Robert Emde

- Born 4/29/1935 in Orange, New Jersey
- Spouse – Joyce Emde
- A.B. from Dartmouth College (1956), M.D. from Columbia University College of Physicians and Surgeons

Major Employment:
- University of Colorado School of Medicine – 1988-2005, Director, Program for Early Developmental Studies
- University of Colorado School of Medicine – 2005-Present, Professor Emeritus

Major Areas of Work:
- Early socio-emotional development
- Infant mental health
- Early preventive interventions

SRCD Affiliation:
- 1974-1982, Committee on Interdisciplinary Affairs
- 1977-1982, Publications Committee
- 1989-1991, President-Elect and Governing Council
- 1991-1993, President
- 1993-1995, Past-President; Chair of Nominations and Awards Committees

SRCD ORAL HISTORY INTERVIEW

Dr. Robert N. Emde
University of Colorado Medical Center

Interviewed by Marshall Haith
At the University of Colorado Medical Center
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Haith: We're beginning an interview about his intellectual history and his experiences with SRCD as part of the series of interviews with presidents of SRCD. I've known Bob for, 25 or 30 years now. I've been at the University of Denver as a Professor, and Dr. Emde's been at the University of Colorado Medical Center, had many opportunities for collaboration and friendship, and served together on several committees. And so I'm now interviewing him about his experiences with SRCD. And we'll begin now with my asking, "Bob, when did you join SRCD, and what were your earliest contacts with the Society? Who do you remember? What was the first biennial meeting you went to?"

Emde: Actually the Institute of Human Development sponsored the first biennial meeting I went to in Berkley. There were a few hundred people or so who went to that meeting. I don't remember the exact number.

Haith: Do you remember the year?

Emde: I think it was 1963. And I'm told that was the last biennial meeting that was on a university campus. I was stimulated to join SRCD when beginning my research observations of infants, where another resident, Paul Polak and myself started observing in a nursery. We found an orphanage nursery in the north end of Denver and started making observations because we wanted to start researching some questions. We wanted to get supervision with René Spitz, which we did. We enticed him to join us even though he was making motions to leave Denver, which he did about a year and a half later to go to Europe for a stint. He was supposedly unavailable to us. But we coxed him to join us when we made a series of observations, called him up and said we had some observations that disproved his theory. Rather than being upset with our arrogance, he was intrigued and guided our observations.

But there were other incentives for observing babies as psychiatric residents. I began doing this in 1961 as a first-year psychiatric resident, and there was active infancy work going on at the University of Denver with leadership of Yvonne Brackbill, who had a strong behavioristic orientation, and also had other broad interests related to new discoveries of research being done in the Soviet Union. She was very encouraging of our operationalizing our concepts as we worked with Spitz. When we met with her she said, "You know you have to get involved in SRCD, this is what's really going to broaden and teach you much more than I can." And she was right. I joined SRCD, we both did as student members then, and I went to that first meeting at UC Berkeley.

Haith: What year did you join the Society?

Emde: I think I joined probably about 1962 as a student psychiatry resident. And I've said, many times, that my graduate education for research was attained through SRCD, the generosity from all of the colleagues, friends, mentors that I sought out and were so readily available in SRCD. Not only at meetings, of course, but following up with all kinds of contacts, including visits, which I eventually was able to do once I had a Research Scientist Development Award from the NIMH, which I applied for and began in 1965 after I finished psychiatric training, and had a brief stint as a community psychiatrist.

Haith: Who were some of the names? You've mentioned there were various people in the Society - some of the names that you remember?

Emde: Oh, their names? I remember my first presentation, at which you're laughing at Marshall, because you know what's likely to come forth here. My first presentation was--I didn't present in Berkeley, and I'm not sure whether it was the next biennial meeting or not-- it was in New York City. It was at the Waldorf Astoria. SRCD in the Waldorf Astoria would you believe! And I presented with, a student colleague of mine. We'd been filming and tracking smiling in infants and we were very interested in rapid eye movement sleep and drowsy stage smiles in the newborn. We classified a number of these, and we were studying their configurations and their psychological concomitants, and we presented a documentary film which showed lots of these newborn smiles. Lew Lipsitt was--I remember very vividly--the Chair of the symposium, and he was very generative and warm, because I was very daunted by this first major presentation at SRCD. When we presented these ideas about REM smiles and our observations, we sort of suspected there'd be skepticism, so we had the film. And we showed the film, and then there was this person who piped up in the audience and said, "How many yards or miles of film, did you take to get these pictures?" So I turned to Lew and I said, "Who is that guy?" "That's Marshall Haith at Harvard." "Oh." Peter Wolf had earlier told me about your very elegant work, when I had visited and he recommended I meet you, describing the elegant way you had been able to view babies' eye movements at the same time as viewing the mother, and video taping the whole thing. So I was a bit daunted but impressed with the question and later was delighted to get to know you. About how long after that did you come to Denver?

Haith: 1972.

Emde: 1972.

Haith: So you mentioned Lew, Peter Wolf, me, Marshall Haith. What other names do you remember from your earliest meeting in Berkeley? You mentioned there were other people in the Society –

Emde: That I learned from, yes.

Haith: -- that really helped your career along.

Emde: I visited Lou Sander of Boston, and Gerald Stechler as well as Peter Wolf there. Although he wasn't one of the first people I visited, I think the relationship with Jerry Kagan began, you know, within a couple of years after I had my Research Scientist Award and was actually working on my research program through meetings and discussions and lively dialogs. And I remember early on going to Michael Lewis' conferences, on emotional and social development in Princeton, which were fun to go to and meet. And early on I remember what was very important in my research training too, was that in those days the NIMH Research Scientist Development Award people took awardees in hand at Level One who had very little research training and who needed training as well as the experience of being guided in programmatic research. And they explicitly provided monies for training as well as a yearly week-long retreat in the mountains with invited mentors. And that was very important. That program dropped the training retreat over the years, which is a very sad thing as I later had mentees who got into that program and didn't have that intensive training experience. And I remember very vividly the first one I went to, which was in 1965. I got in there early, and I roomed with a fellow who was just finishing his five years. And most of the people in the career development type-one awardees at that time were clinicians who didn't have the advantage of graduate training and research methodology through Ph.D programs, and most were M.D.'s or clinician Ph.D.'s who just needed research. And the fellow I'd roomed with was a child psychiatrist, who had also been a mathematician before going into medical school. And so he had decided he was going to do a research program, operationalizing and testing for validity, Piagets' theory of operational intelligence at the advanced stages supposedly characteristic of successful progression in adolescence. So he decided he would work with students in the Bronx High School of Science since he knew it was going to involve formal operations, suspected it would only be demonstrable in a small segment of the population at the highest levels, which he wanted to show. And he operationalized a whole set of assessments using Boolean algebra, and he wasn't able to get one student through it at the Bronx High School of Science. And he was rooming with me and he was saying, "I'm going back to clinical work." It was a somewhat amusing, not very inspiring choice of roommates to be with, but the environment in those times was extremely supportive and diverse in terms of experience.

I remember Jack Cohen came and gave an extraordinary mini course in statistics and introduced us to why one should think in terms of regression instead of ANOVAs and group comparisons. I think the field about ten to fifteen years later came more into that. It was a terrific experience in those early days. I remember Bob Wallerstein was the Chair of the Research Scientist Committee in those days, a clinician who'd come into research later, and was working in the extremely complex circumstances of psychotherapy research. I'm not sure that kind of person would be in that position in today's' world of NIMH, which now seems to be the National Institute of Brain Diseases instead of the National Institute of Mental Health for the most part. [A little editorial comment there.] But there were a number of those kinds of formative experiences. And my relationship with René Spitz was a very important one, which I've written about in the literature, and which I have presented on numerous occasions, a rather whirlwind. Various psychoanalytical groups in particular have honored him on the occasion of the hundredth anniversary of his birth.

Haith: Was he in SRCD or not?

Emde: No. He was not a member of SRCD, but he was a pioneer in introducing research thinking in psychoanalysis and a number of research methods that he brought to bear for psychoanalytic observations including, not only the importance of observing infants and young children and their mothers and parents, but also using filming. He was an adamant believer in filming continuously for documenting and for study. And he did this in the early days when filming was cumbersome, in the late '30s and '40s and he used 35mm film, spending a lot of his own money gained from clinical work in NYC practicing psychoanalysis. He never had a grant. He early on also connected with ethology and was one of the early people bringing ethological methods into developmental observations let alone psychoanalysis. He also brought concepts from embryology, in which he was very interested. His theories made use of early organizer theories, for example, from embryology's [Hans] Spemann and others, and to some extent [Conrad] Waddington. So he was a mentor that, as I went on to become involved in a psychoanalytic career, showed that you could make use of psychodynamics, psychoanalytic thinking, and make contributions to it in an integrative way that was multidisciplinary, multi-perspectivistic, and survive with some excitement doing research, even though he was pretty much reviewed as a maverick in his time by psychoanalysts.

Haith: Just following up with experiences with SRCD, you've talked about some of this, but moving on to the second question here is, "With regards to the history of your participation in scientific meetings and publications of the Society and your work with the Society, not the governance aspects, but other aspects of work with the Society?"

Emde: Very early I was encouraged to become involved in committee work, particularly as Dick Bell formed the Interdisciplinary Affairs Committee. He was an early person I visited, by the way, when he was at the intramural program at NIMH. I also visited Marian Radke-Yarrow and Lee Yarrow over at NICHD. And Dick, when he formed this SRCD committee, asked me to be a part of it. It was first a dinner meeting and then became a regular committee in which he felt it was important to assemble and encourage people from other disciplines outside of traditional aspects of psychology to get together, because he was already seeing that there was a lesser proportion of members outside of psychology in SRCD than had been the case in its earlier days. So this was already an influence in the mid 1960's.

Haith: Memberships were falling off.

Emde: Proportionately. So the main reason being, I think was the success of SRCD in attracting developmental psychologists and the success of the field, and perhaps people being discouraged with the size of the psychological APA, or the American Psychological Association. Since I belong to two other APA's I have to clarify that. And so it was a proportionate thing, but I think whether there was an absolute drop-off in interdisciplinary people, I don't know at that time, but Dick was concerned with the balance, and that the people would indeed be crowded out from other disciplines as a consequence. He wanted in a very positive way to encourage other disciplines. So he started the dinner, the convocation kind of meeting at the biennial meetings, and then the committee was formed. And I was on that first committee. I served for two or three rotations of that committee with Lou Sander, then was Chair after Dick Bell, and Bill Hall was Chair, and then I rotated off the committee. And I became involved and energized working more and more with colleagues of many disciplines in SRCD.

Also very early on, when Frances Graham was President of SRCD, still in those early years, she asked me to take on a task that had to do with some information about drug studies, which was not my area of expertise, but it was so far from hers that she appealed to me to be head of a task group and assemble people to get information about this, which I did. When we got the job done she was thrilled with the result and that she had found somebody who would work outside of developmental psychology for the organization with a task group. And I think that the word then got around that at least I could get things done. So I was periodically involved in different things with the organization and frequently would be called upon for advice or for a little task to get done. Then I was on the Publications Committee and I'm not sure whether I had a whole rotation before they asked me if I would be Editor of *Child Development*, actually fairly early in my career.

Haith: Okay, go ahead. Do you want to follow up on that?

Emde: Yes. The time was when Wendell Jeffrey was finishing his editorship. I believe that's right. And I sense that they were going out on a limb in picking me, as a young psychiatrist, and they were most flattering. I would have been perhaps the first Editor of *Child Development* outside of psychology, which was not only flattering, it was challenging, and I was excited by that. I had a lot of ideas. But I decided at that point in my research career it would have taken me too much off track in terms of what I needed to learn as well as what I needed to do in terms of my research program. And I think that was probably a good decision then, because it just would have been too much time and involvement in editing *Child Development*. And even in retrospect it was at a period of expansion of submissions, which actually I remember in Wendell Jeffrey's editorship hearing that he went from doing it all himself to getting help. The previous editor didn't have any associate editors, she did it all herself.

Haith: Alberta Siegel?

Emde: Yes, Alberta. So the journal was becoming more and more work. I think it was just entering that phase where you needed associate editors. So I stayed on this Publications Committee, and then eventually after that, they came back again and asked if I would be Editor of *Monographs*. And at that time I felt a little more secure and consolidated in my research program. And I also felt that I could probably do the job of *Monographs* Editor coming from training outside of psychology; it was a more manageable job. And indeed that turned out to be the case.

Before I decided to take on *Monographs*, though, I interviewed previous editors. Most influential for me was Fran Horowitz, who was just moving from the editorship and she said she absolutely loved it. She was un-ambivalent. The previous editors of *Child Development* were not that un-ambivalent because of the hassles in addition to the satisfactions of the job. Fran said, "You get to--and this is what I found to be the case--really get into an author’s thinking in a monograph reading," and also you have the advantage of choosing your expert reviewers. And the tradition was not to choose young reviewers or inexperienced reviewers because of the time investment, both for the authors and for the reviewers. Instead you get to choose experts in the field. And you got to go to school with them, and learn from them, which I found to be the case. It was an absolutely wonderful job. I loved it during my editorship that I began when I came back from a years' sabbatical in London in 1981. In addition to being a sheer pleasure, it extended and deepened my sense of colleagueship within the organization, because people were available to do reviews, and most did a fine mentoring job of giving positive suggestions for the authors. And they also mentored me, and I just found it thoroughly enjoyable.

I tried a number of things--because I think this was another thing that history effort is interested in--a number of things that didn't work during my editorship. I had an idea, in order to encourage more submissions from other disciplines outside of mainstream developmental psychology, to help us stimulate a whole range of things, including animal behavior, lifespan psychology, teratology, and more clinically related contributions. So I set up on a group of editorial associates, I think we called them, that were headliners designated to represent certain fields for which we wanted to encourage submissions. The idea was to advertise and in-gather, contributions from under-represented disciplines, and you were one of those people. It was a failure. It didn't result in more recruiting of additional contributions as we had hoped. Where we got multidisciplinary contributions, was, you know, by beating the bushes in all kinds of directions and mainly from what was out there to come in. *Monographs* at the time, and certainly since, does have a reputation of providing constructive reviews and working with authors to make good results happen.

We did institute under my editorship, I think something that has continued, which was a very explicit quick turnaround for submissions that were obviously not going to make it in the editors' judgment. I took responsibility, during my editorship, to turn around within a week or two, about one-third of the submissions myself without further review. Many of these were dissertations, inappropriate for *Monographs*, that Fran had warned me about, and she'd started this policy. However I widened the scope to things that I thought were just not going to make it. They weren't worth the authors' and the reviewers' time, for a variety of reasons that I fed back to authors and to the Pub Committee. So a thirty to forty percent under my editorship went back in that way.

Editing *Monograhps* was a wonderful experience, and the publication has a very strong place in my heart. Not only was it a job where you felt you were doing some service and could have some creative input too, and encourage people, but it was rewarding in terms of all that I learned, and I learned an enormous amount.

Haith: You mentioned the Berkeley and the New York meetings. Are there any other of the SRCD's that kind of stand out for you in your mind or with your publication activities?

Emde: Let me comment on the meetings. The heart of the Society is in the biennial meetings. And the liveliness, I became aware, and the expansion in the interdisciplinary nature, and the diversity of the organization was reflected in the biennial meetings, and grew. And that was not the case in our publications, and especially in *Child Development*, a matter that became of increasing concern to me as I moved into the executive roles as President-Elect, President and Past President, which I'll comment on later, because that became one of the things I wanted to address.

