mainly theories of development. So we had the Freudian theory, Erikson I think, some other people, Sullivan, a guy named Cameron who you've never heard of and --- certainly taught quite a lot of him. But then there was this guy Piaget, which of course I knew about him I knew about him in graduate school, everybody knew about him in some sense. But I didn't know all he'd written because most of it stayed in French at that time. His first five books were translated early on in the 30's. Even late 20's and they were well known. People tried to replicate them during the 30's even. But the rest of his work done in the late 30's, 40's and 50's, there is a ton of it and none of it was out. So anyway I was going to write, my plan was to write a theories of development book. Like the kind Pat Miller now has. It's exactly what my plan was. And each chapter would have a theorist basically, maybe a couple chapters per theorist. So I started preparing for that looking through and surveying the territory and suddenly in digging a little deeper and going to the library and checking these up. I found out that, here's this guy named Piaget who's written as much as all the rest of them put together or more. Unlike Werner he doesn't have any book about him. He hadn't at that time written a book about himself, about his work. So here's Werner, for example who has less work and has his own summary book and also for other people. Harry Sullivan and some other people. Here's this guy Piaget who's always done lots and lots of stuff and I didn't know much about him, I read summaries here and there. There was no book summarizing his work. So that seemed to me a much more important project to do. So I put aside the whole plan of doing the theories, thinking that I might go back to that some other time. And just simply decided, I'll write a book about Piaget, and that's how it began.

Lyon: So how much of your time, I mean that was in 1956, how much of your time were you able to spend putting that together?

Flavell: Well it took me about seven years to write the book all together. Of course I was doing other things, other stuff you see, but just trivial things, and you know, teaching and all that stuff. But I sort of did it a piece at a time. I would take a book, first I had to teach myself French.

Lyon: Yeah, I wanted to ask you about that.

Flavell: I had a couple years in high school. And I passed a very easy language exam for the Ph.D. That you had to pass, two languages. But my French was pretty bad, so I basically with a dictionary and gradually I got so I could read faster and faster.

Lyon: Weren't there words though like that weren't in the dictionary?

Flavell: Oh yes, I mean, that's the hard part. It turned out there was a big surprise to discover that the Piaget was just as hard in English. If it's unclear, it's unclear there's no way you can make it clear. And so the translations were, I mean I used to pity the poor translators, once I realized what the original was like. I began to have some empathy for the translators.

Lyon: Well wasn't that, I mean I would guess that the reason people hadn't written about him before was the combination of him being very difficult and being in French? I mean what was it that kept you from saying, "Oh well, you know, I'll just write the much easier review of the people that are easy and more accessible"?

Flavell: I don't know. First of all I had no time table. I didn't have any particular time that it had to be done. And I didn't set myself and deadlines. I just sort of chipped away at it. It took a long time, but I also had a lot of it. It was sort of the sense that you had then, and even today that somehow or other, part of what it means to be a professor is to eventually write a book of some kind. Not just articles, but a book, do a bigger project. A bigger project in those days, you know, for most of us meant having a mammoth grant and running ten longitudinal studies over 6 counties, but rather something like that. And also I kind of liked library work. I sort of liked going to the library and taking these things out. And I could take them off on vacations or take them out for the summer. We used to spend a number of summers up in Vermont in this little place, some relatives had a place up there. I would just take along this stuff and simply go through book by book. And it took these minute notes, which I've got here to show what they look like. They were just 5x8 cards. I didn't type and I still don't. So I just simply took lots and lots of notes. And finally when I read enough, not all of it by any means, but enough to try to figure out how to put it together to make a book. And that was the hard part. The rest of it was sort of just like be a monk, copying the letter M for the day in your manuscript. Just sort of, what does he say in this chapter, what did he say in that chapter. I can remember for example one frustration, this may take longer than we want to spend, one frustration was reading all of his articles about perception. He wrote a lot of stuff about perception, many people don't know much about. And then after that was all done out comes his book, summary book on the same stuff I had read all the original articles which were long and turgid and difficult and technical. Anyway so that was Piaget. So I didn't really have any sense that I was doing something, you know ---.

Lyon: Well I know in the book itself he writes a very nice introduction talking about... well it's interesting to ask you about that. When was it that you first actually contacted him and told him, "I'm writing this book about you."

Flavell: Oh, I've got the correspondence someplace, but it probably was in the neighborhood of 1959 or '60 somewhere in there.

Lyon: Did he seem very friendly to the idea at first?

Flavell: He didn't really say, he wasn't a big correspondent. He wasn't sort of either friendly or unfriendly. I corresponded a lot with Inhelder. She was more helpful and she supplied me I think, with some bibliography which I needed and a few other things. I also met him at Brandeis University in Boston, about 1961 or '62. He was getting some kind of degree there or giving a talk or something. So I got a chance to meet with him. Of course my French, my ability to speak in French was really pretty lousy and his ability to speak English wasn't terrific either. So it really wasn't an easy communication.

Lyon: He must have had your manuscript and he must have read it before he wrote the introduction.

