



SRCD ORAL HISTORY INTERVIEW

Marshall Haith

Interviewed by Robert Emde
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Emde: Okay, Marshall the first part actually of the interview has to do with the heading of general intellectual history. And what this is, is an interest in sort of having you reflect on your family background and any kinds of experiences growing up that might be of interest. I can either start with some specific questions like, where were you born and your schooling and early work experience, and then we'll get into moving onto your intellectual development in high school and college, and how that moved into your getting into child development.

Haith: I came from a family that was not well educated, but was extremely supportive. Probably the prototypic Jewish family that emigrated from Eastern Europe, for whom education was terribly important, but people being so poor that they couldn't really afford it when they were growing up. My dad emigrated from Romania in 19 - I think around 1914 or so. He was born in 1907, and he didn't go past seventh grade. My mom made it through eighth grade, and so you can kind of get a feeling for what understanding people of that genre had of education. I was really the first person in my family to go through college, and the first person in a very big family. My mom had nine brothers and sisters, my dad had nine brothers and sisters, and among all those people I was the first to get a PhD. I had one cousin who went to medical school. But the family was very supportive. I grew up in a middle-class neighborhood where intellectual activity wasn't really very salient at all among my peers, and engaged in a lot of sport activity and social activity, and that went certainly all the way through high school. There were a couple of courses that grabbed my interest primarily in English, but I really didn't turn on to academics at all at high school.

Emde: And where was that?

Haith: That was in Kansas City. I was born in Chillicothe, Missouri, and my folks moved very early to Kansas City, Missouri where I grew up. I went to the University of Missouri, primarily because all my fraternity friends did from high school, and immediately became social chairman of my freshman class in AEPI, and social chairman for the general fraternity. Again, found classes pretty easy, but not something that I wanted to spend a lot of my time at.

And then, also at the time I was working quite a lot. My father owned shoe stores. I really started working when I was about nine. I used to come to Denver, where I am now, and my uncle owned a drug store, and he taught me to be a soda jerk; I used to make sodas and malts behind his drug store counter when I was nine. And when I was eleven -

Emde: What was the best soda you made? Do you remember?

Haith: Cherry.

Emde: Okay.

Haith: But my uncle taught me how to make ice crème cones with a lot of air in the middle, so they looked big and they don't have a whole lot of substance.

Emde: Yeah.

Haith: And in Kansas City I worked most weekends in shoe stores from the age eleven on, selling shoes. That's what my dad did. He owned self-service shoe stores. I really thought of myself as going into business with my dad most of the way through college. I did pretty well in my courses, was at a branch between literature and psychology in my junior year, and the University insisted that I decide. And because of my parents' business background and my feeling that they couldn't really understand my going to college and coming out with a degree in literature, I went into the other area where I had about an equal number of courses, and that was psychology.

But really, again, I was just sort of motoring along until I took a course with Melvin Marx, who taught a course in history and systems of psychology and wrote a very strong book in the field. He was testing out drafts of his book with students, and these were the old purple mimeographs that we used to get. At the end of the course he said he wanted them back, except for those people who were going to graduate school and asked how many there were, and some people put up their hands. I was too embarrassed at that time to ask, but after the class was over I went up and asked what graduate school was, and he described it to me, and it sounded to me like a good way of staying out of the shoe business, so I applied.

Emde: It was a better field.

Haith: Yeah, right. I applied to only two places, Columbia and UCLA - oh, actually three, USC. I was turned down by USC. I was accepted by Columbia and UCLA. I picked UCLA because a friend was going to California, I didn't know anybody there, and I was going to be accepted on probation, because I had decided so late to go into psychology that I was missing eight courses.

Emde: That's quite a few.

Haith: So during my first year in graduate school, and because I was so low on the priority list, they didn't have any support for me. So I took a RA-ship with Margaret Jones, who was the wife of Noel Jones, who was the Chair of Psychology, and she was an engineering-type psychologist and was doing smog research in the engineering department. So I did my first year RA-ship testing - cooking up smog on the roof of the engineering building at UCLA, and then piping it into people's eyes through goggles to determine thresholds for various pollutants.

Emde: And this got you into your interest in vision?

Haith: Yeah, right. Or lack thereof. At the same time I was selling shoes on Thursday nights and Saturdays to make some extra money, and making up the eight courses that I was missing, and taking the regular graduate load, so I was pretty busy my first year. And I was in clinical. The one thing that had turned me on in undergraduate school was a visit to the Menninger Clinic in Topeka; I took a class in exceptional child, and thought that I wanted to become a clinical psychologist.

Half way through my first year, taking a course on TAT and Rorschach and reading Fenichel, I decided that I just didn't have the tolerance for ambiguity that was required by a pursuit of clinical, and so went into developmental psychology. My second year - I'd done quite well in my first year, and so Wendell Jeffrey asked me if I wanted a trainee-ship on the training grant, and that sounded real good. So I just kind of ambled into developmental psychology, in fact, I really spent my first couple years studying early experience manipulations in rats, and then my wife Sue got pregnant. I got married in my third year of graduate school, and she got pregnant nine months later. I was studying curiosity in rats, but I had a fellowship in development and a child on the way, so I figured maybe I ought to start doing some child research, so I sterilized my apparatus from the rat cages and took it into the newborn

nursery and started studying infants' curiosity, visual curiosity. That's really how I got into visual development and stayed there ever since. Arthur [Hawley] Parmelee was at the medical school at the time, and helped me get into the hospital there and to start carrying out newborn research. I was really the first person in psychology at UCLA to work with young infants. I started the tradition of infant research there. Hawley was really helpful to me. Ivar Lovaas was on my dissertation committee also, and I did some summer TA-ships for Ivar, helping construct some of his M&M machines that he used with autistic kids. So basically, I can't tell a story of a strong intellectual background that clearly directed me toward my life pursuit. I was working in shoe stores and doing what all my peers did, and some things caught fire for me in undergraduate school, some people were pretty influential, Melvin Marx, Ed Ziegler. I ended up doing a senior honors thesis with Ed Ziegler who was at University of Missouri for a couple of years.

Emde: Oh, I didn't know that.

Haith: Yeah, he was quite influential in supporting my going on to graduate school. We had a class of some forty first-year entries into graduate school at UCLA, and eight of us came out with PhDs. It was sort of survival of the fittest philosophy at that time.

Emde: Was that deliberate?

Haith: Yeah. We were told in our first meeting of the pro-seminar that we should look around because half of us wouldn't be there the next year. It was deliberately set up. It was a head to head competition.

Emde: What do you think of that?

Haith: Well, it certainly works as a screen. I guess we had an over-abundance of people going into psychology at the time, so they could afford to do that. We have a much more caring and nurturant philosophy now, but I'd say of the people we turned out, probably a higher proportion of those who got PhDs in my time ended up in productive careers than the people who are going through now.

Emde: Productive careers in general, or are you talking about research academic careers?

Haith: Research academic. I guess there's a reasonable number of people who end up in, you know, other positions, certainly in clinical, but there were also a reasonable number of people who go through research training and don't do a whole lot with it. Still there are plenty who do, but I'm just saying proportion-wise, the screens were so difficult in those days that I think they probably lost a lot of talent by doing what they did, but on the other hand, you had to be pretty serious about what you were doing to get through it.

Emde: It maybe steeled your motivation as well if you stuck it out?

Haith: Yes.

Emde: You certainly described having contact with a number of people who certainly became or were very prominent in developmental sciences and developmental psychology, Wendell Jeffrey, Ed Ziegler, Hawley Parmelee in pediatrics and broadly behavioral pediatrics. It seems to me that might be one thing that you sought out and made use of, people who inspired you in some ways?

Haith: Right. Well, I was quite fortunate, and maybe it's leaned me into the direction of really trying to help younger people. It's been an important part of my life, because so many people helped me. After I left UCLA, where I was primarily under the influence of Hawley Parmelee and Wendell Jeffrey, I went on to do a post-doctoral fellowship with William Kessen, and that just couldn't have been a better environment. And not only because of the direct effect of the mentors, but because of the kind of environment that they create, you know, I think you and I both believe that people learn as much

from their peers or more than they do from the mentor, but it's up to the mentor to establish the kind of environment that makes that possible. If you create an environment where people are super competitive and not contributing to one another they lose a lot in terms of what they can get from their peers. In UCLA, Jeffrey was very much oriented toward creating a social environment, and an environment of collaboration so that a strong peer influence on me was Patricia Goldman Rakic, who I went to graduate school with. We were very close, did some research together and we felt, I think, that our dialogs and arguments and opportunities to discuss things were tremendously important to our development. At Yale, Arnold Sameroff and Susan Harter, Phil Salapatek were people around me who just made an enormous difference in how I developed.

Emde: So they were all peers when you were there?