Haith: But you felt it at the meetings?

Emde: Yes, at the meetings what I felt was more and more in contradistinction to the journal, that, not just from my point of view, but from what I would hear from my colleagues—particularly from clinicians and systems-oriented developmentalists, and people in pediatrics, sociology and clinical psychology. There was concern about the perfectionism involved in the review process, the lack of readability of articles, and the growing the lack of incentives to submit to *Child Development*, narrowing the scope of the child development. This, as I said, was in contrast to the meetings, which were very exciting, and providing lively opportunities for discussions which were far-reaching. So that was the contrast I became more and more aware of.

Haith: And in-- So in general, your own publications in SRCD journals—does anything stand out for you in particular?

Emde: The biggest problem was recruiting more submissions during my editorship. At the beginning of Fran Horowitz's editorship, there were 30 submissions or so per year but that fell off so that towards the end of her term it was less than half of that.

Haith: A year?

Emde: Yeah, a year. The submissions went down during her editorship. And this trend continued such that during my editorship, there were about a dozen a year, or ten a year.

Haith: Wasn't this a period though when small books were coming to the floor?

Emde: It was a period that we had competition with books. And perhaps that was the main thing. It was also a period when people explicitly felt for career development they had to have numbers of shorter publications. To some extent this influence continues now, but I think it may have been even more then, a counting of number of publications for getting tenure and promotions—and we should be concerned about that. And there were some, I think, false beliefs about making money through commercial books. We did counter a number of that through some newsletter communications—the values and virtues of an archival-based monograph publication—but it didn't seem to make a noticeable difference. I don't think the rate has changed substantially since then, although there've probably been some blips where there'd been some surges of more coming in. And that's okay. So that was a concern to us, as Governing Council, Publications Committee and myself during my editorship. The unevenness, of publications is of course also a problem. You may have a larger number in one year and a smaller number the next year. And I must say the University of Chicago Press and the Governing Council were very flexible and good about that, so we worked that out. So some years you might have five, other years you might have three or two. And some years you might have a double number with a longer monograph.

My most creative thing as Editor of *Monographs*, which I'm proud of, was my idea to put together the Growing Points in Attachment monograph. The idea emerged when I went to two different meeting symposia. Actually it was in the course of an ICIS meeting as it turned out, not an SRCD meeting. It was an off-year meeting of the infancy group, but involving SRCD people. These were two different symposia workshops on novel aspects of attachment that were not in the mainstream of that emerging field. One had to do with cross-generational studies, and the other had to do with cross-cultural studies. It seemed that findings from presentations of research in these separate symposia were questioning major tenets of attachment theory as it had been developing in a sort of narrow-way, from my point of view up to that time. The latter had focused on the Strange Situation method of assessment and arguments about the purity of ABC and D attachment classifications, and whether you had to have two separations, and who was certified to do this and who not, etc. I could see that leading nowhere, and the attachment field seemed diverted into a cul de sac. These two symposia were very exciting because the old classifications didn't work in the same ways; they were very much context bound with variations according to culture and ecology. Additionally, one symposium introduced cross-generational work and “the move to representation” as Mary Main put it. I thought this would really be exciting to put these two things together into an SRCD *Monograph*. So I sought out Inge Bretherton and persuaded her to take this on, and then she got Everett Waters as a co-editor. They did a magnificent job. And for a while it was the largest selling SRCD *Monograph*, stimulating a huge amount of cross-generational research as well as cross-cultural research and bringing in new methods.

Oh, we also instituted during my editorship, come to think of it, a sales promotion of separate volumes outside of the membership volumes, and under separate covers. And that took a while to get going, but several volumes sold quite well that way. In addition to *Growing Points of Attachment*, a *Monograph* of Harold Stevenson, sold well that way too.

Haith: We wanted to talk about issues and problems. Just quickly, you mention that you really felt the reviewers were very helpful. Did you have problems getting reviewers cooperation or was that reasonably easy?

Emde: I found it not a big problem. First of all I'd get lines on reviewers. I'd ask around and network with colleagues to do this. It's like getting people on committees, you want people that will do work, you just don't want names. So I'd find out who were good reviewers, and people know that, and actually the editors of *Child Development* and *Developmental Psychology* were great resources for that, and then my own networking through the Interdisciplinary Affairs Committee and just other colleagues or people outside of psychology. So I'd come into the reviewer selection process knowing the reputations of who were good reviewers. And those people would of course be able to say, "No." It was a minority who would say, "No," but I would appreciate that. The only sticky times were over summers, which you would predict. That's true for all reviewers I think. So over summer instead of people devoting more time for that kind of thing, it ended up taking longer. But no, I had terrific relations with the reviewing process and the reviewers. It says a lot about our field because people are dedicated and spend a lot of time doing this.

Haith: Did you hold other offices on Governing Council other than as President?

Emde: No. I ran once as a non-psychologist early on for Governing Council, and was defeated.

Haith: Well you won out in the end.

Emde: With affirmative action, when the organization decided to have elections for President with a slate in some years containing only non-psychologists.

Haith: How about talking about some of the major issues and problems that you faced as President?

Emde: Okay. Leading up to it I can give one other anecdote, which demonstrates some of the perfectionism of the *Child Development* review process. This is before I was President, but it just burned itself in my memory. Mary Ainsworth gave an SRCD presidential address, which I thought was a terrific review of her own work and looked forward to speculations from her point of view about where attachment research was going. Her Presidential address at that time was then sent out for review by *Child Development*. There's now a policy like with other organizations, that Presidential addresses are typically published, often with a friendly review by the editor. However at that time that policy wasn't in effect in SRCD so Mary Ainsworth wrote up her Presidential Address, and did a review with her perspectives for publication in *Child Development*. It was sent out for review, and I know it first-hand because I was one of the reviewers. I made some constructive, very minor suggestions that were advisory; I thought it was a terrific piece and thought it should be published. Furthermore, I asked, why is this going out for review? Why isn't this just published after the editor provides any needed minor suggestions? Well, the historian will know looking it up, that the editor at the time was Mavis Hetherington who was from Mary's own institution, which was curious. Maybe that was the reason why she wanted to send it out for other broad reviews. Still, there was at least one other reviewer, maybe two other reviewers, who were questioning and negative, so it was rejected. It was rejected! I was horrified, and I related my comment to Mavis, whom I have valued as a friend. This illustrates, I think, some of the problems that perfectionism that *Child Development* was getting into, and that was one of the things I wanted to counter, when I became President.

The new editor had been selected, Sue Summerville, and prior to when I was President-Elect, actually also prior to when I was on the Governing Council's Executive Committee. With the Executive Committee, the idea for an Executive Committee consisting of the executive secretary and the three presidents; that is, elect, serving and past was instituted at the beginning of my executive term. I was one of the people that strongly suggested this, just to make interim decisions. But just prior to that, just in the rotations of the way things happen, the new editor was selected. I did not approve of the process for the selection as I heard about it in retrospect.

The process wasn't one that was informative, I don't think, to the candidates, including the one selected. It did not discuss the issues that were accumulating about *Child Development,* which were already in the air about perfectionism and the problems of needed interdisciplinary contributions and readership and a lot of issues around it.

So when I became President-Elect, immediately there was a new editor. Yeah, I was saying that so when I became President-elect there were many issues that I wanted to address. And one of them had to do with the perceived perfectionism of *Child Development*.

Haith: And you mention that you had no influence over these selections?

Emde: Yeah. So the question was, "How to have a positive influence and constructive influence?" And what I did was try and gather a lot of information. During the time, the two years of my being President-elect I essentially did a lot of informal survey work on my own, and talked with a lot of people, not just about *Child Development* but about the Society, and then formulated ideas as to how I could make a difference. As incoming president, I felt a sense of deep responsibility and obligation to the organization, and I wanted to have a presidency that would make a difference. So, in some senses I wanted to be a change-agent in terms of certain issues that I felt were around, and that I uncovered during those two years of my being President-elect.

Child Development was only one of them; it happened to be the most visible because it is a publication, and because it's been a very successful publication by any measure. But I could see from discussions with many colleagues that there was an increasing problem of perception and attitudes about it that were strongly influencing readability and submissions to *Child Development* from non-psychology disciplines and applied areas. There were also issues relating to diversity itself, which was the broader area that I decided to implement change in. Overall, I called these 'diversity issues' and identified five different diversity issues for change in SRCD. *Child Development* became only one focus of working for change, but since I was on that track in our interview, let me start with that. What I did as soon as I became President was to get a convening, a meeting where Sue Summerville, the Editor and her associate editors, could meet with other people from the other committees. By this time I also had a role in appointing committee chairs that represented diversity interests. And so that was for the Interdisciplinary Affairs Committee, the Publications Committee, which had the overseeing role for *Child Development* and other publications (and had strong interests in diversity representations on the publications committee). Bill Hall was the Chair, an African American himself, and shared a number of these interests. We had people from the International Affairs Committee and we had the Chair of the Racial and Ethic Affairs Committee, as well. And we realized that there had not heretofore been a more relaxed opportunity for exchange of ideas face to face with the associate editors and the editor. And the number of associate editors had expanded in number, during Sue's editorship. I think it went from three to five or six associate editors, all of whom had separate offices. And so there was a two-day retreat convened in Chicago, in which there was an opportunity to exchange ideas and views in a supportive way.

To go back in time though, there was a lot of contentiousness exchanged during the two years of my term as President-elect, quite a bit of it. The Racial and Ethic Affairs Committee submitted its resignation in strong protest because they felt they weren't being listened to. That was the first Governing Council I went to. There was this letter, and it was – …

Haith: This was when you were President-elect?

Emde: Yeah, the first Governing Council I went to in my new role. And this was only the tip of the iceberg; there was much contentiousness around. Various elements and groups felt they were not being represented and the crystallizing part of it was around *Child Development*, where *Child Development* was seen with its perfectionism attitude. I had seen this in other organizations that I've been involved in, not just in the journals, but in admission and membership policies too, for example, such as in the Psychoanalytic Institute wherein I'm a faculty member. It can be devastating.

Haith: Is this perfectionism or elitism? Or both?

Emde: I think its more perfectionism; there may be some elitism, but I think it's more perfectionism. I don't see it as a class warfare thing at all. No, I think more perfectionism. Let me give you some examples. There were repeated stories of people having not just two, but three or four review cycles for their *CD* submitted manuscripts, with not just two, but three or four or five reviewers at a crack. I noticed you're pointing to yourself that you had that experience. I had that experience myself, which I kept under low-key because it concerned a major paper from my research program when I was President. It had five reviewers and it went through either four or five review cycles. At that point we were already implementing a number of changes, but they take time. But what I would hear from colleagues is that not only were there seemingly interminable numbers of reviews for a submission, but with cumulative requested changes in text people would ask themselves "Whose paper is this anyway?" You would hear from others, "I'm not going to submit there anymore," instances I remember from colleagues in pediatrics and sociology. The perception of perfectionism itself was devastating and cumulative, because whether true or not as a typical experience, it was getting around to people from other disciplines.

Especially, in applied areas people were saying that there's no sense in submitting to *Child Development*, you've got to go elsewhere, unless there's a special issue, and there were mixed feelings about special issues. I myself had mixed feelings about special issues. They're very hard on an editor in terms of planning. They do allow you to in-gather from some needed areas, but that shouldn't be the only reason for a special issue. But it was turning out that was the only way you could get major interdisciplinary contributions where you could farm out the guest editorship in affirmative ways and avoid this perfectionism of regular editors and associate editors. Special issues are also expensive to the Society, although Governing Council had been fairly generous in adding special issues. The articles are varied in quality, frankly, but you know, that's a bit of my perfectionism coming in there. And special issues often resulted in further publication delays for everybody else, so that was a managerial problem. So special issues weren't going to be the answer to this perfectionism or in-gathering problem from interdisciplinary fields. Plus it's hard to get people from other disciplines to publish in another sense, because you have to have a core track record in your own discipline to get promoted if you are in pediatrics or psychiatry or linguistics, for example.

So my view this was symptomatic of a larger problem, not just with *Child Development*, but a problem for SRCD if it wanted to maintain its major broad diversity interests. SRCD needed to come back to its origins with broad interests in child development from members and participants in many disciplines and with contributions from many perspectives. So I started advancing this view and as I engaged in discussions I became aware of support for changes but also of some resistance. Some were concerned with major changes of any sort. Others were quite happy with rigor and the mainstream of the core of developmental psychology and didn't so much oppose me directly but, indicated less than enthusiasm, as I proposed changes to stimulate more diversity in our organization.

What I proposed is documented in the first SRCD *Newsletter* that came out under my presidency. With Governing Council's approval, I set up a major task group as soon as I took over, which I called something like 'A Presidents Task Group for an Integrative and Diversity Perspective.' And I wanted to include the 'integrative' so that this would be a constructionist, not a deconstructionist, venture and build that in there from the beginning and let the Society know that was the intent. It was not the intent to have a diversity perspective for its own sake, only to further the best workings of the Society—that was the purpose. So that group was set up, and I also was able to have a very strong role in the appointments of the chairs for key committees, as was Governing Council's prerogative. And so we were able to get strong key people in those chairs, all with strong diversity interests.

So the announced theme of my SRCD presidency was that of strengthening our diversity interests. I defined diversity interests in five sectors or dimensions. The first was interdisciplinary. The second was international, which the Society had only recently taken an interest in, having more international members in activities and having formed an International Committee. Another major area, of course, was that of ethnic and cultural diversity with had its committee that could benefit from re-vitalization and more influence in our organization. A fourth diversity area was life-span diversity, which is something that interested me since SRCD had lost some of its lifespan research interests and foci that it had had in previous times. Interests in lifespan development waxed and waned, but it seemed to have been losing a lot of that at the time. Our central publication being called *Child Development*, periodically had people asking whether the title should it be changed to represent and attract more of lifespan development, and meanwhile SRCD had lost some of its life-history researchers. So I wanted life history as a diversity dimension, not only in terms of the lifespan of what you study, but I also wanted it to take up lifespan diversity interest in terms of the membership. So my fifth identified diversity interest was to address membership issues related to career level and age. In particular, what more could we do for students? In fact, some major things were done to further help and encourage student members, it turned out. That was one of the areas we did some good things in. Could we do more? And what should we do for our elderly members around retirement? So I wanted these five diversity areas to address membership participation as well as research areas.

So I appointed Lew Leavitt, the pediatrician who was chair of the Interdisciplinary Affairs Committee, as Chair of the overall task force I mentioned. I guess the way I could evaluate its activities over the two years of my term as president could be put in terms of whether the cup is half-empty or half-full. The convening function and the stimulating function I think resulted in a number of really good things. But it fell short of expectations of what I and others really wanted it to do, and there wasn't a strong substantive report afterwards from that committee.

But, as I mentioned, along the way a number of things got stimulated and accomplished. Further, there was a momentum that has continued I think since then, which Bill Hartup continued in his presidency, and that I'm very pleased about. Bill made some structural changes in organizational functioning based on what had emerged from this committee and our dialogs from all the other committees involved in facilitating the diversity interests of the organization. I think our many discussions set a momentum for positive change.

And I should mention one other key influence in fostering positive change for diversity. This involved the SRCD Program Committee that had increasingly incorporated and successfully encouraged diversity in presentations of research in our biennial meetings. Recognition of this, and what they had accomplished in wide-ranging programs at our meetings, became appreciated and more and more people recognized that their accomplishments could be incorporated more generally throughout the Society.