Flavell: Well he certainly had it that's for sure. Because it was the publisher's idea to see if they could get him to write a foreword to it or something like that. So I don't know the exact sequence of things, but at some point we sent him the manuscript. And my guess is that he did read it, or at least he skimmed through it. Although I think the foreword gives you the impression of being pretty positive, my reading of it was kind of ambivalent. I mean it is ambivalent. He hated to be criticized and he also didn't like to be Americanized either. And he felt and many other Piaget scholars feel that, although they don't think my book is a bad book, they think it doesn't present quite the real Piaget. And they're probably right. In a sense it was a Piaget of a sort of reconstructed form, for people like us.

Lyon: Right. Well he does say, I know he says something about how this is for psychology and I'm not really a psychologist. It's an interesting statement about how I seem to be more understood from without than from within. So there definitely is this feeling that you're this person who's not quite into the way he thinks.

Flavell: Oh absolutely. That's right.

Lyon: So at the time you wrote it, I mean, you have a chapter where you criticize, or have your own feelings about the validity his work. How did you feel at the time? Did you feel as if even though in taking all this time to work out his ideas, that you were sort of finding your own space, your own beliefs about things. Did you see yourself as a Piagetian at that time?

Flavell: Well honestly, no I didn't, and I never did. My cognitive style is sort of not to be a true believer. I'm not a skeptic or a cynic or anything like that. I'm not all that much drawn to theory and certainly I'm very much not drawn to being a theoretical disciple. That just isn't me at all, and never was. It's just kind of a personality thing more than anything else. So I certainly appreciated Piaget's stuff a lot and also I appreciated it even more once, it's almost like the Asch effect, once everybody else started appreciating it too, once the book came out. And also Hunt's book which came out two years earlier was important, too. The two of them probably helped to make him better known. Also the French people used to read my book because they couldn't understand Piaget. They told me that in Geneva one time.

Lyon: Like the Tropic of Cancer or something.

Flavell: So I didn't think of myself as a Piaget disciple, my own research wasn't, by no means, replicating Piaget. I never did any of that science stuff. So I felt I had, I wasn't making any big deal of it, I sort of had my own schtick and then I had this book which was sort of my side project. So I didn't feel like I was a devoted Piagetian. I would never for example join the Piaget Society. Which I always thought was a ridiculous idea, in fact it's really a good society. What they do is fine. But I never could imagine belonging to it.

Lyon: So if someone asked you, what influence did Piaget's ideas have on your own work, what would you say?

Flavell: Oh, I think a lot, in the sense that, for example, you know much of my life has been dealing with kids developing understanding about psychological things and physical things. But certainly Piaget in all kinds of ways, must have influenced that concept of perspective taking, egocentricism and so on. All of that really came from Piaget as far as I was concerned, that was where I got most of that sort of thing. And by reading, for example, the first line of work I ever really did was on communication and role taking. And that you see, Piaget's first book was a book on language and it ... child. And that had communication experiments and so some of the ideas I got, that I worked on, came directly from his early work. He talked about the idea that children don't take the role of the listener when they communicate. Obviously my work was a follow-up to that. And similarly his ideas about spatial perspective taking very much influenced the level one, level two theory. The memory research wasn't at all influenced by Piaget, so there's no connection there. But some of these other things certainly they were. So I feel I owe more to Piaget than any other single, more than to Werner. Werner kind of got me going in the area, so I guess maybe that's the biggest debt of all in a certain sense. But subsequently I think Piaget, although as I say I wasn't a Piagetian, I didn't exactly do Piagetian work, I did Piaget inspired work I guess you could say.

Lyon: So could you say a little bit, because I'm not as familiar with, and I'm not sure the people are, with the original work that you did Like the first kinds of tasks or things that you were looking at when you did work on role taking and communication in the 60's?

Flavell: I'm really pretty sure I can remember this really clearly. You can clock certain sort of insights that you had, that are important. And I can remember a few of those. One of them was this idea, Piaget said children don't take the perspective of their listener when they communicate. So I got the idea, and I guess I was the first one to think of this, I think anyway. To find out whether they can or whether they can't or to see if that changes developmentally. What you can do is to vary the listener, you can create change in the listener's communicative needs, by blindfolding him,

for example, the first study we did. One of the first ones was simply to... the child learns how to play a game, a simple board game involving men that goes up and down, something like Monopoly, but And once the child learns the game, these are middle childhood, adolescent kids, their job is to teach it to an experimental confederate. This Charles Fry, one of the authors of this book, came out in '68. And we varied both within subjects and between subjects what his situation was. And the situation was either he was sighted looking at the game materials or else he was blindfolded and couldn't see them. Now trying to communicate a game board to somebody who can't see the materials is a vastly more difficult communicative task than to somebody who can. So when a person can see you can point, you pick up this and put it there, and so forth and so on. So what we could do then is to, since the child had the same task with these two listeners, was to simply compare their communications to these two people. And what we found was that early on, and I think in this case they were second graders, there's no difference. And in eighth grade they communicated differently to the two listeners. So you've got this nice neat scientific looking function. So that was a kind of an idea I had that proved to important in studying... this is the same kind of were studying with some of their referential communication stuff Where children were given a task to communicate which one it is on what things, the same idea.

Lyon: Now at that time what was your take on... I mean I know it wasn't until much later that you started talking about the level one, level two distinction. We'll get to that, but at that time did you have something akin to that? Some kind of distinction or some sort of structure that you put children's development into?