Haith: Phil was in graduate school, Susan was finishing up, and so was Arnie. I was the only post-doc. Katherine Nelson came afterwards - I mean there were other post-docs in the department, but Bill was a tremendous intellectual influence. He really helped teach me how to write, wasn't terribly directive but he was there when he needed to be.

Emde: It sounds like there was an open atmosphere?

Haith: Very open, very open, and we felt on the top of the world, I mean we felt like we were at the forefront of everything that was important in developmental psychology. And as you know, I went on to Harvard, and Jerry Kagan stepped right in and without getting in my way in any way, shape or form, was very, very supportive and encouraged me to continue my collaboration in New Haven, which I did, while I slowly grew into Cambridge. And it just - that makes a tremendous amount of difference in how a person develops, whether they develop as an isolate and always in competition with others, or whether they see themselves as a collaborator and sort of play their role as time moves on in passing on to their own students what they got from their mentors.

Emde: Well, that's a wonderful account really of that very point, and I was intrigued also to have you reflect a little bit more on the personal, in addition to the mentoring that you received in the sense of the strands that I heard. It seems to me another theme that ran through your thinking about this was your curiosity. Is that right? Your curiosity drove you to go beyond. To go outside of any particular place, time or box that you might have found yourself in. That seemed to be a theme?

Haith: Well, I've been a little stubborn. The starting of infant research at UCLA, especially with newborn babies was no treat. Jeff was encouraging, but the UCLA environment at that time was very empirical, very stimulus response oriented, "Why in the world would you ever study infants?" You couldn't talk to them, you couldn't cut them open, you couldn't shock them, so at least with adults you could talk to them, you could get direct answers, you could set up apparatus, have them press buttons when they're instructed to do so. With mice, rats, you could shock them, do whatever you want, but here you were with babies where there was not a technology. This was before Fantz's work came out. Actually a large part of my support system came externally from communications that I had with Herb Kaye at Brown University and Rachel Clifton at, I think she was at Wisconsin at the time. Each of us discovered an article that came out of Russia on infants sucking activity as being affected by a perceptual input, and we were all so hungry for a methodology that we jumped on techniques to measure sucking activity to try to get at what kinds of perceptual sensitivities these -

Emde: The Bronstein effect and things like that?

Haith: That's right. Bronstein, Sytova, Antonova, it was quite a few. We had trouble replicating some of the results, but it was just indicative of what the technology at the time was. It never occurred to us to look at the babies' eyes, we were studying things that you could count, and those were sucks or respirations or heartbeats, because the field was very quantitative at the time, and it just didn't occur to us to look at the eyes. I had to overcome quite a lot of sneering to do infant work, but the exciting

thing was that hardly anybody knew anything about it. And, I guess if there's been a theme to my career, as with your career, I've gone for the islands. I just haven't felt good on the main continent, where people are all doing similar things. The advantage of that is they have hypotheses, as you say, and they can test them and make progress, but I've always gotten myself into areas where not much is going on when I get started and I'm kind of floundering around. It's led to a lot of my research being in a way demonstrational.

Emde: Well, you've innovated in method too, because you had to.

Haith: Methods and areas. I think it's hard for people to tell if I'm in perceptual development or cognitive development, and I almost don't know myself. The work that I did on visual scanning early on, the work that I did on trying to get at rules that govern infant behavior and the agendas that they have, those weren't questions that people were asking, in fact, to some extent I think people still have a little bit of trouble understanding what I was really after in that work, and it continues to this day in the work that we're doing on future oriented processes. That was a new area, and it still to some extent is.

The one area where I researched in - we got in early, but so many other people jumped in that I got out, and this was in the area of information processing, visual information processing in older kids; kids between five years, ten years of age. I don't know if I don't like competition, or if I just like being where there isn't a whole lot of activity going on, but I've always found it more fun to try to work in areas where one has to develop new methods and figure out new ways to get information about phenomena.

Emde: Well, that's fascinating. Let me also take another cut at this. Why newborns, and why vision, because it's really early for both in your career, and it's persistent?

Haith: Right. Well, remember I came from an SR training in graduate school, and there was a real excitement about getting this organism that had absolutely no experience. In those days we really didn't think about intra-uterine existence as constituting experience for the baby. We really thought about the newborn as being a blank slate. So here you could take this organism at the very beginning and you could learn about what kinds of inputs it was sensitive to, and at least in those grandiose, completely unrealistic days, you could imagine that you could track the experiences of infants into childhood and really understand how everything got put together. Now, it seems silly at this point, but at least as a young graduate student, there was something very exciting about studying newborn infants. And you did have a captive audience in the newborn nursery., They were there, you didn't have to go out and bring them in - and we didn't know how to do that, I mean nobody was doing that, nobody was bringing in infants, so how would you bring in an infant? There was no lab there to do it in. So I think to some extent it was something that you could do.

Emde: And you had Hawley Parmelee.

Haith. And you had Hawley Parmelee who knew what he was doing over there and had some residents who were doing research and helped me out. Why vision -

Emde: Was it trying to get the smog out of your eyes?

Haith: Right. Vision is I think we all agree, man's most important sense, and something exciting about this newborn infant looking out in the world and what kind of sense could you make out of what it was saying.

Emde: And you didn't subscribe to that common belief then that newborns couldn't see?

Haith: No. I mean all you had to do - I mean it was amazing that that kind of stuff existed in medical books, because all you had to do was go pick up a baby and look at it. I mean I saw babies, how awake

they are when they don't have a lot of anesthesia, I mean you could look at a baby in the first half hour and it just was looking all over the place; looking out of windows -

Emde: So that must have struck you right away?

Haith: Yes. And I was able to show that babies would track moving lights and would sustain interest in them at a time when people had no way of thinking about that kind of stuff. Berlyne's book had come out on curiosity, but people were still trying to think of curiosity as primary motive and instrumental, for getting food and whatever. Who talked about babies just looking around? Why would a baby do that? And I had just discovered that, in fact, rats would press a bar just to see lights move around - I used the same panel, I really did, in my rat research as in my infant research. I created an array of lights in a circle and never published this work, but people published work like it many years after. To demonstrate in a skinner box that these rats would hammer away at this bar just to see these lights move around, well this was a time when people were saying that rats only hit bars to get food or get water -

Emde: Especial Jerry Bruner twelve years later.

Haith: -- but they would sit there and pound away at these bars to see these lights move, and that was fascinating to me.

Emde: And you appreciated it for what it was.

Haith: Oh, absolutely.

Emde: Because most people wouldn't have.

Haith: Absolutely.

Emde: And you didn't publish it?

Haith: Jeffrey just about had my neck for not publishing that work, but I was a real perfectionist in graduate school and I never could get it quite perfect enough.

Emde: You mean you didn't have the key values down, or the controls or what?

Haith: No. I either wanted to have more rats or, you know, I didn't have the timing right. It was just one thing or another. And -

Emde: But you did believe it?

Haith: Oh yes. There was no question. You could run control periods where rats were in the box and the lights could be on or they could be off and they wouldn't press at all if nothing happened, but as soon as the lights started moving when they pressed the bar they really increased their bar pressing.

Emde: So how do you view curiosity? So you don't like to view it as a motive?

Haith: No. I don't think the motivational analysis worked very well. I think curiosity is a property of living organisms. It's almost a definition of life.

Emde: Like activity? Like cognitive assimilation?

Haith: You know I have a salt-water aquarium, maybe you don't know, but I have anemones and I have some fish, snails, and corals. These anemones - if they move around anything moves around. They're

getting fed where they are. They're getting light where they are, but they explore. And I just think it's almost a characteristic of life to explore and become acquainted with your environment.

Emde: So cognitive assimilation is just another example of that in a more complex way, right?

Haith: Right.

Emde: We're seeking out the new in order to make it familiar?

Haith: We're information hungry machines. One example I like to give is you are driving to work and all of sudden you see a green house at the corner on the left on a path that you've taken a hundred times to work. And you say to yourself, that green house wasn't there before, or it wasn't green before - it was red. And there's no instrumental purpose to your knowing that that was a red house, but you collect that kind of incidental information all throughout your life. It played no role in your earning a living. It played no role in your getting food, sex, water, anything else. It's irrelevant whether it was red or not, but you have that information, and you collect enormous amounts of that kind of information throughout your life, because people are information collectors. Some of that information is more salient than others, but you know about your environment and your surroundings, and there's no good way to talk about this stuff in terms of drives and motivation or anything else. It's a characteristic of human activity.

Emde: So you don't need to think of a property of seeking out the new in order to make it familiar?

Haith: And infants do this all the time. They collect enormous amounts of information. I mean that's really what we've learned in these last few decades. The stuff I do on infant anticipation - why would they acquire this information and try to form expectations for it? It's what makes people human.

Emde: It's complexity, yes, and the future orientation aspect of it. We're really into another domain, which is wonderful in the interview in terms of aspects of your work where you see continuities. What are the main continuities you see in your work? I think we've been talking about some, but it's phrased that way in the inquiry here so I'll ask it of you?