During my presidency we also set up a special students' committee to promote the roles of students and young faculty, which I appointed. We had three who met to do this, and the effort was coordinated by David Oppenheim. We wanted to address, not only student involvement in SRCD and young faculty, but also the transition from student to faculty involvement in SRCD. They did a survey, which entered into their task report to Governing Council. As a result of those activities regular student awards were set up for the biennial meetings, and that's now on-going. We also set up more explicit student involvement in committee functions, appointing students to most of our working committees.

The integrative committee served as a vehicle for the heads of the committees to talk, which hadn't happened before, and this was one thing that caught Bill Hartup's attention as a very positive thing. In his presidency, which followed mine, he institutionalized it, setting up a committee of committees. It's an awkward term, but functionally it's helpful. After that Glen Elder did this in his Presidency, as well. The idea is that soon after the SRCD president is elected he or she convenes a small meeting in Ann Arbor where our executive offices are, of the Executive Committee and the heads of the major committees to be able to talk and plan their two years work. This is important because memberships and chairs on committees rotate, some on a two-year basis. So that seemed to be another good thing that grew of our work during my presidency. We had that almost as an accident, because I'd had the integrative affairs appointed which did that, and we also had that extra meeting with the *Child Development* Editorial Board.

So the diversity initiatives did facilitate communications and awareness. It also led to a lot of discussion about *Child Development*. Some of the discussions initially were tough on Susan Summerville, because it was perceived as criticism of a negative, harsh sort early on in this process as people were airing all of the issues, including myself, trying to be constructive. But it was a tough time for her because she was struggling with a lot of difficulties from a managerial standpoint and working far too many hours. The job description for the editor had grown to be too much. And it was swamping her at times. And so this was a tough time. I had to really work to get into a supportive position with her, especially during the time when I was President-elect. And I guess I would have to say that the President before me didn't make it any easier, but she was undergoing some personal difficulties, I think, and was under great stress.

When I assumed the presidency it was tough for the first Governing Council meeting, and some transition issues were complicated by some of the factors I alluded to. But then we got over that and we worked quite constructively for the remaining times of my presidency and during my term as past president. We also had a smooth transition to the next president (Bill Hartup). I would have to say the transition to my presidency was not smooth. Partly you could say, and maybe the main thing to say, is that I was pushing change if you look at it from a systems point of view.

There were other publication issues during those years that had to do with budget. *Child Development* went from going to bringing in some $70,000 a year that could support other aspects of our budget like mailings to international members, to a zero balance. And in fact, it was a little bit on the negative side. That was okay. But then when we began to do special issues, which were a great expense, and added all of the additional associate editors, which were put in place partly to respond to some of the diversity concerns and partly because of just the things getting out of hand managerial-wise, it became clear that we couldn't go on this way. And the communications difficulties made it worse; this was a terribly unwieldy system. Each of the associate editors had a part-time research assistant or secretary, and they had staff, so it was an expensive office. Moreover, a lot of things would get lost in the shuffle and coordination was difficult.

The University of Chicago Press and other organization presses told us that we were the last one in their portfolio that didn't have a managing editor at a central office. Gradually we moved toward doing that as a recommendation, and we realized that we had to have a major structural change. And we engaged in strategic planning discussions to counter a number of these procedural issues that I labeled earlier as perfectionism. And so that was set in motion during my last stint on the Executive Committee when I was past-president under Bill's presidency, but I was on the Executive Committee and a player in the committee that recommended for the *CD* editorship. And so we have a different system now and I'll be interested to see how well it works. We're just starting to see the fruits of the new editorship changes.

We do have a managing editor hired, and in Ann Arbor close to the central office, and Dan Bornstein runs it through that system. And he has a different philosophy that's enabled by that system, which is a meld of ideas that the newer Publication Committee had and, of course, his own views, and so we'll see how this works.

To pick up on another trend of my presidency-- As I mentioned, what was called the Ethnic and Racial Affairs Committee, at the time when I became president-elect, was going to disband itself. It was in revolt and not only wanted to disband the committee but many were talking about leaving SRCD. We spent a lot of time both with individuals and with the committee, and we affirmed the importance of their work in basic and applied fields having to do with cultural studies and interventions, as well as the study of health and developmental disparities, and they came to feel more appreciated in their active and participatory roles in SRCD. And Cynthia Garcia Cole, during my presidency, was my chair of that group. She did a terrific job. And then Suzanne Randolph took over and also did a terrific job. I think that committee is a now established as a hard-working, much-appreciated committee—central for our organization.

The Interdisciplinary Affairs Committee has its ups and downs. One of the problems with that committee was that they were given, for example, control of the Lawrence Frank Symposium, dedicated to interdisciplinary research at the biennial program, and they come up with interesting ideas, but few would show up to their symposia. And that was a reflection of serious concerns having to do with planning and matching a topic with people. This was partly a result of the chair of the committee with some chairs over the years being activists and some not, but it also indicated a need for more commitment and support from the organization as a whole.

I don't know how the International Committee and its focus in increasing international participation is stacking up now. That was an especial interest of Bill Hartup, and he decided in his Presidency that he wanted to continue the momentum of these changes, and I think he's done that effectively. I think that also continued his interest from having been President of ISSBD.

I have also had a strong interest in promoting more membership and activity from clinicians in the fields of child development. And through my connections, over the years, I'd persuade people to get in, maybe bringing in three or four or more each year. That reminds me. Another thing we did during my presidency is we that got the first non-North American Governing Council appointee, Peter Hobson, who's just now finishing his rotation and has enjoyed his work on governing council. He's a very respected researcher from London who is a child psychiatrist, also trained with a Ph.D. in developmental psychology, having strong interests in cognitive development and psychopathology. He has been very well respected widely in Governing Council, so I hope the trend of international leadership in our organization will continue. And I had mentioned David Oppenheim's work with SRCD, who was a former postdoc of mine and now at the University of Haifa. I could mention others, colleagues who are quite active in SRCD who are outside North America who are working one way or another for the organization. I'm wondering if there will be an "international slot" for a rotation on our Governing Council. Those are some of my associations about SRCD.

Perhaps I could add a coda. Since I finished my term on the Executive Committee (after two years as past-president), I've had no communications about SRCD since--it's almost like a total shut-off. And I'm not sure what that has to do with, but that's one thing I might inquire about. There might be a more concerted role for past-presidents to provide some services for the Society.

Haith: What would you see as the major highlight for you as President, and the major disappointment?

Emde: In terms for me personally?

Haith: For you personally.

Emde: For me personally, it was fun to articulate these issues and the need for change and then be able to help accomplish most of what I saw as needing to be accomplished. There was a momentum and the depth of the personal collegial relationships that was just wonderful. I mean the people in this organization are wonderful. SRCD has been my favorite organization of all those I've been involved with. It has remained so, and I thought it was thrilling to be involved in a leadership level for a while. I loved even the contentious times. I can look back constructively at all those things, because there were no relationships that were fractured without repair; they were all mended even when we had rough disagreements. And there were many new relationships that were formed and expanded. I enjoyed it all. But I guess I think of some personal giddy highpoints. These occurred toward the ending times of my leadership. One was in New Orleans when I gave the presidential address and ran the business meeting and then we had the receptions, which were great fun. Joy Osofsky who was local host for that meeting and her group had organized the "second line" of New Orleans jazz marchers that came up to the podium after the conclusion of the meeting. They took those of us there and then with everyone else in the audience we marched around the outside of the hall-- it really was a "second line"—a seven-piece Dixie land band—and we all marched following them all the way into the reception hall. Then later we had the traditional president's reception with this weird three-piece Dixieland brass band. What a time that was!

Another very personal highpoint I enjoyed was the giving of the awards as past president. The whole process was fun, acquiring the nominations and, helping people write the inscriptions. But most of all it was gratifying being up there as past president giving those awards. What a thrill that was to have those shared moments with the people being awarded and the audience.

Haith: Moving on to talking about the field, whatever that is, developmental psychology I guess. Comment on your history of the field during the years you've been in it and major continuities and discontinuities and events related to them. Have your views changed concerning the importance of various issues changed over the years?

Emde: I'd like to start with the first part of that. From my perspective, which I admit that attitudinally, I'm more of an optimist than a pessimist, so I'm apt to have a bit of rose-colored glasses and see things in positive functionalist terms. With that preamble, I would like to dramatize the changes over the past thirty-five years as once where predominant modes of thinking and models about development were mechanistic and reductionistic, with the aim of securely "predicting outcomes" to now, where from my point of view, that's all but disappeared. To put it in another way, people now realize that mechanisms operate under very limited boundary conditions where you can only have predictability under limited circumstances. Developmental predictability is always probabilistic, and it only approaches any degree of certitude if you restrict the boundaries of your system tightly and even then you're going to have uncertainty and indeterminism.

When I began my research, it was exciting to learn I could satisfy my curiosity in systematic ways. I was thrilled to learn about this from Yvonne Brackbill, among others. Yvonne guided me in readings about behaviorism—and radical forms of behaviorism were still in high vogue in some sectors, especially here at the University of Denver. And there were great and interesting things happening here. Yvonne was pioneering in taking conditioning in infancy from classical to operant modes, and she was also interested in the Russian behavioristic approaches, such as in linking orienting responses and doing experiments with Bronstein effects in infant sucking. In psychoanalysis, which is one of my fields, the zeitgeist was in many ways equally mechanistic. This was, of course, paradoxical, since psychoanalysis was a discipline fundamentally concerned with meaning and individuality in that sense. The models in psychodynamics and psychoanalysis also had an SR, Stimulus-Response, kind of linear formulation to thinking about the mind for the most part, and it was embedded in drive reductionism. That was just another form of this kind of thinking of the times. Things have really changed since then.

I was fortunate when I got into research. I was fortunate partly because I had René Spitz's mentorship, as well as some of Yvonne's mentorship, and I was fortunate partly because of the times of creative ferment. When I began observing infants during my psychiatric residency with another resident (Paul Polak) we were open to a variety of perspectives and read widely. We became interested and excited by ethology, and animal behavior, in reading Tinbergen and Lorenz. (Konrad Lorenz visited Spitz in Denver early in my career.) We also read in systems theory, and I was very excited to read Bertalanffy and following up with some embryological readings, including some early Waddington. And I seized upon a systems framework for guiding my thinking. Although this wasn't so much a theory as it was an orientation or a framework I used it to organize my thinking about psychoanalysis, which was the main theoretical orientation of the psychiatric residency in which I was involved in at the time here in Denver. And so I put what I was learning about psychoanalytic theories in terms of a developmental systems framework. That turned out to be a good strategy for me so for I could see and be a part of the changes that occurred as people moved into the field more and more with process-oriented concepts. What I think of there is an appreciation of development as involving processes of increasingly organized complexity, whether you're looking at a micro level or macro level over time, and processes that are extremely dynamic, complex, and changing over time. And so now we know the artificial intelligence models of parallel distributive processing and things of that nature. Dynamic systems theory can be encompassed and made use of by these kinds of dynamic orientations to increasing complexity and change over time. Thinking of simpler systems can be thought of, as I've said before, in ways in which one can be clear about the boundary conditions of both the upper and the lower levels of the system that you're involved in.

I was influenced about twenty-or more years ago by the philosopher of science, Platt, who wrote about strong inference, in an article in *Science*. He discussed the boundary conditions for the phenomena under study and the limits and possibilities for understanding. I summarized my insights stemming from this in an early career talk given as a plenary to what was to become our emerging off-year infancy SRCD research organization, now known as ISIS, the International Society for Infant Studies (given in 1978; published in 1980).

Haith: We're continuing with discussion of the field.

Emde: There was quite a vivid, in many ways, a metaphor in my mind as well, that one can look for boundary conditions at systems levels and that the upper boundary sets the conditions of determination for the system below it. It's been a very helpful way of thinking about things and leaves room for me to fit in people who do talk about mechanisms, understanding they're approximate and we're not analogizing to a machine, but a biological system with increasingly organized complexity. It's also given me some ways of understanding why it's so hard for the field at times to fully encompass what we might think of as post-modernism in science, meaning post Heisenberg, where we know there is no strict determinism and we are always influencing by our methods and ourselves what we are observing—as well as how we're interpreting. And so much of our field often behaves as if there is strict determinism, but this larger view helps me fit that in and orient myself to it. But there has been a movement of the field, as a whole, to a greater appreciation of processes of increasing complexity and away from thinking of simple mechanisms and determinism in isolation. There has also been more of a tolerance in the field of child development, and in SRCD, for various applied child development activities and an appreciation of how those activities in context are part of the overall scientific wave.

I think advances in women's rights and women's opportunities are major changes also. On the other hand there's still a long way to go, as can be seen by faculty statistics for appointments and promotions in any of our major universities, in psychology as well as other fields, where women are disadvantaged. It's also relevant that population trends are important where I gather that a huge majority of graduate students in developmental psychology now are women as opposed to men and that's changed over the same period of time. And in some places it's very hard to persuade men to come into child development.

There of course have been other changes. When early-on I committed myself to self-study in research training, I decided systematically I had to learn Piagetian psychology and so I started reading Piaget and gathering colleagues in a reading seminar. The Piagetian orientation served me extraordinarily well at that time. It was again a systems orientation from my point of view, and it provided anchor points about developmental levels, for example with what we mean with respect to object permanence. And I found the Piagetian system extremely useful including, you know, going up to its idealized forms, which I couldn't follow totally into adolescence at all, but extremely useful for the early years and for a system of thinking. And it integrated quite well with Heinz Warner, and it still does. And one of the things—a comment I'm surprised by—is the extent to which my colleagues have to castigate or deconstruct or put down giants in our field, and it's certainly been true with Piaget. Even visiting in Geneva, which I've had the opportunity to do many times, it's there; it's as if you can't have your young academic muscle or maybe even mid-career muscle unless you find something to insult about Piaget. People are throwing out the baby with the bath water. I find Piaget still extremely useful, extremely orienting to how I think about intentionality. And periodically when people say, what do you mean by this or that, I go to those anchor points in observation and theory from Piaget. That's just a side comment. I don't know why we do this kind of thing in our field.

We just read a terrific article on the occasion of Joe Campos' visit, one of my important collaborators and colleagues that I could comment about. I've learned a lot from Joe and we've worked together. And he just came back on a visit from Berkeley, and Joe is very much into what he calls following Richard Lazarus, the functionalist theory of emotions. And we read a terrific current review article that he thought was-- and it was good, and recommended to us in our seminar that we have on-going for learning about emotional development. It was an article that took the functionalist or contextualist view of emotion, and reviewed far different areas or sub-areas of emotion brilliantly and extensively. But it was deconstructionist. It critically reviewed all these areas, pointing out all the problems and put nothing together, and it really pissed me off. And here's this brilliant article presumably by-- I hadn't heard of the young people who were authors. But why do we have to do this? Why can't there be some interests when you're critically taking something apart to putting it together, because I have very little patience with this kind of criticism in its own terms. It's probably a matter of style. I'm much more interested in integration than differentiation, but you need both for development. And these are the cyclical processes, and they should be interweaving and they should lead to increasing complexity in development; you need to have both. So I don't know why that is, but I've been dismayed periodically and surprised. I suppose the psychoanalyst in me shouldn't be surprised, perhaps reflecting on Oedipal themes. Or, Frank Sulloway would try and reduce it to birth order maybe, but it still surprises me when it happens so repeatedly.