Flavell: I didn't have that distinction. That one came in, I think the early 70's. Let me think a minute. What I did do is to try to organize the results, was try to try to come up with a simple little model involving things the child had to know in order to communicate properly and to perspective take properly. And some of that was written in that book. Also need and --- and that sort of stuff

Lyon: So was the idea, I mean I hadn't seen that before when I was looking at some of that, is the idea that children, the earlier notion is that at some point they develop this understanding that they need to take perspective. But then there's this maybe more computational ability that comes later?

Flavell: Right. Right. Something like that. I don't think that's quite right but it had the ingredients in there. You know, whether you need to know or whether you have to know that other perspectives exist. That would now be translated as understanding the representational mind, I suppose. But people could have different beliefs about the same situation. In those days where I thought of it then, the thing was, do they understand that the other person's perspective, as a fact of life, might differ from your own. If they don't have that they're nowhere. So that's existence. Then need was, well was there any need in this situation to figure out what the other guy's perspective is? Is it useful? Then the theory is t well once you know they exist and that you need to find it out, can you find it out? Which I guess I'd call inference or something. Then it was something like the idea of maintenance, can you maintain it in the mind, in the face of the press of your own egocentric perspective? It's difficult, right on line, to keep the image of what the other person needs, because it conflicts with what you do. This is an idea that comes right out of the book on egocentrism by Piaget. So, I mean, that kind of stuff. Then finally application, I mean once you've figured out the person's perspective then you know how to translate that knowledge into an effective communicative message. I mean, you might be good at perspective taking but you don't know what it's implications are. What you should say to the person. So that was the idea anyway. That's in the book in a few other places.

Lyon: That's interesting. Now you mentioned that the meta-memory work did not come out of Piaget so much.

Flavell: No it didn't.

Lyon: One thing I noticed that in your paper on Heinz Werner which came out in 1966, as part of a series of papers in memorium of his death in 1964, you mentioned that one of the interesting notions he had was that change is qualitative, rather than merely quantitative. And you talk about mediational deficiency hypothesis. Now as I understand it, your first meta-memory task, I have of course a special interest in this, but your first meta-memory task, was a test of this mediational deficiency hypothesis. Could you say a little bit about that, because it's sort of neat the way that was one thing that led you into meta-memory, in sort of an indirect way.

Flavell: I think I can give you, by the way some of this is, if anybody ever wanted to read about it, some of this is in that 1992 thing, a chapter in a book edited by Harry Beilin. This one for Beilin. This was, what this is a chapter which sort of gives my own intellectual histories in these areas, in a way.

Lyon: Let's say perspectives on perspective taking. OK great.

Flavell: It just came out last year. They asked me to sort of trace my connection....

Lyon: Oh I wish I'd reviewed that.

Flavell: Well the gist of it was, just quickly on that, earliest role taking and communication. And then later on it becomes meta-memory then meta-cognition which is clearly also, about the mind. Then that's transmuted into the theory of mind and we've got the short history right there. But you see they're all about in some sense the same sort of thing. Like figuring out what's going on in someone else's mind and the interviewer's mind. But the meta-memory stuff I think has a pretty clear beginning. I was interested in private speech. Some of Vygotsky's ideas about private speech. And I wondered if there were some way we could test private speech in children and how could we get at it. So what we did basically was to, we thought we could get at private speech by giving children a memory task that involved rehearsal. That's like a simple study-able type of private speech. And then we would even have words that they had to learn that involved big lip movements, when you pronounced them. So what we did, we did this first study with Beack and Chinsky on memory. So it looks like the beginning of memory strategies and it is. But it wasn't intended to be originally, memory was a convenient way to study private speech. But once we found out what the kids did, they' show this production deficiency, which is that they failed to rehearse very much. Even though they could rehearse if we asked them to. If you asked them to they would rehearse.

Lyon: Right, so what was it? So the fact that it was, I mean this production deficiency thing is interesting because of the fact that if they produced it, then it improved their memory. It meant that when you started thinking about production deficiency, did that lead you in some way to think about memory strategies or self-conscience realization of the need?

Flavell: That's exactly right. So think of it this way, it began with an interest in private speech for which the incidental task, the task that we use, and incidentally is a good vehicle for studying it was memory, and then when studying what they did it's clear that little kids, even though they could do it, weren't using rehearsal as a strategy and older kids were. So that was sort of the first study, at least the first study I did, involving children's memory structure. So that became interesting in itself, so then we studied it further. Then a little bit later, we got the idea somewhere around 1968 or so or '69, somewhere in there. The idea, well gee, maybe this isn't just that they use strategies, maybe they use strategies in part because they know something about how memory works, so the strategies of meta-memory. So the idea, it was sort of based upon an observation by Madigan, and who was the really really famous Finnish born, Latvian born, memory researcher? The most famous guy in the area of memory there is besides Gordon Bower...I can never think of his name...he is a Toronto....

Lyon: Tulving? Oh OK.

Flavell: Tulving and Madigan have an Annual Review article, something about, you know, if you want to find out a person's mind why not ask them, or something like that. That is sticking in my mind.

Lyon: Oh, I've seen that quote.

Flavell: Anyway the gist of it was, let's study what they know, what do they know about their own memory abilities. Etc. etc. etc. It became a mix of strategy research and meta-memory. In fact knowledge about strategies was considered to be part of meta-memories. So you can see how it went from private speech to memory study, memory strategies of different kinds, a proliferation of them Then kind of meta-memory on top of that. Then finally from there the meta-cognition more generally and comprehension monitoring, communication monitoring as well as, memory monitoring. So it sort of expanded in that sense.