Haith: Well, I think the main thread as I look back - well, a couple of obvious ones, an interest in very early development, interest in perception and cognition, but I think the main thread is a focus on something that now has become quite fashionable, and that is, what the organism brings to the world and the agenda that they pursue, and how they play a role in putting it together. You know at least when I came up, if you think about where I came from, stimulus response psychology was an organism waiting for things to happen to it - to act.

One of the discoveries that I'm most excited about throughout my career, it's so simple, but it's when we turned off the lights in a newborn nursery and we were in absolute darkness recording with an infrared television tube on video, and this infant opened its eyes and started looking around actively. It just hit me immediately. Here we've been thinking of the all- mighty world imposing itself on the baby and somehow the baby assimilating all things that were coming to it, and we gave no thought at all to the fact that this baby comes into the world prepared to act upon the world. It sounds so simple and obvious, but at the time it felt pretty profound. And if you look at the work I've done on rules that babies live by, it's really what rules do they bring to the world to act on it. Of course the world is there and they have to have something to act on, and they do differently depending on what's there, but really the energy comes from the baby itself. That's the starting point.

In terms of the work on expectations I think that what I'm doing is not teaching the baby anything; I think I'm just serving as a really precise observer of the baby organizing the predictable events that are playing off in front of him. The baby can simply respond to these things, but a baby has, I don't want to use the word drive, a strong inclination to organize things, and this is with static-stimuli in

terms of organizing faces and the features in faces and various other fields. So for me, the main thread has been - now they talk about it in terms of biology or in terms of nature, whatever, but it's really been the role that babies' inclinations play in how they put together their world.

Emde: I can't resist asking as a former shoes salesman, have you wrestled with the ideas of goodness of fit?

Haith: A little bit. For my money there are strong individual differences between babies, and you can see this in newborn babies. I don't think I've ever talked about this in literature, but in recording visual activity in even newborn babies, there are some babies you put up a display for that baby, say a triangle, and one baby will come in and it'll look at one angle and it'll inspect it a whole lot, and then it'll move to another angle and it'll inspect it a whole lot and so on. Another baby will come in and it takes inventory of everything instantly. It's not only all over the place on the triangle, it's all over the field, I mean the baby's head will move and the eyes off the camera field. So you see strong individual differences from the moment of birth, and a lot of other evidence documents that. I think you have strong individual differences in parents too.

My own suspicion, and something I'm not going to live long enough to do, is to understand how parental influences affect the degree of future orientation that kids have. I think they naturally begin to forecast predictable events in the world, but I think this occurs at a fairly concrete perceptual level, events, the consequences of their own action. When you get into more abstract levels of verbal planning with parents of understanding the meaning of traditions that haven't occurred yet, for example, their talking about Christmas, "What are you going to do for Christmas? What are you going to do for a birthday?" I suspect that parents have a lot of influence in the degree of optimism and stability and predictability that kids form as mental models of their world. You have the individual differences of the kids too. We all know even kids in the same family who've probably been raised pretty much the same way, some of them constantly worrying and thinking about the future, and others being devil-may-care. So my guess is, if you have kids who are relatively impulsive, relatively present-oriented, and you have parents who give their children a fairly unpredictable, disorganized life, who don't have a lot of traditions, who don't talk to them about things that are coming up and are going to happen -

Emde: Or have television on all the time?

Haith: Right. You're going to end up with kids who don't do a lot of planning, don't think about the consequences of their actions.

Emde: It sounds like there's a role there and you're thinking that there's a good—of different matches of the parent environment with different givens of individual differences. That's very interesting. You describe continuities very nicely. What about shifts in your thinking or your work?

Haith: Well, the major shift was at Yale, where I went as a stimulus response psychologist --

Emde: Really?

Haith: -- and everybody was around --

Emde: It didn't sound like it?

Haith: Oh, yes. Well, I mean that was my training, and actually even the stuff on curiosity and on sucking activity at the time was people were trying to cast it into a reinforcement framework. I think of Harriet Reingold's work where infants had to press bars to see things so that the visual events were seen as reinforcers. In fact, even for a little while at Yale, we were not - we didn't have video at the time, we had cameras and we took pictures, once each second, and so it was possible to see the eye in

different locations on successive seconds as being a kind of response with the baby looking at a particular location, habituating to that location, and then being attracted to something else. But, there were people there who were talking about this peculiar guy from Geneva, Jean Piaget, who I had learned was just totally off the wall, and I had pretty much rejected him. I had tried to read him and couldn't understand a thing he was saying and felt he just didn't fit into the quantitative experimental milieu that I was trained in. But, as you know, Kessen was very sympathetic to Piaget, and I think in a way I had no sense of how much I was influenced by, maybe not Piaget, but the environment that was so receptive to him. Over the years, it seems to me, I've gotten closer and closer to Piaget's thinking, and Kessen's thinking and several others around me. At any rate, I certainly came to understand that stimulus response psychology was not the be all and end all, and I think the seeds were sown at that time for me to have this revelation that constitutes the major theme that I mention, the continuity that the baby brings to the world, what Kessen calls congenitally organized skills. I guess I maybe even go a little further than that, because once we were able to get video and could follow continuously what babies were doing, talking about eye movements as responses was just ridiculous, they're moving two to three times a second, they are fleeting from one place to another, it looks like more program of action -

Psychologists, for my money, have still not come to grips with the continuity of action. You heard this from me before, but we have very nice measures for discrete responses and reaction time, indices like that, and we think about experiments as consisting of trials, but we have very poor methods for analyzing the dynamic of behavior over time. Kinesiology gets closer why some people are doing work on tracking hand movements and arm movements and the like, but if you look at the analytic tools that they have for describing what's going on, they're pretty weak, which I find really ironic because psychology for so long was defined as the science, the study of behavior but, in fact, we don't study behavior, we study primarily reactions and responses, which are supposed to index deeper cognitive and social processes, and certainly those are legitimate things to study. But there is a real gap in terms of our ability to study ongoing activity, and that was really part of my mission in studying eye movements, although I can't say I've been terribly successful at it, nor have many other people. You know, when you're having the eye move three times a second and you are trying to capture let's say ten minutes of ongoing activity, how do you capture it? Well, you say the average looking position was here and the average size eye movement was this, and use the standard measures of psychology, but how do you characterize the pattern of visual activity, it's not easy to do.

Emde: There's a query here about your reflecting on the strengths and weaknesses of the research, it sort of gets into that. That's one of the no less challenges and difficulties.

Haith: Right. Well, I think the strengths of the research are development of precise measures and willingness to take on new areas that I think have turned out to be quite fruitful. The weakness is the lack of strong theory.

Emde: Oh. And you're describing spontaneous "never before studied behaviors?"

Haith: Well, but it is a weakness. I have hungered through my career for other people to take on the same problems that I study and to argue with me about them so that we can develop precise testable positions. It only happened once when Phil Salapatek and I differed on what looking at angles meant in newborn babies. He felt that there was a preference for angles, an angle operator, and I felt that it reflected the attractiveness of contour density because there are two edges near one another and I set out to test the difference, and that's the only time that I can remember in my career where someone has directly challenged me on a specific thing and we could go off and do research. And it's a little sad; I mean it's something that you bring down on yourself when you go off in areas that nobody is studying. As I say, I stayed away from areas where there's a lot of other activity, but you miss something and you don't develop theories in as a precise way when other people are missing to challenge you on the specifics.

Emde: What manuscripts do you think best represent your thinking about child development?

Haith: You know, I'm very fond - you mean that I've created?

Emde: Yes.

Haith: I'm very fond of the rules book, *The Rules That Babies Look By*. I think it was a really neat package. People concentrate on the rules, which I think is a little unfortunate, because I feel they're simply a byproduct of what I call the "infant's agenda," and I think the agenda is much more important. But nevertheless, I'm really happy with that production.

Emde: You mean the agenda for constructing the rules in the first place?

Haith: No. The agenda, as I suggested, was that babies come into the world to activate their own cortex, their own visual cortex, and that the rules are a consequence of this agenda, so when the baby is in dark it scans very broadly because nothing it can do can activate the visual cortex. When you present contours to the baby it tends to dance back and forth across them, and the rules really relate to these sorts of things, but the fundamental underlying principle is that the baby is trying to create its own cortical activity, which has tremendous adaptive significance because it's only through activation that neuro-pathways get consolidated and maintained.

Emde: And now also as a role in pruning?

Haith: Right. And that's part of the maintenance function of the functional pathways and the termination of less functional ones.

Emde: Which get selected?

Haith: Right. So it all made a nice neat little package, and I'm very fond of that one. I'm also very happy with the way the work in future oriented processes is going, and several chapters I've done there, but I'm pleased with the book that we published on that. I think, still there isn't a whole lot of activity in the field, but I think as time passes on that book will be seen to be kind of a watershed in this area. Those are really - and I guess I should go on to say that my involvement with the handbook has been tremendously gratifying. I've helped in one way or another with that since, well, writing the first chapter that I did with Kessen and Salapatek that was published in 1970. I feel really good about that involvement too.