Other main trends in the field, well there's one that bothers me a lot, and I think most of my colleagues in our developmental sciences would be extremely bothered by this. I've been very fortunate up to now and through another year when I'm going to leave this source of funding self-consciously and deliberately, that is proactively. I've been continuously funded by the National Institute of Mental Health through my whole research career up to now, both in terms of the Research Scientist Program and work on my projects. And I'm very grateful for that. And it's been flexible for me. It's provided continuous training in education. It's done everything. And I've been involved in different things with the NIMH over the times, as we would as programmatic investigators, involved in study sections and advisory committees of different sorts, and I have to express my frustration that at the beginning of this interview I reflected that this seems to be no longer the National Institute of Mental Health, that is concerned with mental health, health promotion, prevention of disorder in its fullest aspects as well as the treatment and the mechanisms and the research of disorder itself. Instead it seems to be not only disorder centered only, for the most part, but brain science has centered. Now a disorder focus, I would say as a psychiatrist, certainly is extremely important. It should never lose any focus that way. The brain sciences are extremely important, certainly should be A-core, but it has moved away not only from prevention and health promotion, it has moved away from the behavioral and social sciences in general. I think that's a very sad commentary. And it's particularly sad coming from a developmental psychiatrist and a developmental psychologist in the broader sense, which I would like to think of myself as being.

Finally, I would like to say again on the more optimistic side in terms of changes in our field, I think we are in an extraordinarily exciting time scientifically, from the point of view of basic science in terms of the genetic revolution in medicine and beyond. It's exciting, it's also—we don't know one iota of the consequences of what it's going to take us into scientifically or in terms of public policy in our professional lives—but it’s very clear that it's introducing an extraordinarily profound set of changes. Our diagnostic system for mental disorder is not going to look anything like it is now in ten years. I'm convinced of that. It’s going to be a sea-change much greater than the change from DSM-2 to DSM-3, which is when you had the change from a theoretically based diagnostic system to an operationally based descriptive system. The new changes are going to be even much greater. We're going to have to address things that we're not addressing in the areas of prevention of disorder and health promotion because it'll be discovered that everyone who's examined will be more or less at risk for a whole spectrum of disorders, with personalized specificity. And we have no idea about how to handle that as healthcare practitioners, let alone as citizens because you can't pay for everything in advance, and you're going to have to make awfully tough choices. And at individual levels, will people want to know about this because it could have devastating consequences to know you're going to die at forty, for example? For other people it might have consequences they would appreciate knowing so they could plan their lives. For insurance, right now people shouldn't find out, for the most part, because if the insurers find out they won't insure them. So to have Huntington's gene means you’re uninsurable, whereas if you don't know, and you've not examined, you’re insurable. So there are these huge consequences that lie ahead. From a developmentalist’s point of view, in science we obviously know that the only way genes work is with the environment and there are very extremely dynamic processes by which that happens that we're just learning about; with regularity genes that turn on and off, and we know little yet about genetic expression and the conditions under which genes are either expressed or not at different times in development. So as we learn more about the genetic spectrum of temperament, personality traits, which are probably going to be a lot more specific than we ever imagined. We're starting to see that there are certain genes that control receptors for risk taking behaviors and other things. We have exciting opportunities to look at developmental systems in increasingly organized complexity as it happens at individual levels as well group levels. It's going to be a totally different world and we probably will be able to have some more predictability about some things, probabilistic predictability again. We will be able to have probably a more rational basis for decisions or advice, if not control, about the range of environments that will allow people to develop in particular ways. We'll learn an enormous amount more than we have up to now about alternative developmental pathways. I guess that's another thing.

A little bit of a disappointment is that we do know a lot about some major developmental principles, and they've not been exploited very much in our field. And they have to do with these tough areas that involve increasing organized complexity. They have to do with the conditions under which alternative developmental pathways do and don't take place. I think the genetic discoveries are going to help with all of this, to help us be able to specify optimal and non-optimal environments. Unfortunately environmental changes are occurring all the time, which we have less and less control over in many regards, and that is influencing this and we need to study that too. I think of toxins, especially, which we didn't know to be toxins before, but we will now know are toxins with specific genetic types of vulnerability and things of this sort. Enough for now?

Haith: Yeah.

Emde: Good.

Haith: This is now December 3rd and we're recording our second session with Bob Emde as the interviewee and Marshall Haith as the interviewer, and we're going to start by picking up on last session with a few additional comments.

Emde: And apologies to the transcriber since the battery ran low at the end of last time. Yeah, I just had a thought that I think you asked a question as part of the schedule, something about what did I find most satisfying in my professional life or something?

Haith: It was one I made up actually.

Emde: Oh, you made that up?

Haith: Yeah.

Emde: Well, right after we finished I thought, you know, of the two most satisfying things I didn't mention. Perhaps most satisfying have been the students and mentees that I've had over the years. And I think probably that would be the case for most with a satisfying academic life. I've had some absolutely fabulous students, many of whom have gone on to top notch academic careers, as chairs of child psychiatry divisions or departments and many K-awardees who've gone on to research career awards and got funding for that in child psychiatry and in developmental and clinical psychiatry. Also I’ve been blessed with a number of medical students who've worked in my research and many have been inspired to into psychiatry and child work and to research, as well as related aspects of child development in other medical fields. So that's really been the most satisfying.

And secondly, it’s just been a sheer pleasure to have colleagues throughout my career who have been fun to be with and work with in terms of stimulating ideas, opening vistas. You were the one who said something a long time ago that has often reoccurred to me in a social context with colleagues and students. At times research is tedious and discouraging in its many aspects, and that’s just part of the job description. At times it's quite lonely. And t times you find yourself in one blind alley after another, and it seems as though no one’s going to appreciate what you’re doing or should do. And at times it seems like you could get paid more, particularly early in your career. For me, early on I felt badly for the family because I'd look at colleagues in medicine who were getting much more money in private practice as I was in academic work. But a phrase that you said way back is that, every once in a while when you get in that situation you would think, "Hey this is incredible. I'm getting paid to satisfy my own curiosity!" And that's a wonderful thing. It's wonderful to reflect upon that in an atmosphere of discovery with colleagues. That's been such a satisfying part of this whole enterprise, to be able to do that, to be able to look forward to that and to have a continuing college of colleagues throughout one’s career. So I just wanted to state that because it's absolutely the case. And to be able to pass some of that on to my students has been a special pleasure when it's caught on and they've felt the same sense of a zestful life in scholarship ad research as well in clinical work.

Haith: Well you used to use a word that you don't use very much any more, 'generativity,' and I know it's always been a major feature for you and very important component of your life.

Emde: Yeah.

Haith: You know, you're saying it now in a special way.

Emde: It's a special thrill. Right.

Haith: Let's shift now and talk about your background; where you were born, grew up, a little bit about your parents, what they were doing with their lives and what you remember.

Emde: I was born in, Orange, New Jersey. And when I was five we moved to a small town about fifty-five miles north of New York City, Somers, where I spent the rest of my time until I went to college. So I grew up in a small town. It was then a small rural town. There were a few people who commuted to New York City, and my dad was among them. But most of the people were trades people or farmers at that time. Now it's exurbia, and that town has completely transformed; since I left PepsiCo and IBM moved their headquarters to there. It's transformed.

Haith: What were your parents doing?

Emde: My dad originally was in the newspaper business where he sold advertising for the New York *Times* during the Depression. Then he was in the business of selling space and features to weekly newspapers, which in my early years growing up, was a lot of fun to travel with him and my mother to small towns. He had grown up on a farm in upper New York State, and so he loved that kind of thing and did very well at it. Interestingly enough he had control of the business east of the Mississippi and the competitive business west of the Mississippi was centered in Chicago. He was centered in New York. And others in his business eventually got together with the competitors behind his back and ousted him. He had forty-nine percent I think, of this small business. So he was out on his ear and was unemployed for a couple of years. He went—it was a failure. He then got into a marketing research business where they literally did research and questionnaires, and by that time I was off to college and was getting involved in sociology and interested research-related things, so there was some common interest then. But in growing up he always said to me, "Get a profession. Don't be dependent on being a salesman or on others in business." And that sat well with me.

I always felt that whatever I was going to do it was going to be in a profession, and my mother had ambitions for me. I think her influence was even more focused and coherent in that she loved reading and scholarship. She had went to Radcliff and got a Masters actually at Harvard and in psychology, studied with Edward Boring, and who was a personality-esthetics guy.

Haith: AIlport

Emde: A little bit with AIlport. But no, there was an esthetics guy who was her tutor, her special tutor. A young man, I think he actually died early in his career, but I may think of his name later, psychology of esthetics.

Mother had psychology books, and in this small town - somehow early I was in touch with ambition. By the way, I went to the same school from one through twelve. It was the same building.

Haith: Wow.

Emde: There were twenty-three in my graduating class from high school. I accelerated in high school because my parents thought it would be a good idea in light of the Korean War, and since I wasn't sure what profession I wanted to go in to; medicine, the clergy or law, for example, but I was focusing possibly more in medicine. So my parents thought it would be an idea for me to get a leg up on my education before I got drafted into the Korean War if that happened, and possibly make use of the medical training if I could. So I accelerated. It was easy for me to do three years in high school instead of four.

Haith: Were your grandparents around?

Emde: I had a grandmother who was a family matriarch on my mothers' side, a wonderful woman. She had twelve children. My mother was the youngest. And my mother's father died shortly after she was born; he had a heart attack and died. And she always joked that he looked at her and then died when he saw his twelfth child. It was a big family. On my mother's side ours was a very close- knit family centered in the Boston area, and we have a genealogy on two sides of that family, going back to the Puritan days in coming here. Mostly throughout that family they were trades people living in the Boston area. But in my mothers' generation they were all educated with higher degrees and went into different kinds of things, not professions, business, writing, that kind of thing.

Haith: Did your mom work at all?

Emde: Mother began as a telephone operator in the Depression, because she graduated in 1936. Was that right? No, it would have been '32 she graduated. That's right. She graduated in 1932, but still. And I went with her to her sixtieth reunion at Harvard and Radcliff, which was a time when Joe Campos', one of my colleagues and your colleagues, daughter also graduated from Harvard, and we had a nice combined celebratory time. So I grew up in a small town, twenty-three in my graduating class. At the time I was the only male who went directly to college.

Haith: -- one of the few to go on to college?

Emde: There were three who went on to college directly, actually three or four, but I was the only male. And so maybe to go backwards a little bit, growing up in a small town I think was influential. I think it was a good experience. I felt I got in touch with a lot of very valuable things about being in a small town, also a lot of the limitations. I always knew I didn't want to stay in that small town. I sensed the smallness of it, where people cared for each other but there was a lot of pettiness too. I enjoyed the diversity in terms of a lot of my friends being from families that were working-class families. I didn't see it as such then. So I got in touch with people, a lot of walks of life in that sense. But in another sense it was a small town that I grew to later realize was an extraordinarily homogeneous in many ways, and there were a lot of prejudices that were very narrowing, which I only realized later. One thing I encountered early in my experience, and only really sorted out clearly in early adulthood and in my own psychoanalysis, was prejudice against Germans. Now we didn't have blacks or Hispanics in our town to any degree at all, but this was prejudice against Germans, and there was prejudice against all kinds of groups that were in the town that came up in different ways. But the prejudice against Germans came up during the Second World War, which I remember when I was five, six, and seven, and I was very confused. My grandfather, my fathers' father, was senile, and he spent time with us. And he was confused about our German heritage last name, Emde. My dad grew up in a small town in New York and it turned out later, I realized there was a lot of prejudice against the Germans there in Troy, New York where he grew up, at least in the sector where he did. So he never wanted to talk about his family background, and it took us a long time to figure that out even though the Emdes came to the U.S. in the 1880's from Germany and were successful tailors.

In my town growing up there was a fellow nearby named Kraut, unfortunately who had that last name, which at the time was the derogation used in our propaganda to refer to the enemy in WWII, and he was the subject to a lot of teasing, some of it ugly. My grandfather started bragging about "we are the Germans and we should be proud of that," and he thought we were related to those connected to the battleship Emden. In his ranting he was talking about the First World War, confusing it with the Second World War. Also one of my neighborhood friends who was a year or two older than me was telling me all these stories from his imagination about how the Germans were about to invade our town, and I was listening to the radio programs at the time, which were incredible propaganda in which Germans were portrayed as evil in one way or another, stereotyped and equated with dirt and all this kind of thing, as were the Japanese.

Haith: You were around nine or ten?

Emde: No, I was younger. I was six, seven and eight, so I was scared. I remember when the attack on Pearl Harbor was announced, my friend had prepared me, Cooter was his name, that we were going to be invaded anyway, and that the war would come to us. So when the announcement came on the radio and my mother in our kitchen said something like "Oh my God" I thought that meant the enemy would be coming over the hill behind our house, and it was scary. And then my grandfather added to the scariness, and there were many elements of that in the small town—that's part of the small "townness."

Haith: You had no siblings?

Emde: I had no siblings in the small town, and that brings up another event of importance. I had a brother who was born when I was three. And when I was five, just before we moved to New York he drowned; he went out on the ice and drowned. And I remember a lot of things about that, the search for him and my mother calling. My mother found him—found the hole in the ice—and went out to retrieve him, and then she was hospitalized with pneumonia. It was a terrible time of grieving in the family that I remember for what seemed like months and months. Mom and Dad were crying at night.

Haith: You were five then?

Emde: Yeah. So we moved from New Jersey to New York after that and otherwise then I was raised as an only child in a small town. My mother then in this town became involved in politics. She was part of a party that was a reform party, helped to overturn a longstanding political machine, and she became town clerk then a council woman. Then she became the first woman on the council at the county level in the State of New York it turned out, when she was first at Westchester County and then New York. I found that out because about eight years ago and they announced that. Maybe it was the first Republican too. At that time she was Republican. And thereby hangs a tale. At a more recent celebratory occasion set up by the Republican party, she was invited because of this and sat at a table with prominent NY State Republicans and was harassing them during the whole awards ceremony apparently, because she's quite liberal I think in her views now. So mother she was involved. And I remember growing up we were also involved in politics. Later my dad was on the school board for about three terms, and so as a young boy growing up I was always involved in distributing leaflets and going to political meetings and hearing gossip in our home. It was all very exciting. And during those times I thought I would be a lawyer, and I would pretend to be sick at times of the national political conventions so I could stay home, listen to them on the radio and then summarize them for my parents at night. Political conventions were quite different in those days, where things got actually selected and fought out. So that's the small town part of it. As I said, I knew I wanted to get away from there although I appreciated some aspects. I had some good teachers, although there were things I even knew that I wanted to study that weren't there in the school. For example, a couple of summers I tried to learn Greek on my own and sought out a Jesuit priest to give me some tutoring. That actually was beneficial for me, but I couldn't keep it up. It helped with later vocabulary building particularly in the medical and scientific area. So I went to Dartmouth-- Should I just go on with the narrative?

Haith: Sure.

Emde: I went to Dartmouth College, we'd had a lot of family members who'd been to Dartmouth, and that was where I wanted to go, and did go. I enrolled as a pre-med there, and it was in retrospect bad. They advised an intense curriculum in basic sciences for me right off. First of all, I should mention that half of the class at that time were enrolled as pre-med, so there was a huge pyramid related to academic competition and survival. They had a two-year medical school at that time at Dartmouth and they wanted you to enter their two-year medical school after three undergraduate years, combining it with your senior year. I had already accelerated in a very small high school to begin with, which was hardly college preparatory, so I was trying to survive in this environment. They advised, in order to make their selection of twenty-two people from three hundred and sixty pre-meds, in a way that made it tough on the pre-meds and they gave really bad advice. So I took four heavy science courses, in addition to the other required courses during my first two years. Two of those were laboratory courses, so it was an incredible grind that included four physics courses. It took me a while to sort out that at Dartmouth at this time they had more set requirements for this little medical school than was the case for any other medical school in the country.