Lyon: It gets harder and harder to talk about it. It's not just a simple line anymore. What's it like when you were there anything special... did you start to feel like you'd exhausted meta-memory or was there something that led you into the broader issue of meta- cognition that you can recall?

Flavell: I don't remember exactly how it came about. It just seemed that it could be generalized. Some of the same considerations, I think maybe I was invited to do a chapter somewhere and I sort of took the meta-memory thing up one more step and made it cognitive strategies in general.

Lyon: Were there students who, and you don't need to stick to this early 70's point, but since then as well, were there students who played a part in either broadening or changing the direction of your research? Or even just students that you've felt have since that time of your influence have gone off in interesting directions?

Flavell: Oh, yeah, certainly. I've been really lucky in the students I've had. I've had a number of them over the years, Not so many in Rochester, because Rochester was a clinical program.. Actually I did have one student who has become a full professor at the University of Maryland, namely Ellen Klofsky. She was my first student.

Lyon: Oh sure, really. I know some of her work.

Flavell: She just got an APA prize and all that stuff. She was my first developmental student And then other people since, especially Henry Wellman, would be clearly my most distinguished former student. He worked with me on his dissertation. You can look at some of these joint publications. Some of these people have gone on to become something in the field, others have not. Peterson and are both professors at different places. Sims-Knight is too, Steve Yussen is a Dean, Bob Cooper is down here at San Jose State. These are looking at these names. Elise Masur is at Northern Illinois University I think. Brian Vaughn is well known in attachment research.

Lyon: Oh where's Vaughn, I didn't know he was one of your students?

Flavell: Right here on page five. Well he wasn't very much but he was in one study.

Lyon: Oh that's interesting.

Flavell: He was one of the only

Lyon: Is Ritter still in...

Flavell: Ritter died, he had a bad disease.

Lyon: Oh, I'm sorry to hear that.

Flavell: Gordon did not stay in the field to the best of my knowledge, nor did Drozdal. Lempers is a professor at University of Iowa, Iowa State rather I think.

Lyon: And I know Spear is in Texas now.

Flavell: Wilcox is at the University of Missouri now I think and Martha Everett is in Washington.

Lyon: That's quite a list. Now with Henry Wellman, he would have finished around 1975, right.

Flavell: Yes I think it was '75 right.

Lyon: And he of course was heavily involved in the meta-memory research. Did you, now when, it seemed like both you and him were moving into the broader theory of line questions at about the same time. So did you, was there a lot of interaction between you after he left? Or was he doing things, and you just coincidentally were very similar work.

Flavell: Let me think. He really got into it before I did. The present field theory of mind. He wrote a paper that was published about 1985 that was done about four years earlier than that, much to his chagrin. So he really early on, had talked about theory of mind development. He was one of the real pioneers, I would say, more than I am. And we were kind of pursuing separate tracks. We would meet each other at conventions. He did lots of other work too that were partly, you know, I suppose part of the work rd done and partly not. So he kind of really found his own voice in this theory of mind stuff. He also did a lot of meta-memory stuff, as you know, in his early career.

Lyon: OK. Let me make sure that I'm asking. There are some things that I want to, some gaps I want to fill. When you moved to maybe you could tell me a little bit about the institutional affiliations. So you moved to say to Minnesota, how that came about. And then to Stanford.

Flavell: I came to, as I said, to Rochester in 1955 and spent exactly ten years there. And in 1964 I suppose it was, I was interviewed for several jobs; one at Yale, one at Duke and one at Minnesota. Minnesota and I liked each other a lot. And the idea, since I was so clearly in developmental by that time, it made a lot of sense to move out of the clinical situation. And Minnesota seemed like heaven in a way because it's this big institute of all developmental stuff. Harold Stevenson was the director at that time, and he's a good friend and I still see him. I'm going to Russia with him in fact. So that was how I happened to begin there in the fall of '65.

Lyon: I know there were a couple of years that you spent at the center, here at Stanford?

Flavell: One year, one year, '69 and '70.

Lyon: What was it that led to your coming to Stanford?

Flavell: Oh well, Eleanor Maccoby and I had known each other for a long time and she was sort of eager to see if she could get me to come here. She was a chairperson at that time. And so she, I think when Bob Sears retired, I think she somehow got his billet, his slot. Or something like that, I mean, around that time. Because I think Dan Osherson who you may of heard of, Osherson was my predecessor here, and then he left. And his leaving, probably his leaving vacated a spot. Then in '73 or so, early '74, she invited me to come out here, I think, to interview, '74 probably, I'm sorry I don't mean '74, do 11 Yes I do. I came in '76, anyway there were a couple years of fooling around with this. She thought maybe there would be a position but there wasn't one yet. Then there became one and I came out and interviewed and all that stuff I spent about three days here and loved it and decided to come. So I came in the fall of '76.

Lyon: OK. Now this was about the time that...

Flavell: And stayed here for the rest of time.

Lyon: Right. You're still here. I know this about the time, mid 70's, was when a lot of your visual perspective taking work had started. When was it that you first, was that inspired in large part by the mountains test that Piaget did in the 50's?