Emde: Good. That's really important to do. A lot of people see that as plain hard work. I say that too.

Haith: I pretty much burned myself out on this last chapter.

Emde: Do any of your contributions seem wrong-headed; is another query we have here?

Haith: Some of them seem pretty light-headed; we all have our lighter and heavier pieces. I don't think so. I don't think there's anything I've taken a real strong position on that turned out to be wrong. Maybe when people finally go back and start doing things, similar things to what I've done it'll be turned up, but I can't think of anything in particular.

Emde: When you were thinking of lightheaded, what were you thinking of?

Haith: Oh, I did a paper for you actually on the hidden meaning of the infant smile. Oh no, that was for - I can't remember if that was for - I think it was a symposium for Phil Zelazo. I said, "Infants smile because they feel good." People seem to cognitivize and make these things so grandiose and intellectualize them, and the basic message of that paper is 'babies are smiling because they feel good about what's happening.' I was trying to get the emotional motivational component in there.

Emde: Good. Glad to hear it. Maybe we could conclude this session with reflections on research funding over the years and - reflections on that including your participation. I know you participated in a number of study sections and councils, and just the whole process including changes that have taken place, your participation and -

Haith: Well, the gods have been good to me personally, I was funded on a training grant through graduate school, got a post-doc grant to go and work with Kessen on an individual one, got a grant my first year at Harvard in 1966, and have been funded continuously since then. And as you know, I was able to join you in a research scientist award for seventeen years, so the gods have been really good to me and it's made an enormous difference in my career and what I've been able to do.

In your interview you mention how you have to convince your mentees about the importance of leisure time to do research. I might say it a different way. I think you have to learn that you have to do things that almost seem like a waste of time to do thoughtful work, especially when you contrast it to administration and teaching, where every moment of your time results in a product, a memo or a paper or a lecture. A lot of the heavy intellectual work is just sitting there grinding it out and chasing a lot of blind alleys. And then discovering something that seemed so simple that you are embarrassed to even, to some extent to publish it. It took me maybe eight years to discover the principle that I put forth in the rules book. I mean reading hundreds and hundreds of papers, spending enormous amounts of time thinking, I came to such a simple solution, and it embarrassed me that it took me so long to get there, and that's happened to me several times throughout my career. And so the funding makes that possible, and I think given the strains that institutions are under these days to meet the bottom line, the pressures that take you away from that kind of very productive, but non-product oriented activity are severe, and that funding is the only way to nurture and preserve this kind of intellectual activity. So it's certainly been helpful to me. I have served on three NIH Grant Review Committees for twelve years, and on the March of Dimes panel with you for seventeen years, and certainly have seen trends come and go. I obviously find something about it that's attractive. I think there's been partly a feeling that I should pay back for all that has been done for me. But, you know, that's not all of it. It's another intellectual activity, reviewing grants with your colleagues, and getting to know colleagues and thinking through what will make a contribution both to the field and to the people. Sometimes the people get forgotten in this business, we tend to think that the content of the field is somehow independent of the people who do it, but the people are important too, and you're making a bet on people when you fund them.

In the early years it was wonderful. I think we were able to fund most of the stuff that we felt - and you and I served on small grants together - most of the work that we felt should be funded we were able to fund, even though we used to obsess over our grants of five thousand dollars each to people like Harry Harlow. I think we did a lot of good and I think we felt we had the resources to do most of what we wanted to do. I think that was true to some extent of March of Dimes too, although a lot more money could have been put to good use in March of Dimes.

There was a period there in NIH, which I know you're well familiar with, where it just got to be outrageous. The funding levels dropped to the twelfth and thirteenth percentile, even lower than that, and very high quality people were not funded and were just actively discouraged. And I think there's no way that we can get across to the legislature, the government, what sort of long term impact those kinds of barren periods have on a person's career. I think it finished careers for some people, for other people it made them very bitter. For others there wasn't the continuity in their intellectual life at a particularly flowering time for these people, it would have really helped them to grow, and I feel real bad about that. I know many people who simply turned off from research, and I think that the quality and number of applications to graduate school declined precipitously after that experience.

Emde: This period roughly when?

Haith: I would say roughly 199 - well, going backwards - no, let's go forward. Maybe 1990/92 to 1997. Does that sound right to you?

Emde: Maybe even a little earlier.

Haith: Maybe a little earlier than that. We saw clear effects on the talent coming to the field, and now it seems as though we're coming out of it again, but I still feel there's a problem in that the drivers for the up and down cycles are the medical profession, and that the value of psychological research is still not fully appreciated, we sort of ride the tails of the medical industry, and the pressure groups that get the Congress to vote the funds.

Emde: Marshall, this section of the interview has to do with institutional contributions over the course of your career, and in the last session we had you talk a lot about your intellectual biography in terms of important teachers that you've experienced and places you've been in the course of your career. In this section, I think we're interested in your reflections on the universities and the research institutes that you've been affiliated with, changes that have occurred and achievements, frustrations, and any thoughts that would be useful for historians in terms of the context in which you've worked in these institutions over time, and it occurs to me it'll be particularly interesting to have you reflect on it, because not only have you been in a number of different institutions, but since you've been, I know, here at the University of Denver, you've played ever-widening roles in institutional settings with respect to training and with respect to research and research administration. So with that background I'll let you reflect.

Haith: Okay. You may have to remind me some of these questions.

Emde: Sure.

Haith: I'm going to kind of overlap a little bit with what I said before. My undergraduate work, as I mentioned at the University of Missouri where I did a senior honors thesis with Ed Ziegler, that was really the only research activity I was involved in, and I did that on the effects of early experience, reviewing Pinneau and Spitz's controversy, which - and I was very much on the Pinneau side at that time, being a fairly obsessive experimental type. I've softened in my years, and went on to UCLA to graduate school, and as I mentioned, there was no infancy work going on there, and I had the good fortune to be able to combine resources over in the medical school and in the psychology department in the developmental area to start a tradition of infant behavioral research there, but more psychological type because, of course, Parmelee had pioneered behavioral research with infants. At any rate, I thought I got it going as far as the psychology department was concerned.

Then moved on to Yale where, as I mentioned, a post-doc with Kessen changed my whole view of development, introducing me to Piaget and moving me away from SR psychology. At Harvard, Jerry Kagan played a really instrumental role in my development, and early on Jerry Bruner did too. I was part of the Center for Cognitive Studies at Harvard in the Psychology Department, and then the other side of my appointment was in the Department of Social Relations, at least for the first couple years, and then I became full-time Social Relations.

Emde: So you were in inter-disciplinary settings in a sense?

Haith: Right. I sure was, and I came across some very interesting people there; Colwyn Trevarthen was one of them who I interacted a lot with in my early years. And, you know, that was really the seed of my career development, that's where I ran my newborn studies and made the research that was responsible for my book later on with newborn babies at Cambridge Hospital.

Emde: In terms of the institutional background, do you think the inter-disciplinary environment that was there at Harvard at that time where you're reflecting on now was crucial, and if so, in what ways to your career development?

Haith: Well, that's kind of a hard one. I don't think that the inter-disciplinary influence has been reflected that much in my research, although, you know, in a way perhaps because I have worked over a fairly broad age range, all the way from newborn babies to adults, and I think my exposure to George Miller in the Center for Cognitive Studies, Jerome Bruner, a person named Phil Liss and others had an influence upon moving me into the information processing tradition at Harvard, and that thinking pattern that went along with that has influenced my infant research and affected how I think about what's going on in infancy and in development, but I was exposed to a lot of people in anthropology and social psychology. It's hard to really point to influences there. Some of my research on infant looking at adult faces and how I've conceptualized that, I guess was affected by my exposure to people in social. I think it's affected more my dealing with students who have wanted to go off in different directions and my tolerance of their desire to do that, my awareness of what's going on in developmental psychology. But you know, developmental psychology by nature is interdisciplinary, you simply can't stay in a particular fold in developmental psychology, there are too many forces pulling you away. After I left Harvard and came here, the developmental psychobiology research group played a large role in keeping me from living in my closet and only considering the local perceptual research that was going on at the time. It's such an eclectic context that, as you know, you have to think at many levels at the same time because people force you to do that in that environment. I think my experience in Denver has been very rich; you've contributed a lot to it, Marty Reite, many people in ways that they don't even know, just by virtue of their willingness to share their work and to make it available to people outside of their own disciplines. It's been a very interdisciplinary environment, and probably one of the best places to work in the world in developmental psychology.