So what happened was that I dropped out of officially being a "pre-med" during my sophomore year. I thought, 'this isn't what I want to do.' And I decided I wanted to do a real major. I wanted to be at Dartmouth for four years, I didn't want to do the three-year thing, and I didn't think that I really wanted to go into medicine with this kind of stuff. Moreover I loved the social sciences and I had been interested in psychology since seventh grade, which is another story. But I loved the social sciences and had the opportunity to get into a tutorial major in sociology, which I did. There were only tutorial majors in sociology and English then, and the tutorial system of learning appealed to me. It meant that I would read with two tutors a semester. And so that is what I did for the four semesters of the last two years in my undergraduate days. I selected two tutors a semester and we would negotiate a topic in sociology and cultural anthropology; and I would then read a book and write an essay for each one on alternate weeks. So, in other words, I read a book and wrote an essay a week in my major, and it was fabulous, just fabulous. I also selected an overall theme for my readings with my tutors, of course, and the theme I selected was the social self, and I read in cultural anthropology and sociology, and I loved it. And when I came around to completing my college time and moving toward thinking about what I wanted to do, I knew I was going to apply for graduate school in sociology. Still, I also realized that I had met all the medical school prerequisites and then some, even though I dropped out of the Dartmouth pre-med program, so I decided that I would apply and do the medical school interviews, because they came earlier than those for other graduate schools. I then had some really terrific interviews with people who were very different from the people at Dartmouth, and who were interested in me rather than being exploitative. I just had a wonderful set of interviews at PNS Columbia, and I thought if I can get in here that's where I'm going to go, and I did. And I loved medical school. Even though I hated pre-med, I loved medical school. It was hard and a grind at times. But at other times it wasn't, and there were wonderful teachers and I just loved the whole experience of medical school. So that was curious. There was some developmental stuff in there too, you know, because I had good study habits by the time I went there. When I went to college I didn't get them going, and then at Columbia I had more of a sense of what I wanted to do.

Haith: Did you have any military experience at all?

Emde: No, because I was declared 4F. When I was in medical school I was diagnosed with rheumatoid arthritis, and it got rather severe at the end of medical school and during internship. And when I got called up for the draft physicals it was during my internship when it was at its worst then. But I was surprised to get the 4F ranking as a physician.

Haith: What about work experience?

Emde: What about it?

Haith: Jobs! You know earning money!

Emde: Early ones?

Haith: Yeah, through the school did you have jobs?

Emde: Yeah, I always did from the time I was in junior high.

Haith: What kinds of things did you do?

Emde: -- From about fifth grade on, I was involved in summer and part-time jobs of every sort. Having jobs was built into me, and saving for college, which I did. And I always saved one-half to three- quarters of what I earned for college, and I think my folks matched it and it was my own savings account. And then the other part I could spend. And so, I did that. Early jobs were what you might imagine, gardening and lawn mowing and doing errands and selling seeds and all kinds of stuff. But then later in summers I began to work in resorts, which I enjoyed, as boat boy, then bellhop, and I really enjoyed those jobs. And then from college I worked in a mental hospital just before college, and then I started having more experience with career-building and related jobs. I worked in a pharmaceutical factory one college summer (Nepera in White Plains) as a stock boy, and I remember spending my lunch hours reading Proust. Then the next summer I was promoted to working in an experimental unit in that same company, participating in developing screening procedures for new psychopharm drugs at the dawn of the new era for that. Working with mice, we actually developed an audiogenic seizure protection screening method for evaluating phenothiazines. I learned a lot and I remember my young mentor then, a neurobiologist named Nicolas Plotnikoff.

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Haith: So we're still talking about early jobs, career-building jobs. Go ahead.

Emde: -- My state mental hospital job in the summer after that was quite influential and sometime I do want to reflect more about it. I have the fantasy of writing a professional autobiography some day in which I'll talk in detail about some of my experiences in the state hospital because they bridge from an earlier era in psychiatry. It was an old-time mental hospital, built in the early part of the 20th century, and I worked in back wards. I participated in a lot of earlier somatic interventions for treating severely disturbed conditions, and I saw a considerable number of individuals with syphilitic psychoses, conditions that we don't see today. After that summer in the mental hospital--I had another job after the first year of my medical school, the only summer I had off. I worked in pharmacological research at Lederle labs in New Jersey. That was quite an opportunity, it was a real money paying job, but I was able to be an active investigator in a research project in neuropathology. It involved trying to work out a facet of pathogenesis related to kernicterus, which refers to a syndrome of hyperbilirubinemia in the newborn, wherein there is not only damage to the basal ganglia, but as the name implies, you see a staining of the basal ganglia where the bilirubin gets through the blood-brain barrier. The problem was: how does this happen? And there was a neuropathologist I was fortunate to work with who was interested in looking at a mouse model to see if we could bind the bilirubin in a certain way with albumin so that it would go through the blood barrier. So I worked on this problem with newborn mice and rat experiments all summer. It could be thought of as rather gruesome, trying to get the bilirubin through the blood-brain barrier. In our experiments, we eventually got it through the blood-brain barrier, but it was not physiologically relevant. So it was fascinating. I read a lot, and learned a lot, and did a lot that summer.

In medical school I did a traineeship in epidemiology with Ernest Gruenberg, which was also influential. I got very interested in psychiatric epidemiology, that obviously combined interests from sociology which I had some training in as an undergraduate. Those interests are coming back now in the later phase of my career as I'm turning more toward intervention work. And current interests are conceptually in the social self and in cultural anthropology, because a lot of work in intervention involves cultural adaptation. And we have been able to use methods that we developed in our basic research involving cultural adaptations for different groups that are in need of intervention. Especially important is being thoughtful about early developmental processes that involve not only emotional regulation but self-processes.

Haith: How did you get interested in development? When did you first start asking about things related to psychology--?

Emde: I remember thinking I'd never grow up.

Haith: I just didn't state development, right?

Emde: I still wonder about that. I remember very vividly those feelings that kids go through, looking up to adults- I remember - development, that's an interesting thing. II have reflected on how I got interested in psychology and I do go back to when I was young. I was sickly and had a number of major illnesses when I was young, ages five to seven. And I believe that's one thing that makes one more likely to go into the helping professions when one is in a helpless state for prolonged periods of time. There was most of one year during my scheduled first grade when I was out of school with a series of surgeries and infections. I've since been interested in the fact that there is a similar background for many people in the healthcare professions. I used to ask the people when I taught a course on development to medical students-- I'd ask them how many had a major illness prior to age ten, and some two-thirds of each year would raise their hands. But returning to my childhood narrative, it was in the seventh grade--when I didn't have the same kind of continued illnesses, but I still was periodically out of school, I think for a variety of motivations including, maybe I was mildly depressed, but I would also read at home and do other things. I think the little school I went to wasn't as challenging as it could have been for me. One time I was home with some flu or something and my mother said, "Here you might be interested in this book." It was *You and Psychiatry*, by William Menninger as told to Monroe Leaf. (I reconstructed in recent years that Monroe Leaf was the guy who wrote *Ferdinand the Bull*, and he was a children's writer, but he was also a ghostwriter and he wrote this for the public about psychiatry with Will Menninger.) The book was psychoanalytically oriented and quite Freudian, and I just thought it was the greatest stuff.

Haith: How old were you?

Emde: Twelve. And I started, after reading that and some other things, psychoanalyzing all the neighbors. And I found all the different varieties of neurosis around us that were then described. This included a neighbor who was a hypochondriac and had all kinds of psychosomatic and occasionally hysterical conversion syndromes. I think that was the case, even as I reflect back on it now. She had a lot of secondary gain that I could see she was getting from her husband, and so at one point I thought well I'm going study to be like Freud and Menninger in order to cure M… T. That was her name. And that became a goal.

And then that summer in going to one of my dads' resorts experiences with his weekly newspapers I saw a hypnotist entertainer. And there was this gal my age that I was very interested in, and she was hypnotized by him. So then later out on the beach I hypnotized her and after that I started hypnotizing people. And I found I was very good at it. So all during my early adolescence, I was hypnotizing people. And I'd try a whole lot of experiments. I'd read books on hypnosis and try these different phenomena. And finally I stopped doing this as a result of a rather dramatic incident. One time when I was working as a boat boy in a resort I used mass hypnosis (which I got a reputation for at the resort) and I hypnotized about forty people, most of them are kids in the boathouse. Somebody observed this—one of the parents—and went panicked up the manager of the hotel who came screaming down, "Can you get these people out of this?" And so that was the beginning of the end of that, although there were other things that contributed to my stopping to use hypnosis a lot at least at that time, particularly as an adolescent. Some of this had to do with my sense of grandiosity and defenses against it in relation to hypnosis. I think that may be one of the things that led Freud to give up hypnosis back in his early practice and writing.

So I got interested in psychology that way. And I should also say that my mothers' bookshelf was full of psychology books. I think she had in mind that I would be a lawyer and a politician, but she had all these psychology books and so she was interested in talking about psychology too. The picture the SRCD *Newsletter* that Barbara Rogoff put in the issue when I was *Monographs* Editor shows this. Barbara wanted to get pictures of editors and officers at SRCD when they were babies, and I found a picture of me in the family cradle in front of the bookshelf of my mothers with the psychology books in the background. One of the ones that you can see in that picture is Edwin Boring's *History of Experimental Psychology*, which I still have.

Haith: I do too.

Emde: So those are origins of the psychology part.

Haith: And child development?

Emde: I think I got into child development by accident, partly. Although I had interests in psychiatry I loved medical school and at Columbia internal medicine was the big thing. The best students were recruited into internal medicine because of Robert Loeb, who was the professor, and there were many terrific teachers of internal medicine and I was inspired. And I did a medical internship, thinking, you know, I could do this, this is really fun and exciting. And I had a good experience doing a medical internship at the University of Minnesota. Still, my primary passion was for psychiatry and I was attracted to the Colorado program chaired by Herb Gaskill, who came to Minneapolis and gave me strong encouragement. When I came to Colorado, I knew by that time I was interested in an academic career and I wanted to do research. I'd done some research in medical school, epidemiology with Gruenberg and some other experiences as I mentioned in the summers, and I knew I wanted to do research. The question was how to do it in this small program. Residency training at the University of Colorado at the time was psychoanalytically oriented and didn't have a research-training program or anything like it.

But a propos of development, there was a background and tradition for it in Colorado that was quite important. There were two people there at the time in the psychiatry department who were associated with research, and both worked in early development. One was John Benjamin, who was a brilliant man, but suffered from obsessive-compulsive problems. Students would come to him interested in research and he'd have them read more and more and more on some idea and then, write on something or and then come back with a design. But nothing ever happened to do research. So I knew about that coming here just from asking around. He was the Director of Research. Wanting to do research, the question became how to get around John Benjamin. Well, there was also the elder René Spitz, who was there and known to be a pioneer in infant observation. Benjamin, by the way, also did developmental research and had made some fundamental contributions in methodology, how to observe babies, and he wrote about logical processes of inference and things like that, and he also did early twin observation studies. Spitz was in his late seventies at the time and we were told he was unavailable for residents or students and moreover that he was getting ready to leave and go to Europe. And it turned out that just before I'd arrived for my residency in 1961, his wife had died, and as I've reconstructed this, there was a complicated grieving process, among other things, because Spitz had decided he wanted to go to Europe and die among friends, and he was withdrawing from Denver activities. Nonetheless another resident (Paul Polak) and I decided we wanted to observe babies and engage Spitz as a research supervisor. Spitz had enough of a reputation where we thought we would be able to bypass John Benjamin, and if we could get to Spitz perhaps we could do research and not get caught up obstructions from Benjamin. And so Paul Polak (who was two years ahead of me in residency and from whom I learned a lot) and I went ahead on our own. We found an orphanage nursery with the help of Yvonne Brackbill who was here on the faculty at the University of Denver where she was doing research with infant conditioning. So Paul Polak and I started making regular observations on our own in this orphanage nursery. And then when we built up observations over several months, we called Spitz on the phone and we said, "We would like to meet with you. We have made some infant observations that disprove some of your theory."

Fortunately Spitz was delighted rather than offended by this bit of arrogance, and we told him about our observations and ideas. We were looking at Spitz's writings at that time about the developmental origins of the smiling response that occurred in infancy at three months. And at that time Spitz was borrowing explanations from ethology, and theorized that when smiling occurred it was an innate releasing mechanism. And we thought we had observations, which disproved that, and we wanted to devise a full experiment. So he came out and he became engaged. He came out to the nursery and the babies really turned him on, when we drove him out there. And we then began weekly supervision with him even though he was making plans to go back to Europe. Thus we had supervision with René before he left, and it turned out to be for more than a year.

And we wrote two papers then. These were two studies on the natural history of the smiling response in infancy that resulted from our observations. In the course of what we were doing, we came up with a method for quantifying the smiling response over time and we were one of the early people in the literature to do that. Other people were doing it at the same time, which made it even more exciting because we networked with them. And Jack Gewirtz was one, and Anthony Ambrose was another. And another thing we did was to discover and describe a clinical infant syndrome, which extended an earlier syndrome described in the literature by Spitz, that of anaclitic depression. We had observed a particular infant from his earliest weeks of postnatal life in the orphanage nursery and then saw the depressed syndrome develop in the third quarter of his first year. Spitz had described the syndrome as resulting from the loss of a mother, and here we documented this form of infant depression as resulting from the loss of multiple caretakers and other factors. It also lasted longer than the syndrome Spitz had originally described and the child recovered under additional circumstances of return of caretakers as well as other factors that we described. Spitz was able to observe the infant and verify the syndrome with us before he left and guided us in our continued observations. We included Spitz when we wrote that up and published it (in the *Journal of Child Psychiatry*), in fact, we followed with observations of that child up through middle childhood and published that follow-up as well with treatment intervention in the *Psychoanalytic Study of the Child,* a very interesting set of reports which is still informative for people thinking about early depression and psychodynamic features of life. So when Spitz left he was an inspiring teacher and person. It was just a privilege to be with him and around him for both Paul Polak and myself.

After Spitz left I had the idea of developing a research program. When I finished my residency I worked at the Colorado State Hospital which was for the mentally ill. During my residency I was in a program where I could increase my salary, and support myself and my developing family by sort of indenturing myself to serve two years at the State Hospital. When it came time to serve, it turned out to be an exciting time to do this, since state hospitals were changing dramatically and moving to community mental health orientations. And so I was part of that, and I ended up after completion of my psychiatry residency in Pueblo, Colorado running a large set of community mental health services on the entire western slope of Colorado from the State Hospital in Pueblo. The state hospital had undergone “decentralization” so that units became organized according to communities served in the state, rather than according to severity of patient illness (wards and back wards). And phenothiazines and other psychotropic meds had recently been introduced as well as a variety of social therapies. Our goal in our Western Division that I was put in charge of was to eliminate our in- patient state hospital unit at Pueblo, to have all our patients in Western slope communities with services there. It was exciting and we were part of that radical time for developing therapeutic communities. And we put more of our resources that year into the community than were at the State Hospital. We published papers about our work, and t was a very exciting time.

Haith: Did this begin during your residency?