Flavell: It was, yeah, I think in a way, that whole book is about a whole bunch of things. And he had different you see role taking communication was way back there in the late 50's early 60's. Some of the work we did was involved with perspective taking. Some of the tasks were perspective taking ones. And in that work we also used some very simple ones for very young children. Some of...in one chapter it's got kids age 3 - 5 doing perspective taking tests. Including things like, do you see things on the other side of this card? But anyway, the long and short of it was, is thinking about that research and also looking -- Piagets and what he'd found and the ages at what kids could pass

different tasks. It dawned on me one day, one of those kind of semi-high experiences I think, in Hawaii, I was teaching in Hawaii that summer. And the idea that well maybe there are sort of like two levels here, one is do you see it at all, does the other person see it at all, the other is does he see it as being this way or that way, or looking this way or that way. And that's where the distinction came from. In retrospect it sort of seems more straight forward then it did at the time. Like everything else it sort of feels like a discovery when you really finally get to it. You sort of cast around, and try to make sense out of these different findings. So that seemed like a way of making sense out of the fact that young kids can pass some of these spatial perspective taking tasks and not others. Why? And the answer to that led to some work with Masankgay and so on.

Lyon: Do you think, I mean I know that people, when they talk about theory mind research they say that, they talk about visual perceptive taking as one of the origins. You are very modest, so maybe it's hard to ask you this question, but do you see that as a, do you feel that a lot of your visual perspective taking work was the inspiration or....

Flavell: I don't really know. This isn't modesty, I hadn't really thought of it that way so much. I couldn't think of... you know if it's a gradual transition I suppose. Certainly I was studying what we would now call theory of mind during all that stuff. But we didn't quite think of it in that way. I think that probably the origin really would be the false belief task. And maybe in a sense recruited quickly into that about the same time we published our first appearance reality stuff I think to give credit where it's most due I think probably Pemer and Wimmer really owned most of the credit because I think that seemed to clear tracks. You see their work came out of this idea of Premack and something about the notion of the theory of mind and so forth and so on. So I think that probably their work was more like the true origin, although certainly my stuff was earlier and to some extent and also funneled into it

Lyon: Right. Well what was it that led you to the transition from the visual perspective taking work to appearance reality research.

Flavell: I think what it was it that we were interested in learning more about their understanding of perception. So that's where it really did come out of the perception thing. And we wondered about, I mean when you think about level two, level two is the idea of something seeming or appearing to be a certain way. As opposed to just seeing it. And so as I remember we got an NSF grant in this area. And anyway we had, I don't remember exactly what the tasks were, Francie Green could tell you better than I could because she was involved in this. But anyway somehow or other we started studying their understanding of whether things appeared to be a certain way versus really were. And that's where it began.

Lyon: So there must of have been, I mean the false belief research was coming out and you were doing your appearance reality research at some point you must have started to see, hey there's a connection between these two areas.

Flavell: Yeah, I'm trying to remember when that was. It might have been, seeing that connection might have been as even as late as the two conferences in '86 that really kind of got the thing launched. One was at Oxford, where I was at the time.

Lyon: Oh I didn't know that.

Flavell: The spring quarter of '86 Paul Harris and I were involved in getting that conference set up. And then David Olsen at Toronto had one at the same time so we all flew over to Toronto for that conference both were in '86. That was sort of a formal launching I suppose of the whole thing.

Lyon: That led to the Astington, Harris, and Olson book, probably the first book devoted to theory of mind development?

Flavell: Yes exactly, that's why that book has got joint authors from Toronto and Oxford. So it's really a combination of those two conferences.

Lyon: So once these things came together for you did that influence the work that you started to do after 1986?

Flavell: Yeah, it did. For example we started to do, as you can see, some stuff on fact beliefs versus value beliefs which is right in the false belief tradition. A couple of things on that. Let's see what else.

Lyon: Maybe you could say a word about your current work and maybe also to what extent you think that the, well I'll ask you a follow-up on that. But could you tell me a little bit about your current work?

Flavell: Yeah, what we're doing now is I see as continuing in the theory of mind tradition, but trying to look more at thinking and consciousness and mental attention. Kind of more dynamic active aspects of the mind rather than kind of states like belief, desire. So what do they know about how the mind actually operates in real time. For example do they know that people are having mental activity more or less all the time. Even when they're not engaged in any particular task. You know, a person sitting opposite to you in a subway who's not reading they're just sort of sitting there and there's something going on his is head. It may not be worth much but it's something. So that's the stream of consciousness idea and that's been core of what we've been doing so far for the last like two years.

Lyon: Now do you, that seems like, that's sort of break from the way a lot of people are going. And it almost seem that are you consciously thinking we need to take a different approach to theory of mind research and think about processes rather than states. How does your thinking work when you sit down to decide, what is my next research agenda going to be?

Flavell: Boy it's hard to remember exactly how these things began. It certainly is always a reciprocal mediation process in which each new thing you do then you kind of, it's almost a Piaget, you reflect on it and then you make it kind of coherent with something else. But sometimes these things don't, it seems to me, begin in a very rational, coherent, logical, sequential way at all. I wish rd thought about this before the meeting, I would have tried to remember how did we ----. Right now I can't remember offhand, even though it's much more recent than some of these other things. I guess that's decay of recent memory from aging. I know I began by making a list of the thing adults probably believe to be true about thinking and consciousness, and the stream of consciousness research was generated from that list. Anyway it's clear that my feeling is, regardless of how it got started, that this feels to me like a more important contribution than say, playing around with fact versus value beliefs, stuff like that. More like what the crowd has been doing, more like, you know it may be some contribution, it didn't feel like me. It didn't feel like it was something that's my own. I have a, I suppose most people have a pretty strong feeling about their own stuff. You want it to be different from other peoples work.