With respect to other kinds of institutions, I've spent a lot of my time on grant review committees, and one knows how that opens one's eyes, not only with respect to the colleagues that one collaborates with in reviewing grants from other institutions, but the various fields they come from. I think my serving on NIH small grants between 1993 and 1978 was a great example that we considered - you'll remember because you were on that committee also - grant applications all the way from psychopharmacology to anthropology to archeology to developmental psychology to neurophysiology, and I mean grant applications came to us simply because they fell in a certain monetary range, not necessarily because of their content. That was a very exciting time for me, it was my first grant review committee, and I met a lot of people who I've stayed in communications with, and really learned how to think about federal support of research. We considered a lot of innovative and high-risk research. It wasn't a lot of money, it was five thousand dollars a grant, but that permitted us to be much more flexible in our thinking about the kind of research that we were willing to take chances on.

And then I served on the NIMH Research Education Review Committee; these were training grants and pre-doctoral/post-doctoral applications from 1983 to 1987. We made a lot of site visits, and I got a good sense of the sorts of training programs that were going on around the country, and developed a perspective on training that was very valuable to me. Now I'm serving on the NIH HUD1 Review Committee, 1996 to the year 2000, and that's still a different format with focus on RO1s primarily, big individual grant applications, and I've really felt that opportunity was wonderful. It's done a tremendous amount to keep me up-to-date with what's going on, and again, it gives you a perspective on what's going on in the field.

And then, of course, there is the March of Dimes Grant Review Committee that you and I sat on for seventeen, eighteen years, where we saw everything from brain insults, more related to biological pediatrics, all the way to the influences of brain damage on the family, and how family dynamics accommodated to meet the demands of the child. Again, that work came at us from psychology, psychiatry, psychology, and developmental psychology. I think as a developmental psychologist, if you're involved in grant review committees and any sort of wide-ranging forum for reviewing research, you have to be able to think in an interdisciplinary manner, and I think that's what's been so exciting about the field.

Emde: It helps your critical perspective?

Haith: Sure.

Emde: And you're generative?

Haith: Sure. With respect to teaching and research, quite honestly, almost all of my teaching has been done at the graduate level. And for about fifteen years because I had a research scientist award, I didn't spend a whole lot of time in the classroom. Most of my teaching was at the level of post-docs and graduate students, both here at the University of Denver, and I participate quite a lot in training students at the University of Colorado Health Sciences Center through my participation on the Post-doctoral Training Committee for the Developmental Psychobiology Research Group. And, you know, there teaching and research just go hand in hand. That's been a wonderful experience. I've honestly felt that I've learned more from the students than they've learned from me. They've taken me in directions that I never anticipated going, but I sort of think that that's what the research enterprise is all about, that you follow leads and, you know, rather than stick to a strict regimen, I think if you are willing to go along with the excitement and the people you are working with you get a lot more out of them, they get a lot more out of you. And part of this whole enterprise is entertaining yourself as well as laying down an agenda for research and following it religiously, and I've been able to entertain myself, had a lot of fun. One has to be open to that contributions come along because people are most generative when they're doing something they're really excited about.

Emde: And I must say you've been able to convey the pleasure, not just the frustrations of research to your students.

Haith: Well, thanks. I think so, and I think they feel respected, and I think you help to develop future scientists by letting them take the lead and the responsibility while they're still in training. I don't think that you can just slop people into an on-going research program and then expect them to go out and be independent investigators once they get their degrees.

Another question you asked was about the relationship between research and application. I've really been, as you know, more on the basic research side, but I have enjoyed opportunities to participate in applied work where it has naturally arisen, and again, it's often come from my students. We have helped people in other laboratories develop our paradigm for study, for example, the effects of exposure to alcohol, through the Jacobsons at Wayne State University, a case in point. And as a function of our collaboration within, in our own lab, we're now doing some research on exposure of fetuses to alcohol and the effects on brain structure through doing some ultrasounds on fetal brain development, and also post-fetus, and also applying our paradigm to these infants who have been exposed, trying to correlate their performance, both with their exposure to alcohol and the effects on brain structure.

Emde: Were these visual expectations?

Haith: Yes. Additionally, several of my students have gotten interested in using our paradigm as an index of cognitive functioning, and looking to see whether or not this is predictive of later cognitive function. We found in three studies that, in fact, there is a correlation, it's modest, but a correlation between performance on our task as early as three months of age, and performance on IQ and cognitive tasks at three and a half years of age. So again, in developmental psychology it's pretty hard to keep the pace--

Emde: Are people pursuing, by the way, in applied settings?

Haith: Yes. Yes, they are, in Canada and in several places in the U.S.

Emde: To help with what?

Haith: In terms of trying to see whether the measure is a good index of cognitive functioning, and to pick up infants who seem to be falling behind.

Emde: So that there can be earlier intervention?

Haith: Right. And again, being in this field it's pretty hard to keep isolated from application, even if your primary interest is in basic research, because things seem to meld together in developmental psychology, and I'm perfectly happy to see that happen. As you know, Perry Butterfield used some of our measures of looking at eye contact in studying the effects of silver nitrate applications.

Emde: Silver nitrate. And, in fact - yes, she used your infrared technology and collaborative activity in two studies actually in the clinical literature, helped to turn the tide so that silver nitrate, which was then realized to be irritable and affect visual pursuit, the practice was changed?

Haith: Right. So, I've been happy for this to happen. It hasn't been primarily my initiative for my work to be applied, but, you know, I've very excited about it and have been happy to participate. And I think it just shows that basic research, you know, good solid work gets translated and if you are developing knowledge, that knowledge has application.

Emde: It seems to me there are some other areas, I think of one that might be worth a little recounting, where your work, not just in terms of what you've teased out of your own laboratory, but your own work of an intellectual sort and a conveying sort has been very influential in applied settings, such as when you integrate new visions and new ideas. And I'm thinking right now of leadership that you took a decade or so ago in pointing to the need to study future oriented processes, and the impact that that continues to have on many people, including myself, and I think many laboratories and many directions of research. I wonder if you'd care to reflect about that a little bit?

Haith: Well, I haven't come to my work typically from grand theoretical schemes; I've typically let children lead me by what they do in situations that I put them in. We accidentally discovered that infants anticipate future events. This was a long time ago, even as a post-doc with Bill Kessen, and I was doing straight perceptual work and we were studying something else, and all of the sudden I saw these babies making anticipatory eye movements, and it just hit me at that moment how important this was and how little psychologists, and especially developmental psychologists have paid attention to, what I call 'future-oriented processes,' forecasting, planning, goal setting and the like. And my goodness, how important these things are, and we touch on topics that are so related, for example, a teenager's understanding the implications of their action for the future health of their bodies or their future risk.

Emde: By the way, one of the most common clinical things you hear even in counseling with teenagers is trying to get them to understand that actions have consequences.

Haith: Right. You know, planning for their careers, and it just hit me at that moment, even though I was looking at a relatively low level indication of the expectation that infants were forming that, my god the whole field of future-oriented processing is sitting there waiting for someone to take the lead. And you were so instrumental in this whole process through your cooperation in the MacArthur Foundation effort, and I think foresightful in helping me to set up an interest area in development of future-oriented processes. And I was able to involve people from Kurt Fisher from Harvard, Barbara Rogoff from Utah, Patricia Goldman-Rakic from Yale, Rob Roberts, Janette Benson from University of Denver, Steve Resnick from Yale, and we all started programs on future-oriented processes, some of them very low level, such as my own work in expectations, but others more grand in terms of trying to develop the domains of future oriented processes from infancy through early childhood. We're now

looking at language transcripts to see how much parents talk about the future. People are very excited about this notion, because families differ so much in how much they involve kids in talking about the future and planning in providing the infant with a predictable environment. And we're still speculating about the effect that these experiences have on kids' ability to think about the future, that my goodness, these abilities come from somewhere. And one suspects that they come from family organization and how much the parents encourage this activity, and I can't think in terms of cognitive development of anything that could be more important than a child's ability to plan for himself, to think about what he or she will become. Our culture places so much emphasis on goal setting and planning, that it just seems ironic to me that we spend so little time on it. So I feel very good about that. My guess is that if I'm remembered for anything, hopefully it will be partly playing a role in highlighting this important area and trying to get some conceptual grasp of how we frame the questions in this area.

Emde: And in addition to practical applications, you've pointed out quite dramatically from the perspective of an overview of psychology and its history how in contrast to memory processes, how little attention has been paid to this.

Haith: Right. You know I'm fond of pointing out the irony of the thousand-fold interest in memory as opposed to future oriented processes, because when you ask what is the adaptive function of memory, why we remember anything, one is almost forced to the answer that we remember things to make us more adaptive for future situations, mostly in terms of being prepared ahead of time for what's going to happen. So there's this integral relationship between memory and future orientation that people have almost completely ignored, missing, I think, a real adaptive perspective on the role of memory in our lives.

Emde: Huxley, supposedly on Darwin - have you heard this quote about when he was eulogizing Darwin - said he looked at what everyone had looked at and saw what no one had seen. So it seems to be this is the kind of insight you've had and pursued and elaborated at many different time levels from milliseconds as you pointed out to long term planning, and has enormous practical implications.