Emde: No, began as soon as I completed it. During that time in Pueblo at the state hospital I also was doing some

EEG work. I'd started learning about EEG work during my residency with David Metcalf. He was a terrific teacher and I loved the work. I continued working with EEGs at the State Hospital with David as teacher and I put in a research proposal to do my service time there for my second obligated year in Pueblo in a research capacity in their EEG lab. It was turned down by the State Hospital system. But they then had second thoughts as a result of initiatives from the Department of Psychiatry in Denver via its chair Herbert Gaskill and new circumstances, John Benjamin had died suddenly towards the end of my first year at the state hospital. Gaskill wanted to bring me up to Denver to the psychiatry department there to see what could be resurrected from John Benjamin's' files of all the research he'd been doing for all these years and hadn't written up, and he wanted to see if I could I take over a program archive left by Benjamin. Herb Gaskill actually bought out part of my time and persuaded Charles Meredith who was superintendent of the state hospital in Pueblo to let me do the EEG research for the other half of the time. So for my second indentured year at Pueblo I was actually commuting to Denver from Pueblo and also doing EEG research. During that year I did some teaching and went through enough of John Benjamin's' files to discover that this wasn't my style of research. I was too often falling asleep reading his charts, doing archival research. I wanted to do my own thing and observe freshly and design manipulations. I wanted to continue some of the ethological methods of observation and mix it with some experimental stuff. So I made that decision and also helped Herb decide that really there wasn't a lot that could be done with the Benjamin files by me. Kay Tennis who worked with him as a research assistant for years picked up on some of it, and I've tried to foster some of that with her picking up on it.

That year we also started a reading seminar for research colleagues and faculty there, most of whom were close to my age or a little older. The group included Dave Metcalf, Kay Tennis and others and we started by reading *The Origins of Intelligence in Children*, of Piaget. We then read some more of Piaget, went to readings in General Systems Theory. We read works by Kenneth Boulding in general systems theory, and we even got him to join us in one seminar since he was at the U of Colorado in Boulder at that time. That reading seminar group in development continued and expanded and I think of it as the very beginning of today’s DPRG, our Developmental Psychobiology Research Group. The seminar met every other week and expanded its interests to a variety of readings on developmental research.

Haith: This was when?

Emde: 1966 and the beginning of 1967. And when I came back it continued and was a research seminar first for Piaget, Kay Tennis, Dave Metcalf, Tony Kisley and myself, and then it grew to include Sid Workman and some others. It was a reading seminar, and by the time Charles Kaufman came about three years later bringing his pioneering non-human primate lab, we were starting to talk about each others' research and presenting it to each other at its early stages.

Haith: And was Spitz attending in those years?

Emde: No, Spitz was in Geneva when this began.

Haith: Because I remember coming and giving a talk and he was at the talk.

Emde: Spitz would have left Denver for Geneva in 63' or something like that. While he was in Europe we had a good correspondence. He was an active correspondent. He was there about five, five-and-a-half years, and several things happened; I think he got sick, and he just—there's a funny way of telling it—he decided he hated being sick and reflected that he wasn't ready to die. He had some parasitic disease in the Canary Islands. Secondly, he thought he was going to go among old friends to die, and he didn't find the old friends he expected. Even Piaget more or less snubbed him from what I can figure out. The psychoanalysts in Geneva weren't very hospitable, although he helped to found the Psychoanalytic Society in Munich and they were very grateful about that. He did some other things in Geneva but I gather that he wasn't very happy there. He also became more excited about the research program that I was developing back in the Department of Psychiatry in Denver. What happened was that I decided to--with Herb Gaskill’s very strong encouragement and support in every way he could-- to apply for a Research Scientist Development Award from the NIMH, which in those days you could apply for without having formal research training. This was me. I had no research training except, you know, in the early years from my colleagues in SRCD and from Yvonne Brackbill and a little bit we'd done with Spitz. So I designed a research project for the career development award. I decided that one of René Spitz's works that impressed me the most had no empirical support whatsoever. It didn't even have empirical definition. It was called The Genetic Field Theory of Ego Formation. It was formulated in a book that resulted from a series of lectures he gave on the 100th anniversary of Freud's' birth, given in New York and published as a small monograph. It's one where Spitz was quite abstract, connecting with the psychoanalytic metapsychology of the time and I decided to boil it down to operational terms. I did this because I thought it was a brilliant set of theoretical propositions and should be tested. I thought it should be the basis for exploratory work that would open up other possibilities. And these could be tested. Basically the idea was that development occurs in uneven rates, or step-wise rather than in a straight line, and that you have particular times of transition, developmental transitions. And at the times of step-wise transition development may be occurring more rapidly, but more importantly there were qualitative transformations, which he described. And he felt that emotions indicated new organizations and had a major role. He took a bold departure from the way emotions were thought of in psychoanalysis and behaviorism at the time. He was more Darwinian in this regard since in his theory emotions were organizing. And he took certain emotion events that occurred in development as indicators that these transformations had taken place. And he used conceptual basis from Needham's Embryological Theory of Organizers, I think it's Needham, and of course a lot of the elements of that embryological theory turned out to be wrong today.

Haith: There was Spemann.

Emde: Spemann- Yeah-I think that's right. Spitz proposed a whole theory about how ‘early organizers of the psyche’ worked, how the processes happened of the transformation. But at a certain point he said there was a new, what he called 'modus operandi,' everything changed. It's still a very powerful idea actually. He thought of this process in terms of affective changes being indicators of the organizer. The organizer for him was at this abstract level, most people even today don't realize that reading Spitz. And so the smiling response at three months was the indicator of the first organizer for Spitz. Eight months anxiety, what he called “eight-months anxiety”—or stranger anxiety, or what we called stranger distress at that time—normatively would be the indicator of the second organizer. And for him the indicator of the third organizer was the semantic 'no,' the onset of the semantic 'no' that Spitz described to be expressed at fifteen postnatal months. This theory seemed to me to provide a great opportunity to study individual variation. I wanted to see if there were necessary sequences, which I postulated in the grant, and how they might involve a series of specified cognitive emotional and social events. I also proposed some electrophysiological events in development based on my interest in EEG and early sleep and wakefulness states. And, parenthetically, Spitz missed another infant time of transition or developmental shift that we see now, which is around twelve months, associated with the onset of walking and all sorts of other changes. Spitz didn't focus on that one. But the others are still important transition times and we continued to study them throughout my career. In the early longitudinal study that was a part of the initial career development award, we didn't see the necessary sequences I had specified; in fact, we reconceptualized how we thought about these transformations. In fact, emotional events didn't occur at the beginning as Spitz thought of during these times, but tended to occur at the end of the times and they had a role as we saw it in consolidating these new qualitative levels of organization. This seems to make a lot of sense when you see what happens socially when these affective changes occur. And the affective changes themselves may be the consequence of a whole bunch of things. It fits in much more with the way we think of developmental processes and systems changes now.

Haith: Okay. You've covered some of the other things under personal research contributions. And I think we've covered what your primary interests in child development were at the beginning of you career. This is interesting. What continuities in your work do you feel most significant? What themes?

Emde: Development - The basic principles of development, of developmental processes continue to fascinate me, and they just continue to fascinate me. When I began my formal psychoanalytic training in 1969-- I had already formed a developmental systems view, such that this became the overriding feature of my orientation to thinking about research and clinical work. I then nested psychoanalysis within a developmental systems framework as one kind of theory, and this framework has served me very well consistently. I have since written a fair amount in areas of psychoanalytic theory as well as in translating and synthesizing developmental research with psychoanalytic theory and in changing psychoanalytic theory. And one feature that has been a constant one is seeing development as involving increasingly organized complexity. And seeing development as involving continual transactions of the individual in context, with systems that are becoming increasingly heirarchicalized and connected in novel ways. This framework includes seeing development as non-deterministic, and understanding how to think about that, both in terms of ones science and clinical work, as well as ones responsible roles as a teacher and citizen. Those notions have served me well. They came early from Bertalanffy, Heinz Werner, as well as through Piaget and Vygotsky. I think they also fit in a great deal with some of the early ethological work including from Darwin that excited me in the very earliest days of my own work in observing. I have also had a fascination with processes of developmental transformation. I just find these wondrous. And I have a belief in possible developmental transformations in therapeutic situations, where we can refer to them as “new beginnings”. This can be helpful when one feels incredibly stuck in therapeutic situations where people cannot seem to get beyond rigid repetitions of maladaptive behavior and conflict, which is one definition of psychopathology or the repetition compulsion as it would be seen in psychoanalytic work. One often thinks you can never get beyond that, but the most amazing thing is the developmental capacity for transformation. It is there. And it often happens in very constricted, rigid, maladaptive circumstances within the context of a new intimate and supportive relationship. I've been more and more impressed with that. Here we're getting into some of the discontinuities, but the continuity in my thinking is the fascination with this and the belief and the optimism in developmental transformations.

Haith: Have there been shifts in your thinking along your career path and reasons for them?

Emde: Sure.

(Interview continues after a break)

Haith: Okay, we're back again and we were talking about what shifts had occurred in your work and thinking, and what was your thoughts before them?

Emde: As I mentioned, I've become more and more impressed with the power of social relationships in development and in my clinical work. More of my research methods moved to focusing on mutual influences, to coactions to use that term, between genetic-biological influences and environmental-experiential influences. And I think that's going to be increasingly profound arena for all our work as we're mapping the human genome. We're discovering all kinds of patterns, not only syndromes from a medical or psychopathology view that we haven't seen before. These include syndromes of risk and we have yet to understand how that's going to work itself out across development and with transactions or coactions of the environment. This is an area that we're all going to be preoccupied with. There's just so much that we're learning about that.

Haith: It's interesting how you've come full circle back to Darwin in some ways.

Emde: And also to the social self. I'm now involved in providing empirical research training for psychoanalysts at the University College London with a summer program sponsored by the International Psychoanalytic Association. And one of the things that I assert is that psychoanalysis is as much an interpersonal psychology as it is an intra-individual psychology. A lot of the research now that is being done that is psychoanalytic has an interpersonal focus. There have been other kinds of changes as well. Earlier emotions were thought of as reactive, intermittent and disruptive pause points. But we came to find it much more useful to think of emotions as active, organizing and adaptive processes. Using a principle of regulation, of enough and too much, in the mid-zone emotions are going to be organizing.

For a while we got very excited when the early cross-cultural facial expression work came out, following ideas of Darwin and Sylvan Tompkins, where Cal Izard and Paul Ekman in two different programs showed that there seemed to be cross-cultural universality for Darwinian categories of emotional expressions, both in recognition and expression. Because of this work, we thought for a while about discrete categories of emotions and did a lot of work along this line. My colleague Joe Campos and myself along with many students and colleagues, carried on work to see to what extent we could map these discrete facial expressions of emotion in infancy prior to speech development, because the Izard-Ekman work at that time was done with adults, and we wanted to see emotional expressions before major kinds of socialization could happen. And while we found some evidence for discrete emotion patterns and their recognition and use in care-giving in the first year, much more evidence has accrued that the way emotional signaling works in everyday life in infancy is not through these discrete emotional signals. It works through low intensity signals and blends and mixtures. For a while we were intrigued with facial expression centered theories of emotion, but now I think more complexly. The facial system is one of a number of aspects of emotional processes. It is more appropriate now to take an emotion processes view, in which it's most useful to think of emotions in terms of components and processes, not in a linear way that goes from stimulus to response, but in ways that have multiple feed-ins and regulatory components that be modeled also by parallel distributive processing. Today we think less in terms of discrete or basic emotions that build up in some kind of building block-way from early infancy. Emotions are configured in important ways that certainly have biological constraints but also have social constraints, and these are different in different cultures. And emotions then get linked to cognitive representations in different ways during development too. So that's a change in thinking.

Peter Wolf wrote a critical exegesis, supposedly of our work on emotions that was published a few months ago that criticized my work and that of Dan Stern from a number of angles. But the title of it was, “The Irrelevancy of Infancy Research for Psychoanalysis.” His main argument was a hermeneutic one, that psychoanalysis was mainly an interpretive venture, but along the way he criticized some other things about our work up to the early nineteen eighties, and for some reason didn't cover any of the work from eighties on, regarding the last ten or fifteen years of our work, in which this part of our thinking about emotion has changed. That's why I just mentioned it.

Haith: Moving on, we're reflecting on constraints and weaknesses of your research and your theoretical contributions. What do you think its impact will be?

Emde: I hope that our work will be seen in terms of elucidating some basic processes that appear in early development, and that are life long, and that's one of the things that Peter Wolf misses in his critique by the way. I see a lot of what we've been articulating as having to do with basic processes of development that are motivational—what we have described in theoretical writings as ‘fundamental modes of development”—that are a basis for social-emotional development and perhaps for moral development. We see these inborn features in early infant development and they continue throughout the lifespan. They can also be thought of as ongoing non-specific developmental factors important for clinical interventions and for preventive interventions. It's what… [Recording ends here, missing part of the response.]

I was just saying that I’ve decided not apply for a renewal of either my NIMH project grant or my Research Scientist Award, which I've held continuously since the beginning of my research career. I'm not going to renew these, so when they run out in one year I will not be on that track. Instead I plan to move my research program to preventive intervention research in order to see if some of the things we think we've learned can be useful in preventive intervention settings and can make a difference. So I'm shifting my funding basis, and in many ways, the style of my research laboratory. That's not only happening, it's happened, because now we are research partners for two early Head Start intervention sites. And I have two research grants from the Agency for Children, Youth and Families instead of NIMH. It's interesting to me that it's a different world when you are supported to do research by an agency that's not used to supporting research, which is true for ACYF. They are newer to directly fostering research and they do not plan to support your research infrastructure, which the NIH does more or less, and so one has to really scramble to do the research. So right now I'm in the process of hoping to corral private donors to help with that. And of course intervention work is full of crises and different kinds of frustrations. But I do enjoy it.

Haith: Getting back to the strengths of your research and theoretical contributions, what do you feel that you will walk away with and leave to the field?

Emde: I’m not sure if it got on the tape before, but I hope these would involve our elucidation of early developmental processes that are organizing, and that continue throughout the lifespan and about how some early experiences may facilitate or inhibit particular developmental pathways with respect to emotion regulation, and with respect to moral development.

Haith: Social development.

Emde: I also hope that our elucidation of developmental transformations, both in terms of studying them, thinking about them conceptually and making use of them in the context of intimate relationships, whether in care-giving or in therapy will also be a sustaining contribution. Berry Brazelton and his group are making use of theories and knowledge of developmental transitions in terms of providing intervention opportunities for parents. This fits in with theoretical clinical writings I have made concerning ‘new beginnings’ in development.

Haith: But you've certainly helped push it.

Emde: Within psychoanalysis I think I have a substantial role in making available developmental knowledge and thinking for both theory of psychodynamic therapy and research approaches for psychoanalysis and psychoanalytic therapy. Probably now most of my invitations to speak come from that area.

Haith: Well, this is a related question, but what published or unpublished manuscripts do you feel maybe just were wrong-headed if any? In child development especially.

Emde: Actually there's one paper that came in two parts that's had a strong impact in psychoanalysis and developmentally-oriented related clinical disciplines, and it was, I think, published in 1989 in the International *Journal of Psychoanalysis*. It was a synthetic paper that brought together a lot of recent developmental research mainly from infancy, and formulated it under some motivational constructs that are different from traditional psychoanalytic motivational constructs. That paper has had quite an impact. And there was the second part that had to do with the theory of therapy that derived from it, but particularly the first one has had, I'd say, a substantial impact. But there was more there in your question. What was wrong headed?