Lyon: And that's been something that's been true of you? I mean you mentioned you weren't a Piagetian, you weren't replicating Piaget's studies. So that's been true pretty much throughout.

Flavell: Yeah, that's true. And I think that's probably true of most people, as we all know, you don't get to be well known by just doing one more...if that's what you like to do, if you kind of like to twist parameters on things, well you do it, you're not likely to end up so it isn't so much....

Lyon: One of the questions is, you commented on the field in general, like I know at the last SRCD conference Alison Gopnik mentioned that she thought there was false belief task fixation. And that one of the problems with theory of mind research is that there's this obsession tweaking the false belief God's. And you said informally that you think this may have happened to some of the conservation tasks in the 70's. Can you say a little more about that, do you think the field in general has a problem with this or what are ways to avoid that kind of....

Flavell: Well all fields have this, it's just the way science progresses. Somebody comes up with something new and exciting and then people, some of them very good people some of them not so good people, will jump on it. And quite sensibly try to explain it, work it this way, work it that way. Then of course students down below who haven't got a chance to develop, you know, any sort of deep ideas of their own, will then pick up on that and you've got student projects and you've got SRCD and so on. Which is a perfectly natural and a perfectly OK way to go. This is

the way I think you kind of get a sense of what's really going on and then eventually you may decide that's kind of a dead end, we've learned all that we can from that, without gaining anymore. Then we go onto something else. You know, I think in the case of a lot of Piagetian phenomena, in fact most Piagetian phenomena had that kind of thing. You know, conservation, class inclusion, all the things you could think of as a Piagetian. Object permanence.

Lyon: But do you feel like, can we look back on it then, is it just confusion. Or can we look back and say well we can really make sense of the just millions of studies.

Flavell: Well you can't make sense of all of them, and some things don't ever really lead to a wholly clear conclusion. Although sometimes I suppose what happens, again I don't consider my insights on this to be worth very much, I kind of never really thought about it a lot. Historians in science could tell you a lot about this kind of thing, there's a lot of things that happen of this kind that really happen in all sorts of fields. And you know I think sometimes what happens is that people go back to old phenomena and reconceptualize them. That maybe constitutes an advance of some kind. Other times people just kind of give them up. I mean conservations, I think, what did we learn from all that? Probably its that young children don't have a very quantitative measurement type orientation. And also that they are very subject to salient facts and so forth and so on. And they have a very shaky understanding about what quantity means. But there have been millions of studies done on this. We haven't really necessarily led, the training have not particularly led to anything enormously important. Even though there are hundreds of them. And that surely will happen in this area too. I mean I don't pretend to have deep insights into how science really works, but I know a lot of it turns out to have been kind of a, in a sense at least superficially, a waste of time. I mean superficially it didn't end up being part of the permanent anything. Now maybe non-superficially it may have, I mean it's hard to say that things are not having some cause or effect simply because they don't stay in the literature. I rather think they do have cause and effect very often.

Lyon: You mean that ideas have some influence despite the fact....

Flavell: Yeah that you can never really trace back to them I mean, suppose that for an example, that as a result of our doing false belief studies rather than something else that causes the field to shift in some particular way. Who is to say that those extra ten studies didn't have a contribution? But you could never identify enmasse, I mean collectively they do. And if they hadn't been done, maybe we would be doing something differently. You know that kind of thing. But some things I think were purely a waste of time, but it's hard to know what those are beforehand.

Lyon: So in making....some of these questions are, do you think you could speculate or talk about what you hope for the future of the field.

Flavell: Oh yeah, right. I spent some time this morning thinking about these questions. The field, yeah go ahead.

Lyon: Can I suggest we do this and then maybe I'll pause the tape, and we could take a little break. And then I can be more literal and make sure that we cover some of the things.

Flavell: Absolutely. So what do you want hopes and fears for the future of the field?

Lyon: Yeah that would be good. Just your hopes maybe.

Flavell: My fear is reduced funding for research and my hope is good funding for research, OK? That's sort of the main one. Seriously, I don't have the impression that we're stuck in some narrow tradition in the way we work and the kind of theories we work on. I have a feeling that developmental psychology has kind of a healthy openness about it. And I recently surveyed the field in the developmental psych article. All kinds of people going in all sorts of directions. There are soft theories and hard theories and new theories and old theories. All kinds of openness it seems to me, say that wasn't true in the heyday of behaviorism And really not much in common except, well even in the heyday of psychoanalytic theory, there was a rigidity and a narrowness to those things, which I don't think is true in development psych. I think developmental is basically in a pretty healthy state. That doesn't mean you can predict the changes, you just don't know about any field. But it seems to me it's big enough and there's enough variety there

is enough talent that probably things should be pretty optimistic. But of course funding may stay down. I don't know about that.

Lyon: Did you have any comments about the first question? The continuities and the discontinuities. I know we talked a little bit about that.