Haith: Thank you.

Emde: And, in fact, just to take note of a few things that have impressed me, a lot of people who were studying disorganized family in early childhood education environments, like Dolores Norton in Chicago, as you know, have been impressed that one of the things these children don't have when they get preschool and school age because of the disorganized environments, is a time sense. They don't have a sense of regularity of when things happen, sequencing. Other people are concerned with just not knowing about ordinal numbers, and as you know, intervention programs are being planned around that. And in terms of my experience in early Head Start research, myself and many of us are feeling one of the fundamental things that is important for families, as you've said and indicated, as to whether they're successful in overcoming the multiple stresses associated with poverty and disorganized environments. For learning readiness is whether they have regular routines that build in structure of what comes next, which is done next, ultimately planning. So I think it's an extraordinarily practical applications from your research in science.

Haith: I think we can be very happy if we can contribute to those.

Emde: To cycle back on another aspect of what you said about institutions and university, places, times, contexts and study sections, do you have any reflections about the changes over the span in which you've been involved with them, with respect to frustrations, opportunities? Some of the cohort changes over those thirty-five, forty years with respect to generating research and such matters?

Haith: Well, I think last time we talked quite a lot about the funding situation and how frustrated we have all been with the lack --

Emde: And we are in the midst of talking a little bit reflectively about the changes in study sections, funding opportunities in institutional environments that you've noted in terms of where you've been and your experience over the past forty years. As you reminded us, you had some earlier reflections about this in the earlier part of the interview, but whether there's anything you'd like to add now?

Haith: Okay. It occurs to me that I forgot a couple things that I should mention. One of them was the effect of my sabbaticals, one of them, my first sabbatical was in '78-'79, I worked at the University of Paris with Elaine Vurpillot, and also for a while at the University of Geneva with Andre' Bullinger, and that was really interesting. I think one thing that really came home to me in doing that was exposure to a different culture, a culture in which a publication was not as highly emphasized, where a lot more time was spent on theory, but it always - I never quite got into it because the theory was always so distant from the actual research. I think people in Europe are very skilled at theoretical development, but there's - they kind of leap from the theory to the research, that is very, very different from the U.S.

Emde: Do you think that's changing at all by the way side from that tradition, with which I agree?

Haith: I think it's changing somewhat. I think probably movement in both directions because there's a lot more international collaboration in which you're probably the best example in developmental psychology. I made a lot of contacts with - and for several years had communications with people both in Geneva and France, and certainly I think built on whatever influence I had from the Piagetian tradition. In terms of thinking really about a lot of infant skills as tools for the infant, by which they explore the world, how they explore their own tools, and how they then use those tools, and Andre' Bullinger had a strong influence in thinking along those directions.

And then my second sabbatical was at the Center for Advanced Studies, and I spent a good deal of time there working on the textbook that I finally wrote with Ross Vasta and Scott Miller, and writing a textbook in developmental psychology is quite an interesting activity. You may think that you have a pretty good sense of the whole field, but it's just amazing how big this field is, and you really get a grasp of that by doing a textbook. That was a really useful enterprise, and along with the exposure to people in economics and anthropology and developmental psychology, especially Barb Rogoff and Dick Aslin, Marty Banks, Klaus VonHofsten, who was there, it was an amazing opportunity for me, and I think really influenced my thinking and led to communications that I never would have had the opportunity to pursue otherwise.

Back to frustrations, I think that for a very long time when funding got tight, we became very conservative in the kinds of research that we were willing to support, so that not only many young people became discouraged and shied away from the field, I think, as I probably mentioned before, one can document that the applications and the quality of credentials in the field declined after those years. But aside from the people, who are, of course, most important, I think that we really channeled the whole research enterprise into a much more conservative focus, and I think the risk-taking, the openness to explore new areas was really discouraged. We may have given lip service to the importance of people doing that kind of work but, in fact, the funding picture told a very different story.

Emde: Well it penalizes people for taking risk.

Haith: It sure did. Now I think things are opening up somewhat. I am concerned about the direction that educational institutions are taking somewhat. There really is an increasing emphasis on the bottom line, and bottom line means increasing amounts of time spent in the classroom, less emphasis on research and scholarship. Although people are trying to paper that over by saying, "Oh there's a

synergy between teaching and research,” which I believe at some level, it’s getting to the point where the committee work, the teaching and all the rest, leaves very little time for contemplative thought and scholarship in increasing number of institutions. If you just read through the various publications in higher education, which I had to do, I looked through about fifteen years of papers in an attempt I made to bring someone to the University of Denver for a day-long symposium that we had on research and scholarship in the university, and I did that in my capacity as Director of Research here, which I’ll mention in a minute. I looked through magazines to find leaders in the country who are serving as apologists for research and scholarship in institutions of higher education. And in looking over these articles, oh, they were - drop the names of them, major publications in education. I was so struck about the number of articles that demonized scholars and researchers, and really threw the research enterprise into a very defensive position in my view. I could find virtually no articles of the kind that you used to see that glorified the scholarship and research in higher education and how important it was. I finally got a wonderful speaker to come, but it struck me, the assaults upon teaching and research and the stereotyping of the researcher as being completely removed from students, and students being penalized by the time that researchers and scholars spent on their scholarly work.

Emde: Well, what are your reflections as to how that’s come about?

Haith: I think there’s been increasing pressure by legislatures who are trying to justify expenditures on higher education to their voters. I think that universities have increasingly seen budgets tightening, and certainly schools that are tuition dependent feel as though that they have to appeal to students more and more and, you know, have created to some extent an antagonism between scholarship and teaching.

Emde: Is there a communication gap from researchers to -

Haith: I think that’s true also. I think there was a kind of effete intellectualism, as Spiro Agnew used to put it.

Emde: Have researchers failed to communicate with the public and the consumers?

Haith: I think so, but the problem is that the communication that we have been encouraged to develop has been with respect to funding of research, to show ourselves as being more and more relevant to society’s problems. This doesn’t address the issue of education; it addresses why Congress should give us more funding and why we should appeal to the populous out there so that that can happen. But there’s a different issue and that is, how relevant is this activity in higher education to the missions of educational institutions? And so you have to ask yourself, “Is this relevant to the education of the people who come through the universities?” And we have not been encouraged that much to communicate that message, and I feel that the enterprise is very much under attack.

Emde: Well, it would be fascinating to explore that some more, but I also want to explore some other themes that you brought up and mentioned earlier, and I’d like to frame it in the following way. You’ve talked about your teaching, especially of graduate students and post-doctoral fellows, but also, I know that you’ve been extraordinarily influential in mentoring a variety of young investigators and teachers and scholars, and I’m particularly knowledgeable about that here at the University of Denver. And so I wonder if you would reflect on some of them that you feel especially good about, and if through modesty or other reasons you don’t mention them, I will, and their accomplishments that you really fostered and set them about. And also then reflect on your role as a research administrator, because you have been a Vice-Chancellor for Research here, you were starting to mention that.

Haith: Right. I served as -- actually the title was formally Director of Research at the University of Denver for seven years, and in that role I did a lot of things that I think reflect your influence and reflect the influence of a lot of people who have tried to make resources available to young people, and to investigators who are trying to change directions in their work. So at the University of Denver I

set up several funds, internal funds that would encourage people and make it possible for them to get new projects going, or to get funding, develop external grant applications, to develop communications between people within the university among one another, to connect people who had like interests but didn't know about one another. And this has stretched all the way beyond the sciences into arts and humanities where, you know, I directed a lot of funds to people in music, languages and literature, English and the like, through internal grant committees that I set up. So people in the university learned how to review applications by others, standards were developed.

Emde: Was this for empirical work in the humanities?

Haith: Not only empirical work, but artistic work; computer approaches to music, support of people recording CDs, doing art exhibits, traveling to London in languages and literature to have access to archives that aren't available in the University, really support of scholarly work broadly conceived. And I think that I encouraged people that our university cares about those kinds of activities, and I tried to help people develop their careers. And you know, it's a model that you have pursued and that we all have tried to develop, to play our role in passing along the opportunities that we've had and that people made available for us.

In terms of more narrowly, the dissertations, I've supervised the post-docs, I've mentored in research scientists - actually I'm going to forget to mention some people, but, I mean Bruce Pennington was a perfect example, I mentored his Research Scientist Development Award. And what did I know about dyslexia, I mean, it's a joke, and here I was mentoring him, and he was teaching me about behavior genetics and dyslexia, and in fact, we had some publications together on dyslexia and behavior genetics. Bruce has developed into one of the real stars in the field, and I feel tremendously gratified, that I feel almost guilty claiming any credit because it sort of one of those things where you are just lucky to be there when a person passes by. He's just a tremendous influence on me, and has taken me into new areas.

Emde: But you saw his talent early and made it possible for him to actually switch schools, fields, a whole bunch of stuff. You did a lot.