Haith: Were there any papers that you've written that were wrong minded?

Emde: Well, our early monograph, which is, I think, called *Emotional Expression in Infancy*, I feel good about and that it made good use of some of Spitz's theory and led to some interesting findings. But I think it was quite simplistic looking back on it. Simplistic to think that there would be necessary sequences across developmental domains and I think the monograph tends to reify these times of transformation too much. Also, a number of our papers do seem to go along the line that there are discrete Darwinian prepackaged emotions which remain to be discovered, and that these would unfold in development, and that's wrong to the extent that that's implied. I can think of wrong things right now we're working on.

Haith: You should reflect on those.

Emde: Well, that's what's preoccupying me. To what extent Is it appropriate to think of emotional traits as temperament? And I think, for example of high positive emotionality. Positive emotions have been another preoccupation in my thinking and work. That's actually a significant contribution of our work I would hope, that I should have mentioned before. This includes the role of positive emotions in development and some of our early work on endogenous and exogenous smiling systems that I think holds up quite well today.

Haith: What publications do you feel best about in terms of representing those contributions about positive emotion?

Emde: Well, there's one paper that summarizes five previous papers on early smiling. It's called, “Early Endogenous and Exogenous Smiling Systems in Infancy,” published in the *American Academy of Child Psychology Journal* in 1972, and there was a psychoanalytic theoretical paper on the use of positive emotions in psychoanalysis published in 1991 in the *Journal of the American Psychoanalytic Association* that followed another in the same journal the previous year on the fundamental modes of development as they pertained to therapeutic action.

Haith: But almost all the focus in development of emotions has been on negative emotions.

Emde: Right. And recently we've been trying to go after a number of temperamental traits that are related to emotionality.

And I've been impressed with that there's much more change when we look across development than there is continuity. The question is whether you can describe a dimension of positive emotionality that goes across contexts that is trait like, and we thought that should be the case. But I don't think it's working out, even though there are some recent reports in literature that would encourage this. Perhaps thinking about temperamental traits, we're still going down the wrong road. Coming from Jerry Kagan’s work mostly is that first of all you would only see developmental continuity in specifiable contexts, so for behavioral inhibition in his work, that would be the unfamiliar. And perhaps it's useful to think of some aspects of temperament as a pattern or a syndrome in some parts of the population—perhaps as an aspect of behavior in some people, in some specifiable contexts. But even there, there's something unsatisfying about the way we're going about this as we think about it from the point of view of our longitudinal twin study, for example. I think our views of continuity are going to get reconfigured totally with upcoming genetic discoveries and how we once again will take a fresh look at how genetic and developmental processes operate in terms of environmental co-actions, in terms of probabilistic epigenesis as Gilbert Gottlieb talks about it.

Haith: There's an old business saying, leave something for the next guy. And so –

Emde: And that's mostly everything.

Haith: Next question relates to your experiences with research funding. You mentioned a little bit, but what your thoughts are about the research funding apparatus over the years. And something about your participation in shaping research funding.

Emde: Well I've been concerned –

Haith: Study sections, counsels and securing support for your own work –

Emde: I've been concerned over the years that there's not enough funding for longitudinal research, and that periodically in the midst of bio-behavioral research, many aspects of the social and behavioral sciences get short shrift. Actually I think I made a sarcastic comment very early in the interview, during the last time we met, about the National Institute of Mental Health becoming the National Institute of Brain Disorders. I think that the pendulums has swung much too far in that direction. My colleague and friend, Herb Pardes was instrumental in this view of moving NIMH, when he became Director, to a disease in brain orientation. His view might have been wise, since a lot about "mental" and "health" were under attack then politically in the politics of NIH and the government at those times, and advances in the neurosciences were on the horizon. Perhaps that had to happen. But I think the pendulum went too far.

In my field of psychiatry, I would like to see one of the main branches of concentration to be that of the developmental psychiatry. That's what I consider myself, a developmental psychiatrist. Further, I don't think developmental principles have been assimilated enough at the National Institute of Mental Health or in our peer review system in general. Over the years, I have served, not only on review committees for grants at the NIMH but it turns out for a while had advisory committees for NIMH directors, and I served on three of those. And I was a strong advocate for developmental studies in NIMH portfolios.

Haith: Right. Why don't you back up to -- because I know you were involved in small grants for a while –

Emde: I was involved in reviewing for small grants at NIMH on a committee with you.

Haith: Why don't you give us a chronology of your involvement with the funding mechanisms, including The MacArthur Foundation—a sort of chronological sequence.

Emde: I'm trying to think. I started reviewing for journals, because that begins this trail. I reviewed casually as a guest reviewer and then, I reviewed as a member of editorial boards. That kind of work preceded my going on a study section. So I think my research program had been underway, maybe I'd been funded at least four or five years before I went on a study section. And I think that's important, in that at least I had some confidence in being positive, not just negative in arraying criticisms. It was important because when you get on a study section you hear your colleagues competing with hypercriticism and particularly inexperienced and young people do this more. The way you prove yourself or show you are powerful is to be hypercritical; find the flaws that nobody else sees. That's the big deal, and it breeds perfectionism, an attitude that can kill a young person starting research as we've talked about.

When I went on the small grant study section I loved it, and maybe I was even further along than four or five years into my research, because it certainly was in the nineteen seventies. Right? We were in that? What I loved about small grants was that it covered the whole spectrum of NIMH research applications, and again I loved the cross-disciplinary ideas and approaches, and all of us were learning all the time in that study section, not only from the reviews, but in the corridors and the social times in the evenings—we were learning from fascinating colleagues. As you know we both were enthusiastic recruiters for people to get on that study section, because we thought it was a great experience. It was an important program, giving relatively small amounts of money to help beginning investigators primarily and also creative high-risk high-gain pilot work for new ventures. It was exciting, while it existed.

Haith: You were also involved with the March of Dimes?

Emde: Yes again with you I was, right. I think I was fourteen or fifteen years on the March of Dimes study section. I believe it was called the Social and Behavioral sciences study section of the March of Dimes? Again, it was interdisciplinary and cross-disciplinary. A lot of neonatal intensive care unit follow-up studies came to that review panel and it also reviewed related work of children at-risk developing in context, and work about improving interventions. There were also basic studies we reviewed in cognition and language as well as in neurological conditions in follow-ups. Again, there was lots to learn in that work as a reviewer. It was fun.

Haith: And we funded some people early on in their careers who used a relatively small amount of money as a stepping-stone to later funding from the NIH.

Emde: Similarly to what happened in the small grants program. That's right.

Haith: -- Yes leveraging money and early studies to go on to federal grants.

Emde: The next thing I guess I would comment on is our developmental psychobiology research group. I talked about the origins of this already in our early reading seminar. And at that time I began doing a lot of reviewing of grant applications for the Grant Foundation, and actually that may have been some of the first peer-reviewing I started doing early in my career. Phil Sapir who was the research director hooked me into reviewing as well as Doug Bond, who was President of the Foundation and a psychoanalyst I met through my Chair, Herb Gaskill. I got to know Phil Sapir, and I started being a regular reviewer for the Grant Foundation, which in those days was largely focused on infancy research. And during this time Charles Kaufman came to our department of psychiatry with his primate lab and his research programs as well as his wonderful generativity both for research and for developing researchers. He was also connected with Phil Sapir and doing reviews for him as was our EEG colleague, Dave Metcalf. Locally at that time, we had more of a mix of developmental researchers and we were meeting regularly to discuss and stimulate our research, and we now called ourselves the developmental psychobiology research group or the DPRG. We continued our bi-weekly seminar with the purposes of promoting new research and offering a forum for constructive criticism all along the way, but particularly in the early phases where there were blueprints of designs that could be shared, and we tried above all to help young people beginning their research.

And then the Grant Foundation then came to us and said, "We have an idea for funding research in a different way. Would you help us develop this idea?" Their idea was to endow a group to facilitate new research, and particularly new research of beginning investigators. So we said, "Yes we'd love to help you develop it," and we applied for it. As a result of our application, they awarded us an endowment of $500,000 and we delayed investing it for about nine months at a good time for investing, so when we started up our DPRG awards program we had $600 K in our endowment! This endowment fund exists to this day as does our DPRG group with its 2 meetings per month, its own funding of awards and with an ongoing successful postdoctoral program that has had continual NIMH funding. Our first priority at the beginning of the endowment fund was to facilitate the research of young investigators. That remains so today. And the second priority was to provide seed money for special opportunities, one time funding for new ideas. A third priority was to fund projects of the group as a whole, such as our biennial retreats, and providing funding outside speakers and consultants. Funds for emergency one time support was a fourth priority, but was used that way with great caution.

Our DPRG, as you know, has been a model for a number of other national groups. It was a model for the Carolina Consortium, for example. I remember Carol Eckerman came out and made a special visit early on when they were setting up the early version of their research group that, like ours, crossed institutions and departments, and the Carolina Consortium has been an extraordinarily generative group over the years. And we've just gotten, what, our fourth or fifth five-year postdoctoral institutional training program approved. There’s a lot of enthusiasm in our group, as you know, since you're a part of it. So that's an important funding mechanism. And I think it became one of our influential models.

John Conger was an early member of 'our DPRG group. When the MacArthur Foundation formed and they were still trying to figure out not only how much money they had--because it took them two or three years to figure out how money was in the bequest--they knew they had a lot, and they knew that one of their primary designations in the will setting up the foundation was for mental health research and development. I'm not sure if the development was in there or not, it'll be in John's SRCD history, because he may have put that in. But they asked John Conger, who had a strong reputation as having been a successful Medical School Dean and Administrator as a Vice President of the University of Colorado and had retired from that, if he would help set up the foundation for the mental health and human development research program. John went to the new John D. and Catherine T. MacArthur Foundation in Chicago as its first research director which he carried forward on a half-time basis. And I think the DPRG was a very important model for him, to think about what was needed. I remember he had a number of focus groups of leading investigators to help him develop his ideas. He concluded that what was needed was something that would generate collaborative research and would advance a culture that we had very little of for research, a culture of cooperativeness and generativity that was multidisciplinary, and that could encourage high-risk high-gain research. The latter seemed to be penalized in the federal funding system, as was cooperation. MacArthur wanted to encourage a new generation of young investigators in this spirit, so they sent out requests for proposals for forming networks of investigators, across institutions and regions. They asked groups to apply who would then be joined as nodes in particular identified areas. And one of the areas they asked for was research on the transition from infancy to early childhood. This was one of a number of topical areas they had identified through their focus research groups, and I had participated in a couple of those groups. We applied and we were selected and that could be the basis of a whole other interview, about the MacArthur networks I chaired on the Transition from Infancy to Early Childhood and the related one five years after the first on Early Childhood Developmental Transitions.

Haith: Sure.

Emde: I was first called Coordinator and then Chair, Coordinator of the Colorado node and then of the network of five geographically distributed nodes. These involved Seattle, San Diego, the NIMH intramural program under the leadership of Marian Radke-Yarrow, Harvard and Colorado. This transition network with the five nodes lasted five years. And then in the second five years we transformed our network under some new leadership in the MacArthur Foundation. (John Conger had retired.) The new leadership wanted networks to be organized not around geographic nodes but around topic groups so our renewal did that, and in the second five years we were designated the Early Childhood Transitions Network. Doing this, we could still continue some of our interests that we had begun, particularly related to our longitudinal populations and the methods that we had been developing, which were quite new.

Haith: You had a very strong influence on a lot of funding that got things going through those MacArthur networks.

Emde: Yes, and there are a lot of proud accomplishments from those efforts.

Haith: Several books came out.

Emde: They're still coming out. Another four or five are coming out, beyond those published.

Haith: And - yeah, Arnie Sameroff and I we did one together on the *Five to Seven Year Developmental Shift*--

Emde: Is it out?

Haith: Yes.

Emde: How about that. When did it come out?

Haith: Fairly recently. You didn't see it? I'll have to give you a copy.

Emde: Well, they were supposed to send me one.

Haith: Arnie and I should sign a copy.

Emde: Terrific. Congratulations.

Haith: Thank you. But speak about the child language database –-

Emde: The Child Language Data Exchange System or CHILDES was sponsored and enabled by our first MacArthur network. Brian MacWhinney and Catherine Snow made it possible for child language investigators to share data and helped to move that field from case studies to quantitatively-based larger numbers work. The MacArthur communication development inventory, the CDI, again was directly sponsored developed through our network, and there's a whole impetus for early moral development through our network. There's also a battery of story-stem narratives coming from our network (the MSSB), which is providing a direct entree into the child's internal world as well as to behavior problems and psychopathology from ages three through six that's come from our network. We're writing a book on that. The MacArthur Longitudinal Twin Study, which is providing evidence on the dynamics of continuity and change with respect to mutual influences of genetics and environment, both shared and non-shared is another product of our network. This provides knowledge, across time, not only for assessments of temperament and cognition, but for emotionality, from infancy through seven years. Your future oriented processes synthesis and the generative launching of a field related to that also began in the second MacArthur network under your leadership of that topical group and, of course, this topic has a book of its own.

Haith: And the Behavioral Inhibition Work.

Emde: The behavioral inhibition work has gotten a lot of its support from our networks. For some reason in the early days it was difficult for it to get support from other sources, but it did receive support through our network, supporting the creative work of Jerry Kagan, Steve Reznick, Nancy Snidman, and others. In addition, we contributed to the development of psychological measures, with two longitudinal samples supported by our network. And there was a lot of the method development related to empathy and its longitudinal configurations with the studies of Carolyn Zahn-Waxler, Marian Radke-Yarrow, that was enabled by our MacArthur networks, supplementing the government's intramural funds.

A number of intervention probes and methods development in intervention also took place and were enabled by our transition networks-- in the work of Katherine Barnard, Joy Osofsky, Leila Beckwith and others. We were able to supplement and connect data basing that was being supported separately from the NIMH, and we were able to sponsor needed method developments and common data basing, which wouldn't have otherwise occurred. Not that there were dramatic findings from those studies, but a lot of important strategic knowledge resulted from that, and lessons were learned that went into designs of other longitudinal studies. I could go on about contributions from the MacArthur Transitions networks, but perhaps that's enough.

Haith: They were a major influence on the field.

Emde: -- The best thing that we did, and it was a big disappointment to me that the foundation lost its commitment to this, was that we were changing the culture of science in our area and we influenced a generation, an important generation of young people in our first wave (that is, the first network that was organized by nodes of collaborative activity). I wish we had continued funding to continue efforts that resulted in major collaborations across disciplines, institutions and regions that never would have otherwise occurred. I think overall in the two networks we probably supported and networked some 36 postdoctoral fellows. It was a very important part of what we spent our money on, including an ongoing mentoring environment. And many of those network people are now stars in the developmental sciences, or are becoming stars at this point. Many others, young people that participated in our network who weren't postdoctoral fellows have also, I think, been influenced to do collaborative multidisciplinary work and are involved in creative high-risk high-gain ventures that aren't going to have the immediate payoffs. We emphasized throughout our network the value of longitudinal study, with several major longitudinal studies launched and leveraged by the MacArthur networks, so most of the young people got imbued with the value of longitudinal work.

Haith: Can I come back to—what you had started with—some of your activities in policy-making groups?