Flavell: Right here, in the field you mean? Well my feeling as I kind of look back, since I've been in the field anyway, is that when I came into it, learning theory and psychoanalytic theory were the main thing and behaviorism. And those were there. And then Piaget kind of m the field, that really changed dramatically the field, you know what psychology was like. That and other things. Also attachment theory was an important change. It kind of replaced psychoanalytic theory thinking about parent child relations, stuff like that. Then the information approaching movement had a big effect on all of cognitive psychology and also cognitive development. So you've got a lot of our work now really started, well we start thinking, well gee maybe this approaches the main problem. Maybe we even do a form of modeling of some kind like what most people do. So I mean that's really been important. Then the information processing approach affected Piagetians, and some of them became neo-Piagetians, because of that. That's the Case and Fischer example. To really represent an attempt to kind of synthesize the best of Piaget and the best of information processing.

Lyon: So what is the tension that exists between information processing and Piagetians. What was it that the neo-Piagetians had to adjust in Piaget in order to be Neo.

Flavell: I think probably the big across the board stages, had to be pared down and reduced and made more complex and so on. The other thing that clearly had to go was Piaget's groupings. Groups and formal models of the child's mind. Even Piaget got rid of those finally I think. So there weren't, at least those two.

Lyon: Are there ideas that came about out of Piaget's later work that influenced you or influenced the field.

Flavell: Let me think. Not very much that actually I can remember that's influenced me anyway. I think Piagets influence just plain waned. It just simply waned. Most of the influence came earlier. His most recent work didn't, I mean there are devoted followers around quite a number of them. But they are still a decided minority. If you have to ask what is the vanguard in the field. If you look to the people who are the closest, the people who are kind of out front who in my opinion anyway, who are closest to Piaget, would be people like neo-Piagetians. I don't see so much the Beilins or, Chapman. I don't really see that the devotees of Piaget theory are making the big contribution. That's my personal theory, I think these people are able smart people but I don't think that there is much at the fore front in cases and so on.

Lyon: Right do have any feeling for, like are there areas of, well you know people often talk about connectionistic modeling as be a, sort of the next frontier of research, did you have any feelings about that there are new areas that are going to be extremely influential?

Flavell: Yeah I do, I think that's one of them for sure. I think also to some extent the, Esther Thelen stuff, what's it called? You know that dynamic systems. I also you know in the way of the future, it's hard to tell how things are going to work, clearly you have to identify things which are definitely at the forefront right now or just emerging and are promising looking guardedly at least and include those too for sure. Maybe there are some other ones that I haven't, I can't think of at the moment. Oh yeah, of course another one would be, you know cognitive development as theory development. Wellman, Gopnik, and Susan Gelman ---.

Lyon: And that's associated with this idea maybe that thing are more domain specific or the popularity and success.

Flavell: Right this is definitely against the Piagetian grand theory. And against even the neo- Piagetian stuff. There's less belief in grand theory among these people, none at all basically. None at all. They don't expect any synchronism across the different areas.

Lyon: Now from your own perspective are you less of a domain generalist than you used to be?

Flavell: Yeah, I'd say so. I am somewhat reluctantly. I like the idea, I think maybe everybody likes the idea of the grand theory. I just don't think it's sustaining very well. I'm skeptical about the Cases and so forth. As much as I admire what they've done. You know I really wonder if it's really going to work out. If there's any way you can measure the demands of a task. I'm not sure. But I think they're still contributing something here. Halford for example, is a good example, in many ways he's pretty good. Not everybody would agree with me, I'm not sure Ellen Markman would, for example.

Lyon: We'll have to interview Ellen. That would be actually really interesting. So if you were to characterize what are the major changes if there are changes, in your general outlook on how you envision development since the time you started studying this area. What would you say?

Flavell: I think it would be a continued interest in steps and sequences and stuff like that. But less optimistic view that there's going to be grand stages and have med sequences and med ages of acquisition and that sort of stuff. So big stage theory hasn't looked very strong for a long time. And some of these writers will actually say that I guess. I would like to be able to preserve some pieces of it but I'm not sure that's working out very well. So I myself, don't consider myself a stage theorist any longer. So that's how I'm the same and how I've changed. It's pretty easy for me to say how I'm the same, because I have actually a very simple minded view of what it is I'm best at. As long they continue to pay me for it, as it were, I'll do it. I think what I'm best at is turning over stones and finding new development ---, identifying things, curiously a little bit like Al Bandura in a way, in the sense he figures out what could be, and then checks it out and sees if it's there. As opposed to, the very opposite of someone like Siegler who takes a well worked up phenomena like --- balance thing then models it. I'm nothing like that at all. I, in my mind, what I do is characteristic of what I'm doing now, is like what I would have been doing years ago. What is there in the naive theory about consciousness? What is there to be acquired in peoples informal ideas about this stuff? Map it out as best you can, try and figure out what it might be. And then start studying it. Often in kind of a simple-minded descriptive way. You know the little kids don't have it but older kids do have it. Sometimes it doesn't get any further than that and go on to something else. But that's sort of the way I've been doing it for a long time. So in a way, I think what it requires to be honest, not an enormous amount of analytic power, fire power. I mean it doesn't require an enormously high IQ, I think it requires intuition and imagination. I'm better at that than I am at the, I would bet if were to really measure it I would have the lowest IQ of all of the major people in the field. I mean literally measure it in the usual way. And what I'm pretty good at, I think, sometimes better and sometimes worse is this kind of thing, having an image of what things might be like. And also sometimes finding really clever ways of testing it. Not great research designs with all the right controls and all that kind of, not terrific at formal design of research but sometimes pretty good at inventing tasks that would capture what you're after. So that's what I think my strengths and weaknesses are.