Haith: Right. Well, you know, if I played any role I encouraged him at a time when he needed encouragement. It was a kind of general role to play, but clearly here was an outstandingly intelligent person, and he's gone on to win the University Lecture Award at the University of Denver and just this last year the John Evans Chair Award.

Emde: And he was a citation scholar.

Haith: Right, one of the top twenty-five in the country in terms of citation or publication impact, so he's been a wonderful story. Gosh, who do I mention? Joe Campos and I collaborated on many wonderful enterprises. He was really a colleague, but he was a Junior Professor when I came into the University and to head the program in Developmental Psychology.

And then there's been a slew of students who have made important contributions, Dick Kearsley, who made important applied contributions in the field; Morton Mendleson at McGill University; Fred Morrison, who was first at Dartmouth and is now at Loyola; Debbie Holmes at Loyola, those two took me into the field of information processing with older kids in memory. Ann Fernald was a post-doc who worked with me and is now at Stanford; Bennett Berteauthal, I worked closely with in collaboration with Joe Campos, has certainly gone onto a distinguished career at National Science Foundation; Louise Hainline, a significant researcher in early visual perception at Brooklyn College. Gail Goodman has also had a wonderful career. And I should have my list in front of me, but -

Emde: And then there were some people that you brought here and mixed together at the beginning of their careers when you were area head of development psychology. So tell me a little bit about Brian MacWhinney?

Haith: Well, Brian MacWhinney, we got on the front end of his career. He was clearly a fantastic intellect, but was so immersed in his own area, that it took, I mean we all knew how brilliant he was, it took a while for us to be able to connect with him and understand what he was talking about. And many of us worked with him, I did quite a lot to help him develop his grant applications and become - I remember he was turned down several times by NIH and NSF, and he was ready to go pump gas at a local filling - he just was ready to give up the whole field. And we encouraged him and tried to help him communicate better, and all of the sudden he simultaneously, he hit at NSF and NIH, and he's been unstoppable since. He's made tremendous contributions.

Emde: And one of the founders of the child language data exchange system, which sort of revolutionized the child language field.

Haith: It made a tremendous contribution. I was fortunate to be able to recruit - she wasn't at the beginning of her career, but to be able to recruit Susan Harter to the University, and she's just been a tremendous asset for us, really one of the stars in social and emotional development, and a wonderful colleague and a wonder area head. Norm Watt, I was able to recruit from University of Massachusetts. Bruce Pennington, I played a fairly large role, I believe, in bringing him here.

Emde: Kurt Fisher?

Haith: Kurt Fisher came with me when I came from Harvard, and I -

Emde: You fostered his early career in the mix again.

Haith: Right. He's had a wonderful career. Janette Benson and Rob Roberts were post-docs with me and have become faculty at the University. Naomi Wentworth has had a good career at Lake Forest. So, you know, I've been quite fortunate. I've had people, who have been very loyal to me and wonderful colleagues, and at the same time I really felt good about their career development and whatever role I was able to play.

Emde: Good. I wondered if you might reflect a little bit about your experiences with the Society for Research on Child Development. When did you join SRCD?

Haith: I think I joined SRCD in 1964 as a graduate student, and then when I went to Harvard in 1966, it seems to me that Jerry Kagan asked me to help him run the SRCD Conference, which was at the Hotel Miramar in, I believe it was Los Angeles, in 196 - are we on odd years at SRCD? I believe I was 1967. And that was really exciting to do that with Jerry. I can't remember why we would have been responsible for--

Emde: You ran the program or something?

Haith: Yes, running the session at SRCD. But I remember that Jerry and I had VIP rooms on the top floor of the Hotel Miramar, and SRCD was small enough at that time that Jerry and I invited the whole of SRCD to our rooms for a cocktail party, and we had a really wild party. I remember Hans Furth who came to the party, and he was out in the hallway talking loudly and we got a call, you know it must have been two o'clock in the morning, from the desk asking us to quiet it down, we were having this wild party. But that was my early days with -

Emde: That was your first meeting with it, the first biennial meeting?

Haith: No. Actually, I think I became, as I remember, probably a member of SRCD as early as 1962.

Emde: Did you go to the Berkley meeting?

Haith: I believe I did, because I was a graduate student at the time, and I joined SRCD when I was a graduate student. I've shied away from participation in the public offices at SRCD. I don't know why, I mean I've gone to all the conferences. I feel I've been a good citizen. I served on the, I guess the APA G.S. Hall Award Committee, I served on the Distinguished Scientific Award Committee and various committees, but I just haven't really wanted to take the leadership role in a lot of society activities. But I guess my most significant role in the association was serving under you as the Associate Editor for the *SRCD Monographs*, and that was an enjoyable experience, and I didn't actually do that much. I certainly admired your dedication.

Emde: You did more than you thought as usual in your career.

Haith: I don't know if - you worked very hard on that little venture.

Emde: I remember you had some important roles when SRCD was in Denver at its biannual meeting, including a huge party at your house.

Haith: Yes. And that party was really amazing because it was April, and we have a house that has a huge backyard, but it's a very small house, and we had a very big deck, so we invited virtually everyone we knew. It was over two hundred people and one hundred and fifty people said they were coming. And, oh god, it started snowing the night before and we were just out of our minds. And I'll never forget, we had one of my former students staying at the house and I said, "My God we've got to figure out if we can get a hundred and fifty people in this house," because it was the first large party that we ever had. And so, we would stand next to one another and we would measure how much space each of us took, and then we would calculate how big the rooms were and whether you could physically get a hundred and fifty people in that room and, as a consequence of those measurements we were moving furniture into the basement and - as it turned out it was one of those parties where everybody was squashed against everybody else, and that's the greatest kind of party you can have because, you know, when people are too separated and conversation becomes forced, but when they're squashed together and they can hardly move they have a great time. It turned out okay, but I remember, this is funny, Nathan Fox called, he was a student then and he said, "I heard there's a party at your house, and I wasn't invited, but I heard, I think it was from Jerry Kagan that there was a party and I was wondering if I could come?" And so I held the phone and I said to Sue, "Can we accommodate one more person," and she was so freaked out she said, "We're not having one more person come to this house," and I said to Nathan, "This just isn't like me, but we're freaking out that we're just going to burst the seams of this house, and I'm afraid - he wanted to bring a few other people with him. I said, "I'm afraid I'm just going to have to say no." And I have felt bad about that, and Nathan and I laugh about that now still. I didn't know him, it was our first interaction, and I throughout my career felt terrible because I've never turned anybody down to come for a party, but we were really paralyzed for that one.

Emde: Well, I remember there were one or two buses, Fifth Avenue type buses from Larimer Square in Denver that we hired to get people to your party.

Haith: That was one of the funniest things that's ever happened to us. The snow was just horrible; there was just no way anybody could go outside.

Emde: Well the historians will note there were several events of history for that meeting, including when the whole thing almost collapsed in John Congers' interview when they tried to take away the hotel from us, and his machinations about that, but it was quite a meeting. Anything else about that first biannual meeting that you remember?

Haith: Oh, gosh no. I remember that, you know, I was busy buying liquor and getting ice cubes and everything else for this - oh, you mean our - at Denver, or the first --

Emde: No. There's a question of interest here about memories of your first biennial meeting attended, so you've sort of covered that.

Haith: You know the other meeting I remember that I didn't mention is the - and you were participating in this too, was the Committee on Research in Infancy that Jerry Kagan and Bill Kessen and Jerry Bruner started, Lou Lipsitt, it consisted of maybe twenty people; a group that was later seemed to be quite elitist, but at the time, you know, they were just trying to get the people together in infancy, there were about twenty of them. Oh, Yvonne Brackbill and Jack Gewirtz and Frances Graham and Rita Eisenberg, and I'm missing some.

Emde: Hanus Papousek.

Haith: Hanus Papousek, and I think Mike Lewis --

Emde: Arnie Sameroff.

Haith: Yeah, Arnie Sameroff. I think we missed the first meeting, but we came to the second and that was very exciting. Phil Salapatek and Arnie Sameroff and I came to that second meeting. I don't remember when you -

Emde: Probably the third one.

Haith: That was fun, it was like a fraternity, we'd sit around and vote on who the people were that we thought we should bring in, and people exchanged papers with one another, and that was very exciting. It met for several years and then a group popped up that was very offended by this elitist group that was meeting and felt that, you know, this should be open to everyone, and they decided to start the -

Emde: International Conference of Infant Study, ICIS.

Haith: That's right. And actually, our group encouraged that, we were excited about it, and for a few years - and they started to meet, they've become more than a thousand person -

Emde: The first meeting we joined them in Providence because Lou Lipsitt was hosting.

Haith: Right, Lou Lipsitt, and so we tried to be very helpful and encouraging. But at the same time there was some nostalgia for the old, smaller, more manageable group, and we met separately for maybe two or three meetings after that, and finally we just gave up and became part of the whole group because we felt there was continuing resentment. But I felt those efforts gave rise to that group, which has been very successful and has become a very, very large inter-disciplinary group, and it was kind of fun to see how all that evolved.