Emde: Yeah. Over the years, I've served on a number of national committees and commissions. One early one was on the social and behavioral sciences for NIMH under John Clausen's leadership, in which we really came out strongly for support of longitudinal research. I think it may have had some influence. That was about twenty years ago. Then there was another one. Earlier in this interview I was alluding to committees that were advisory to the Director of NIMH, scientific advisory committees, and I served on these. These were just to generate advice and the directors would give them tasks. I served on these during a time of transition, so I caught three different directors; the end of Pardes tenure and the interval with the two different acting directors, and then the beginning of the directorship of Lou Judd. So it was kind of interesting to be in the struggle to keep research related to behavior at a high priority. There was such a rush to "the decade of the brain" that emerged during that time. I think in part this responded to the awareness of a vacuum; the Neurological Institute didn't go into areas where they could have, and so the NIMH rushed into that vacuum.

More recently I've been involved in efforts to advocate for integrating the developmental sciences and behavior with the emerging neurosciences. Last year, after a year or more of planning in which I was on the planning committee, we had a meeting specifically designed to integrate the neurosciences and the social and behavioral sciences. The first meeting was organized around topically the topic of developmental plasticity, and it was an exciting meeting. How much this will turn things around, influence them, I don't know, but the people at the extramural program are very invested in moving this along. I'm a little skeptical about how much this is going to do, but there are some terrific and influential neuroscientists on this committee. So we'll see.

I've also been getting involved in a prevention group as more of our research has been moving over to that area. I was asked to be on the steering committee to plan the Fifth NIMH Conference on Prevention Research, and I actually was quite active in planning. The conference allegedly was different than their previous ones and was quite exciting. They've brought along a group of young people, who are calling themselves "prevention scientists." Now NIMH is getting reorganized again and the new young generation of prevention scientists are concerned. These are young investigators in this area who have been brought along and socializing and just getting up and getting careers started. I don't know how that's all going to play out, because I think what had been getting some momentum is the prevention science enterprise, largely organized around primary prevention. As a developmental psychiatrist I would say there are important developmental processes that keep recurring throughout the course of disease and after recovery which need to be addressed in preventive and promoting activities—beyond primary prevention.

Haith: We're now picking up on this interview after several months of delay, and hope that we're going where we're supposed to be going here. This is now another year. Bob, I'd like to sort of continue by talking about something I know has been a lot of gratification for you, and that is your role as a trainer and nurturer of students and just some of the highlights of your experiences. And then I guess at the more formal level, I'd like to hear about the courses that you've taught and what you have experienced in terms of tension between teaching and research if there has been any, or whether you've seen them to be more mutually reinforcing of one another.

Emde: I think for someone in psychiatry and in a medical school, the tension is to restate the issue- more between clinical service and research for trainees, rather than a tension between teaching and research. There's enormous pressure and it's increased over the time of my career, to do clinical practice. People in medical schools now have to bring in their own salaries for the most part. I think overall there's less government support for research careers, although I suppose that's arguable. It seems to me harder to begin now. When I began I was quite fortunate, I got a Research Career Development Award, and you know, was supported throughout the majority of my career up until last year in that program, which was an enormous help, in offsetting the tension. I think those awards are a less available now. But in any event, even when people will have those career awards, they're now called K Awards of various kinds, I have to spend a lot of time helping awardees in hardening their resolve to protect their research time, because the pressure for clinical work is enormous. I do a lot of blunt work with my mentees to teach them how to protect their research time, assuming they really want to pursue a research effort programmatically. So we do spend time working at that. Young people are less fortunate today than I was in terms of being protected from that. So they need to make their own decisions and then stick with it, and there are a lot of tricks that we talk about in doing that. I try and at the beginning use the word leisure. And I say, "You need leisure time to do research, not just time when you're collecting data and at the computer, you need leisure time." And I use that word because otherwise they feel guilty if they're not doing something that is essential and scheduled, and somebody calls to do another clinical case and they do it. So by 'leisure time' I mean time when they can do scholarly activities and then go to the library or now do computer-literature research, to do a review, to think, to write; otherwise there's no time for that in a sixty-hour plus week. So that's a change and something I feel strongly about.

It's interesting at the other end, to think of the positive incentives for doing research particularly in the clinical medical school world. I've often actually used a phrase from you, that you said, perhaps twenty years ago, that at times this work is really incredibly tedious and at times you don't seem like you’re getting anywhere, you don't seem like your work is being acknowledged or appreciated. And you can think of other people who are doing other things that maybe they're getting more money for. It's been quite true in the clinical area, and it used to be more so than it is right now. The discrepancy between private practice and academic work in medical schools is a bit less so now because managed care has pushed down practice remunerations and also increased the paperwork, but there still is a discrepancy. In the face of all this, you said, "One of the things about this life, about doing programmatic research much of the time, is that somebody is paying me to satisfy my curiosity. That’s incredible!" I remember your saying that, and so I've quoted you and internalized it. It is a picker-upper at times. Overall, I think that developmentalists are optimists and developmental research is particularly appealing. For me, it’s not just because it’s interdisciplinary and open and you’re always learning new perspectives from other people, but it’s also that the people who are involved in developmental work tend to be optimists. There's a variety of styles of people working from romantic to tragic in the literary sense, but overall people interested in development are future-oriented--to make use of another influence of yours--and I think those are nice people to be around. They're thinking not just of themselves but of the next generation. And I tell students all of that. They catch that spirit.

And we talk a lot about surprise. One of the attitudes I like to inculcate in my trainees is a capacity for pleasure and surprise, because you’re more often surprised in your research by an unexpected result than you are gratified by confirming a hypothesis. If you have delight in surprise you’re better off. I do love theory, and I wish more of my research were precise in setting up opposing theories neatly with competing hypotheses, but I must admit a lot of my research has been exploratory, and I find a special excitement and adventure in that. But I do mix it up with people like yourself, and Joe Campos, colleagues who are tightly experimentally gifted, and that's thrilling when it works out. So, I've been pleased with my collaborators and I teach that to mentees—how to collaborate, and how to network. I get them asking questions of senior people early in their careers, and repeatedly. And that process is made easier now with the Internet, although courtesies are important.

You ask about the courses I've taught. I did teach the medical student course for a number of years regarding development, normal behavioral development. I taught that course for maybe a dozen or more years. I then decided it needed to be reorganized for beginning medical students that-- and I found someone at Down State Medical School in NY who was a model for me, Dick Simons, and I visited him to begin reorganizing my course with more clinical applications built into it. Then later as things happened, Dick was recruited to come to Colorado and bring his course here, which was taught much better than I had done. He was able, first of all to combine a number of different courses, in fact all of the pre-clinical courses in psychiatry into one course. The course was developmentally oriented, but it included the whole spectrum of disorders through the lifespan. It included more concentrated time, with regular small groups, as well as a series of lectures and discussions, and it integrated many of the basic behavioral sciences for the medical students in a way that pointed to clinical work for them in a practical way. When I was teaching the course, it was a lecture course that had much more limited time, one hour throughout one year focused on normal development, and it was often challenging to get the students interested in that. Many were, and we did fun things, but it wasn't as good.

My best experiences in teaching? I've taught a variety of seminars in the Psychoanalytic Institute as well, some having to do with development, some having to do with other aspects of clinical psychoanalytic and theoretical work. I’ve conducted a variety of seminars over the years as well in adult and child psychiatry. What occurs to me among the best were those in family therapy courses. I actually established a family therapy training course in our psychiatry department after getting some training in London, in Mike Rutter's shop, in family therapy training. But the very best experiences I've had in teaching have been in an apprenticeship model you might say, with people working in my lab. And I've had students at varying levels, from undergraduates here at D.U. and other places, to graduate students in psychology, social psychology, clinical psychology and some other interdisciplinary studies. I've had medical students working in my lab for major and smaller pieces of time, and postdoctoral fellows, both M.D.'s and Ph D's. And that's been enormously satisfying, because there have been a number of careers that took off after working in our group. We have even had parents participating in our study who then went on for academic and clinical careers afterwards. Many trainees in our program of Early Developmental Studies have had success in programmatic research afterwards as well as in some administrative leadership careers. That's been very satisfying, and I think that's where I've taught best actually.

Haith: Bob, I'd kind of like to ask you a few more questions here. You're being a little modest in your treatment of your role in training. I know it's been tremendously important for you to have taken the role you've taken in developing younger people, setting up programs for them, doing it directly. You've had several mentorship's of Career Development Award people. Could you talk just for a minute about what that's meant for you?

Emde: Well, right now, I've just written the first draft of our renewal for our endowment fund for our developmental psychobiology research group.

Haith: From the Grant Foundation?

Emde: Originally got that money came from the Grant Foundation. I think I talked about that earlier.

Haith: You have, but you haven't talked about specific people and, you know, the roles they've gone on to play in the field; for example, Bob Harmon-–

Emde: Yes, he was one of my early mentees. Bob Harmon actually worked a third of his time in medical school with me in research. Bob then had a Research Career Developmental Award and has continued research in neonatal and perinatal activities, and with behavioral research and a variety of clinical activities in child psychology, and also the head of child psychiatry in our medical school. He is also currently the head of the certification Boards for the American Academy of Child and Adolescence Psychiatry, and has been a leader in a number of areas—in residency review and editorial work.

Haith: And David Mrazek..

And David Mrazek. I was a sponsor for two Career Awards. David now has an endowed chair and is head of the Department of Psychiatry at George Washington University. He has done some fundamental research on genetic vulnerability to asthma, as well as early childhood asthma and social emotional development. He continues work in epidemiology and, in fact, he's going to be developing a program at George Washington connected with a major endowment they just got for a genetics center at that frontier of psychiatry and preventive work.

Robert Clyman was a postdoctoral fellow of mine, has also been successful, as one of the pioneers in health services developmental research. He has just been recruited to be the new head of the Henry Kempe National Center for Child Abuse and is in the process of moving here to Denver right now. And he's also got an endowed chair; it's currently the only endowed chair so far that relates to psychiatry in our medical school. So he's got an endowed chair, and Mrazek has an endowed chair. Hill Goldsmith? I was not a mentor for him, but I guess I was sort of a collaborating mentor with Joe Campos who was his primary mentor. Hill was a postdoctoral fellow with Joe and in our DPRG and has gone on to being one of the major leaders in developmental behavioral genetics. And I think he may have just gotten a Distinguished Professorship Award at the University of Wisconsin. So who else? Ann Easterbrooks who did a postdoc with me is now a professor of psychology at Tufts and doing family-oriented research in early socio-emotional development. Susan Warren who did a postdoc with me is another young and rising star in child psychiatry, doing representational narrative research in relation to development of internalizing disorders, particularly anxiety spectrum disorders in young children. David Oppenheim, another former postdoctoral fellow of mine, just got a tenure faculty position at University of Haifa and is a superb researcher doing wonderful work in early attachment and its clinical implications, again using story-stem narratives, which we developed in our MacArthur group, an assessment technique that has promise of accessing the internal world of preschool children. Zeynep Biringen, another former postdoctoral fellow, now has a research and clinical career. She's now at Colorado State University and is becoming a leading researcher in attachment field related to emotion regulation, developing emotional availability scales, with international use. So there are others, but this seems a bit much…

Haith: I know ….

Emde: -- historians can look at my CV where I list trainees…

Haith: Right. I just wanted to hear you talk a little because you have played a big role.

Emde: And the DPRG has been a major source of pride, as it has for you, where we can see how it has enabled the young researchers in their careers, and I think it's very exciting. -

Haith: Well, then there's a last question for me, and maybe we've covered it, but maybe not. Twenty, thirty years from now when we're dead and gone and you look back from the grave, what would you like to be remembered for? What were the high points in your career and what do you think that you have contributed to the field and take most joy in?

Emde: The most pleasure I've had is the collaborative work across disciplines, countries, and cultures and in conveying that excitement to students who go on to do programmatic research. Hopefully I've brought some vision to bear in terms of new configurations of our fields for training and research. I've never liked being boxed in, and I've never felt boxed in. I've been fortunate in not having to feel that way. I've argued for a lifespan developmental psychology my whole career, I believe in that. I think our medical and developmental disciplines are going to be totally reconfigured; they need to be. And I'm now on a committee of The National Research Council, in the Institute of Medicine, that's going to look at that hopefully as it reviews knowledge from the sciences of early development relevant for policy-makers—an effort organized by Jack Shonkoff. I hope in that effort we'll be able to look at these new configurations for training and disciplinary work. I hope that I've had some vision in that and also in psychoanalysis, where I've been involved in fostering psychoanalytic training for empirical research and developmental research, of course, in particular. I've also contributed to psychoanalytic theory in the areas of expanding its developmental orientation and knowledge base. I'm giving another invited plenary address at our world congress of the International Psychoanalytic Association this summer in Chile, and I'm talking about integrative aspects of developmental processes. I'm bringing in neurobiology, and a lot of systems thinking and I'm anticipating that a lot of psychoanalysts are just going to say, "This doesn't have anything to do with psychoanalysis," you know. "How dare you!" --especially since I don't come up with any easy road for going forward. But I enjoy that, I enjoy being irreverent in that sense, and it's been satisfying because enough of that has stuck. And I believe that some of the theoretical stuff I published ten or twenty years ago is now entering into some of the canon of psychoanalytic training institutes for reading. That's kind of nice. I've had a lot of pleasure in this work and that's been its own reward. I see myself driven less to write a lot of books, although I've still got three I'm in the middle of with others. Perhaps I will do a book on my own. I'd like to do that. There's always lots more to do, than I can do, but I don't feel frustrated in any overall sense with my career. There has been a lot of pleasure in it. And the main pleasure has been with students and especially with colleagues like yourself. You know, it's been a good road.

Haith: And you've had some great collaborations with Joe Campos-

Emde: Absolutely.

Haith: - Goldsmith and I can't think of the fellow in Germany.

Emde: Klaus Scherer, we've worked with, among others.

Haith: Yeah, Klaus Scherer, right. That's it.

Emde: I could spend a tape listing my collaborators. They appear in the publications on my CV. But I suspect this interview has been too long, I apologize to the transcriber.

Haith: Well, I think it's important to get down. You've had a very rich career. Now, we're bringing this to a conclusion.

Emde: Hail and farewell!

Robert Emde – Mentors, Colleagues, Students
Mentioned in the interview

Mentors
Ren Spitz
Yvonne Brackbill
Lou Sander
Gerald Stechler
Peter Wolf
Jerome Kagan
René Spitz
Richard Bell
Yvonne Brackbill
Marian Radke Yarrow
Lee Yarrow
Jack Cohen
Nicolas Plotnikoff
Ernest Gruenberg
David Metcalf

Colleagues
Paul Polak
Marshall Haith
Lewis Lipsitt
Michael Lewis
Bob Wallerstein
Lou Sander
Dick Bell
Bill Hall
Frances Graham
Wendell Jeffrey
Frances Horowitz
Inge Bretherton
Everett Waters
Mavis Hetherington
Susan Summerville
Willard Hartup
Peter Hobson
Glen Elder
John Benjamin
Paul Polak
Jack Gewirtz
Anthony Ambrose
Kay Tennis
Tony Kisley
Sid Workman
Charles Kaufman
Joe Campos
Herbert Pardes
Herbert Gaskill
Charles Meredith
Phil Sapir
John Conger
J. Steven Reznick
Nancy Snidman
Katherine Barnard
Joy Osofsky
Leila Beckwith
Dick Simons
Marshall Haith
Klaus Scherer

Students
Bob Harmon
David Mrazek
Robert Clyman
Hill Goldsmith
Ann Easterbrooks
Susan Warren
David Oppenheim
Zeynep Biringen