Lyon: OK. Great you got to the other questions I had to cover. Would it be fair to say, it sounds like you're a little less interested in say, mechanisms of development. Or causes of development.

Flavell: I'm interested in them, but I'm not likely to be the one that's going to study them. There's a difference really. It's just like in teaching the graduate course in cognitive development. You know, clearly we have like two sections on that. I'm interested in it but it isn't my thing. It isn't something that I'm likely to contribute a lot like Siegler would do better on Piaget than I will. So I'm not likely to do a study, for example, whether children with so and so previous child rearing experiences, are going to be quicker at acquiring theory of mind things than kids that don't. Somewhat an antecedent consequent type study. What are they going to do with this anyway are they just going to keep it?

Lyon: I think someone is going to write it up at some point. OK, these are the questions that we didn't cover in some form or another. The first one is, are there any political or social events that influenced research and writing.

Flavell: Can you show me where that is?

Lyon: Yeah, number four under general and intellectual history.

Flavell: No, I don't think so.

Lyon: Can we talk a little bit about your experiences with the research funding apparatus. What kind of participation you've had in shaping the funding policy and implementation. As well as...

Flavell: I'm looking for that too.

Lyon: I'm sorry, let me just tell you, personal research contributions number five.

Flavell: Number five sure. Well actually I've been pretty well funded over the years. I've had NIH, NIMH, and NSF grants and also fellowships and so forth, for most of the time when I've needed it, during the twelve year. So I haven't been without money or I've not needed money, so my experience has been very positive in that respect. I've also participated in the sense that, I've been on two study sections for NIH, one in the early 70's and one in the late 80's, early 90's. So I've also contributed and of course I've also done some ad hoc stuff for NSF.

Lyon: OK. Under personal institutional contributions we talked a little bit about your experiences as a teacher under number three. But if you could just mention what courses you've taught. And then if you see a tension between teaching and research in the field of child development.

Flavell: Well I've taught developmental psych, cognitive development, language development and I think I even taught an adjustment course one summer at Berkeley. And I've also taught diagnostic testing and therapy.

Lyon: Was that at Rochester?

Flavell: Yes. Not since then. Not since '65. So all the clinical teaching stopped when I moved to Minnesota. Nothing basically but developmental stuff.

Lyon: And do you see a tension between teaching and research?

Flavell: I don't really find any tension. I would prefer to be in a job where I had both rather than just one. I don't find that teaching interferes with my research any more than it should. And certainly I think the research --- the teaching. Of course teaching in the broad sense including training students, especially good students, in research. And that's really what I consider a very important part of my academic life. Probably one of the most rewarding things I do, I mean personally is working with graduate students on research. I really enjoy doing it and I think it's probably a valuable function for the research and for the student and for me. So I think that's teaching in the broad sense to include that. I would not like to do without that. I can't imagine going out someplace and doing my research with just a hired research assistant, and never having any contact with students.

Lyon: Oh yeah, I think that would be harder, it would be less stimulating. The next one is the number four. Describe your experiences in so called applied child development research.

Flavell: I haven't really done any.

Lyon: There are some applications of your work that you probably, in terms of educational applications.

Flavell: Yeah, to be sure. The meta-memory and the meta-cognitive work has had an influence on education. And maybe it's still available but I know that the meta-cognitive ideas have been my most important contribution to education if they've been important at all. Not so much I think in perspective taking stuff. Some of that's been tried with teaching kids. Like Chandler did some work years ago, with kids and problem was trying to teach them perspective taking skills. But I haven't really thought of most of my work as having an applied component.

Lyon: Let's see, under personal notes, the last one. We talked a little bit about your personal interests and your family. But is there anything else you'd like to add about that? About how these experiences may have had a specific bearing upon your specific contributions?

Flavell: Well, I think actually, you know having an, at that time, ordinary but now more traditional or old fashioned kind of a marital relation. Ellie in fact does most of the cooking and shopping and I do odd household chores. But what she does, leaves me more time for research and probably than would be the case, certainly would be the case if both of us were professionals. So that's made, given me more time. And the more general sense of the question she's been enormously supportive in all kinds of ways in sort of keeping my life on an even keel, in a nice way. That sort of made it, you know, kind of cleared the way for happy living. And also I suppose for work. Clearly I haven't had what you would call a stressful life. I wouldn't describe myself as having had great hardships either in childhood or adulthood.

Lyon: I know now you enjoy golf and you play the piano. Are there other things?

Flavell: I play tennis and we like to go on trips. Ellie loves to travel and so do I. We go to the beach a lot and go here and go there. In fact we're going away this weekend for two nights. And then the piano has become an important part of my life. Actually it's been more than just a hobby, it's like a release or something and I really enjoy it. So I faithfully practice an hour a day. And I take lessons once a week, so I expect to keep up with it. I think I'll hit the concert stage.

Lyon: Right that will be the next step. OK, well thanks very much John, this was fun.

Flavell: You bet.