Emde: Well, one of the ideas, as I recall, of that group was - and this is what cooked it eventually, the idea being that you shouldn't have more people in a group than could sit around a long conference table, face each other and have discussions.

Haith: Right.

Emde: And so that put a limiting factor on it, and as the field mushroomed then a lot of the younger people got left out and they formed their own organization in their topical subareas because everybody was generative of the field and of the young people, so it was an interesting process. What do you think are - or do you have any reflections on important changes that have occurred in the organization of SRCD and its activities since you've been a part of it?

Haith: I think that deliberate attempts were made to bring in people in education, pediatrics, expand SRCD beyond what seemed to be a kind of central core of developmental psychologists. Efforts were made to internationalize the organization, and I think those have been very beneficial. I think SRCD is the one meeting that the people in Europe don't miss, and they come at great sacrifice, many of them don't have their expenses covered by their university, and the travel is a drain, it's a major drain to come over. But the organization has really facilitated the communications between people in the U.S. and Europe. I think it's really made it much easier for people outside the country to publish in our journals and not to feel so intimidated by the language and the incredible resources that we have in this country, the support of developmental work.

I mean that was one thing I didn't mention that left a strong impression for me when I did my sabbatical in Europe. God, you know, we probably have more developmental psychologists in Denver than any of the countries in Europe all together. It may be a little bit of an overstatement, but it's not huge. At least at the time when I went in '78-'79, that was close to true, just tremendous numbers of people, tremendous resources, our inter-disciplinary communications among pediatricians, educators, developmental psychologists, seemed at the time much more advanced than communications among those different disciplines in other countries. But I think that we've really helped those other countries - the organization has helped those countries become more interdisciplinary within their own countries and certainly has fostered the communications between the U.S. and these countries.

Emde: Are there any down sides to all of that?

Haith: Well, the organization's gotten so large that it seems to me that the inevitable happens, it gets segmented off and into subgroups, so that you go to the meetings and there's the perceptual stuff going on, there's the social development stuff going on, the more clinically oriented, the cognitive, and, you know, as you get bigger it's just harder to keep track of everything. It seemed to me that when the organization was smaller, people probably experienced more of what one another had to say in these various disciplines than now.

Emde: Do you think they're going to be thirty-nine or forty divisions of the SRCD?

Haith: I don't know, but you know, there is an issue of numbers, and you can only manage so many people. I quit going to APA meetings more than two decades ago, because I just couldn't get my arms around the meeting. I just felt completely overwhelmed.

Emde: We have a similar thing in psychiatry. Well, what about your hopes and fears for the future of the field is a question in here too?

Haith: Oh, I don't know. My hopes are that we can appreciate that a child is a whole system, it isn't broken into neuropsychological development, social development, cognitive development, whatever, these are our methodologies that we impose, but we need to appreciate that this is a whole system. And I would hope that certainly we make advances in developmental psychobiology, through, you know, very exciting imaging techniques and ways to inform us about what the brain is doing, but at the same time, that we don't go off in a reductionist blind alley where that kind of work is more highly valued than more behavioral work. And the hope is really that we continue to regard this as a whole system and develop a vocabulary and a technology that permits people to talk to one another and understand that they're talking about things at different levels, but that the parallels are there, and they're really, as I say, talking about one system.

My fears are that developmental psychology's going to go away. It seems to me that you've got two things going on. One is psychology is sort of going away in this decade of the brain, I mean, and people in artificial intelligence, people in computer science, everyone is excited about what the brain is doing and it's no longer the exclusive domain of psychologists, so we're kind of losing our identity. It seems to me that this is also happening in development; we had these grand theories that Piaget and Freud and others and development psychology had its own identity and integrity, and it seems to me that

now people who were in visual science want to look at the development of vision, but know very little about developmental psychology, and this is going on in cognition, it's going on in perception, it's going on in social psychology, it's going on in clinical psychology, and so you wonder as Kessen has said - what was that paper called, "The death of developmental psychology," isn't the real prophecy. And in fact, that our field is going to evaporate because everyone else subsumes it under what their interest is, so that's kind of a fear. I don't know what it's going to look like ten years from now, I don't know if we're going to be a separate domain, I mean, everybody's organizing the field in terms of content rather in terms of age, and we're really defined in terms of age.

Emde: Let me try a comment for your reflection, for your comment to be an invocative comment, and also a question that follows it. It seems to me that much of what you've told us about your career and the relation of yourself and your work to students and others, speaks against that with every -- and that you have really exemplified integration across multiple systems. This is for your comment that you've talked about integration across vertical systems, you know, from cell through social systems of the person. You've talked about integration that you've been continually concerned with, even though focused on particular features at different times. You've also been concerned about and involved in, immersed in horizontal integrations across disciplines. You've also been involved and have taught about cross-method integrations and sort of, a little bit surprisingly to you as it came out in this interview, you've been involved in integrations it seems to me between knowledge and action of the applied area, it's been exemplified in your students and your mentoring, but perhaps even more or as equally dramatically, you've been involved in another kind of integration, which is at the core of developmental work, which has to do with a deep interest and continual awareness and teaching about developmental processes. Developmental processes that are not simply at one point in development, so you've been focused on the newborn for a lot of your work, but you've worked throughout the lifespan and been able to teach and talk about that, and worry about it as well. So that, it seems to me, much of what you've told us exemplifies those integrated things across the system and your fear that that might be lost sight of, but it is at the core of developmentalists, and not just developmental psychology but the development sciences and the developmental disciplines. And I guess the question is, is to whether you were also not speaking about another kind of concern, not just keeping track of the whole system in that integrative sense, but imploring us to put it all in terms of personal meaning. Would you like to comment on that? I mean that's obviously --

Haith: Well, you are right, if there is a hope for developmental psychology, I think what we do uniquely in opposition to all these different areas that are focused on the particular field, like vision or - or language or whatever, and as I mentioned their interest in the developmental process for those particular domains. One thing we do well is to integrate across domains; cross-cultural research is to a large extent unique to development psychology. Cross-process research, the unification of perception, cognition, biology of the brain, the social affective, you don't see this kind of bringing together of areas in most other fields, it includes pediatrics education. There are a lot of cross currents in developmental psychology, and you're right, this is something we do well, and if there's hope for us, it's that we can not only be interested in the vertical aspects of development, but also the horizontal aspects, and these aren't independent of one another, I mean they affect one another and they interact. So we may lose our identity to some extent with regard to vertical processes, but I think there probably is more hope in terms of the more horizontal cut and the interaction between the vertical and horizontal cut.

Emde: And - the last part of our discussion has to do with something about your personal interest, your family, the way they may have had a bearing on your scientific interest and contributions of wide contributions. And you've spoken a bit more in your earlier biographical account of your family origin, but not so much about your current family or anything else that you would like to reflect about in that area.

Haith: Well, I think we've pretty well covered it. My family was very supportive of education in its own right, even though they didn't have an education to speak of themselves. And in the traditional

Jewish family, any education, any advancement was without qualification, supported and encouraged. I really came to the field through my interest in kids. I've just always been fascinated by development, and even I mentioned I started out in clinical development; it was kids I was interested in. And as I mentioned, I followed my nose. I didn't have any super-ordinate philosophical, theoretical systematic view of the world that led me into what I did. I would see something interesting and I would follow it. And as I mentioned, my kids were an important influence on my research career. We had our first child at I guess - I feel a strange - putting myself in any sort of contrast with Piaget, but at least in this respect, I got interested in my kids, and they have really led me along. And in the textbook that I wrote I got them in there as much as I could, pictures of them and everything else because they really had been influential in my career. And I guess the only other thing to say is that I have a very tolerant wife, who has been willing to go across the country wherever the opportunities led, and to put up with a lot of solitude as I spent my time writing and reading and stressing and grant reviewing and conferencing and all the rest. I think many of us have our wives to thank for their encouragement and tolerance.

Emde: Well, Marshall, are there any other reflections that you want to add as we conclude this?

Haith: I think I've said this before, but I feel it's all been icing on the cake after college. It's just been a wonderful career. And I've been really sincere when I've said, as you've noted, that's its remarkable that people would pay us to do what we've done, and to entertain ourselves while hopefully we've made some kind of lasting contribution to the field.

Emde: But the way I remember it from what you have said involves curiosity rather than entertainment, and it certainly is the case that I would take a little issue with that word entertainment, which implies something that is more egocentric than the widening view of curiosity, surprise and a widening world for yourself and others.

Haith: Sure. But it doesn't hurt to enjoy it too.

Emde: No, unabashedly, and that's a wonderful feature. Well, it's been a pleasure for me to do this interview and I hope that it's a pleasure for those in the future who will read it.

Haith: Thank you.

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