

Philip Roman Zelazo

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SRCD ORAL HISTORY INTERVIEW

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Interviewed by Thomas C. Dalton
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Dalton: Okay. I'm here with Philip R. Zelazo, McGill University. We're conducting an oral history interview for the Society for Research and Child Development. It's Friday, March 30th. We're in Boston, Massachusetts attending the SRCD biannual meeting. Let me start here, Phil, to get this line of questions started in some coherent fashion. At your request, we have arranged an interview today during the 2007 SRCD meeting, in which we'll focus on the influences and development of your neonatal studies of infant locomotion and unaided walking that were based on the remarkable pioneering studies of infant motor development in the 1930s. As you know, I have written extensively about infant experimentalist Myrtle McGraw's research and documented her profoundly significant collaboration with the philosopher John Dewey during the 1930s and 1940s. She evidently attracted a lot of admirers and I'm particularly intrigued by how you were drawn to her work at the beginning of your career as a developmental psychologist. How and when did you become acquainted with her work, what aspects of her research did you find particularly provocative and how did it influence your research agenda?

Zelazo: A tall order. I became interested in Myrtle McGraw because of my collaboration with my wife. She was a pediatric nurse, specialized in maternal and child health, knew the reflexive repertoire of the baby better than most psychologists and I became fascinated with the stepping response and the neurobehavioral repertoire of the baby. In our case, that baby was our son, Philip David, who is now a developmental psychologist. This was a parallel line of interest that I think she stimulated mostly, simply because of our compatible and complementary interest in early child development. But I had been pursuing independently, for my own doctoral research, a study of smiling and vocalizing in three to four month old babies. That's a whole parallel line of thinking, but there I at least had the guidance of Richard Walters, who was my dissertation advisor. He and Ross Parke published a nice paper on the role of the distance receptors in infancy. I was fascinated by that and transferred to the University of Waterloo in Ontario and began to study with them. The problem was that he didn't have--or the advantage was, however you see

it--that he didn't have experience with babies. They wrote this very interesting paper that seemed to be at the opposite end of the assumptions about early motor development, the role of the distance receptors, yet I became, in parallel, fascinated by early motor development as well. And during that era there was no research on early motor development. It had faded away, the work sort of died out with probably McGraw's own work in the early '40s, maybe mid '40s and that seemed to be the end of it. And during the late--mid and late--'60s there was a resurgence of interest in infancy, but that was focused on the perceptual cognitive aspects of early development. So, from the start, I was interested in this new area of infancy, because of its theoretical underpinnings in early experience and there was a resurgence of that research at that time—broadly speaking—early experience and brain-mind based work, as well as research on the role of learning in perceptual development. That was fascinating and, for me, compatible with my own interest at the time. This resurgence, to me, played out in the resurging interest in early experience, early cognitive development, the decline of behaviorism, you know, strict behaviorism at the time. It was just very exciting intellectually and a very good time and it converged for me in infancy, human infancy for many reasons, not the least of which, as I said, was my collaboration with my wife, Nancy Zelazo. So as this parallel work began I had no guidance really on an empirical level, but to me, given my nature, that was a good process and I could just pursue the field without biases, and that I did. And so when I looked for the background in terms of early motor development the work that stood out, of course, was McGraw's empirical work. So I went to McGraw for guidance in motor development and, in parallel, to Harriet Rheingold from North Carolina for guidance in the study of vision and audition or at least vocalization and smiling and the way it translated in terms of the work on the baby. It turns out that Myrtle McGraw and Harriet Rheingold seemed to have the best empirical work, and so I taught myself basically empirically and used their guidance theoretically and methodologically. So that's how I got started I suppose.

The stepping work had interesting parallels because there was a French neurologist by the name of André Thomas who had made two very interesting observations that paralleled our work. One is that he actually did a trial of stepping with a parent who had brought the problem to his attention, the problem of training the stepping response or the effect of exercise on the stepping response and he reported that in an obscure paper published in French. He also made reference in a paper about how the baby, if you stood the child up vertically, would react with his whole body in the direction of a sound. And that intrigued me at the same time. Later I was looking to study auditory information processing in the newborn. And I tried to simulate or create a more rigorous paradigm to study that. I looked to some extent to Brazelton, who held the baby in his hands and seemed to be doing a similar sort of thing. It was the same response. It was the same reaction to sound but he conducted the experiment while recording the behavior and holding the baby in his hand, so the procedure was hopelessly confounded. But fortunately Darwin Muir had worked out a very nice procedure independently and had derived a procedure that took the Brazelton head turning (instead of body leaning as André Thomas had) and put it in a paradigm that was somewhere in between Brazelton's loose dangling of the baby to auditory stimulation and a methodologically rigid component and put it into an objectively tighter framework. That framework, however, was only to study auditory orienting to sound and eventually, when I learned that Darwin Muir was creating just such a thing, I used it to create an habituation-recovery procedure so that we could look at the creation of mental representations in babies and use a clearly objective head turning response. That work has panned out very beautifully in the sense of having many replicable components and a whole body of studies that led to really good insights about the newborn's capacity to create mental representations of auditory information and how they respond to the repetition and change of that information.

Both speaking at once

Zelazo: --if you would like me just to continue right on. Because what that did was that body of work created some of the fundamental properties that we would later--we meaning my son later in life and I--would rely on to characterize what the infant's consciousness--the mental world of the baby--might be like during the first year of life.

Dalton: And many of the people that you mention, Phil, Brazelton and André Thomas and others were also influenced by McGraw's work. They were, in fact, carrying that forward so that the paradigm was being restored in their hands, their capable hands, and you also were working from that basis. So we could say that the thread was carried through, but it was not--it was just a few people, but it was very important research.

Zelazo: Yes. But clearly that work was solid and it was the stuff that we gravitated toward as we started sifting through that work on early neuromotor development down to seeing that McGraw's work was among the best. Since you asked what of that work appealed to me the most--

Dalton: Yes.

Zelazo: -What aspect did I find particularly provocative? Initially, it was the learning aspect. As you well know, there was no room in theory or in the research of early motor development, aside from McGraw's work on the twins, Johnny and Jimmy, that really allowed for learning to have a role in motor development. And this was a note and a thread that was consistent throughout the early experience work by Hebb, who showed that we can learn to learn, or Melzack's work on learning about pain. And that work was going on at McGill largely. To me, that was fascinating and appealing and just intuitively correct and exciting, because then, like now to some extent, there was too much appeal to some people about a nativist perspective on early development. And I think that that unfortunately caused some confusion about brain development. As you know, the neurosciences have shown quite clearly that we even wire our own brains, and not just rewire them, but I'm positive in terms of early development that we wire them. So there's a good deal of interaction going on here that's bi-directional and both nature and nurture. So nurture was left out of the equation, and nature was a given at that period of time, and so it did appeal to me to study and to push the role of learning and experience in early development.

Dalton: And how did you feel about how your colleagues received your work given the fact that you were coming out of this paradigm that had been ignored for a while and was resurrected? What was your perception of the way your colleagues received your work, your early work in this regard?

Zelazo: Well, it's interesting, because it got an enormous amount of attention when I--we published a paper on the first study in *Science* on this early stepping, taking the stepping response and showing that training or exercising it actually increased the response. It didn't just maintain the reflexive response as we thought it might, but actually increased the responsiveness so that children stepped more. The more they stepped the further they stepped as with a positive feedback system. That paper in *Science* got initial criticism, heavy criticism in fact, because it's a renown forum and interdisciplinary. And so we defended that and responded to it, but being young this was a bit of a shock. It was obviously received well by some or it would not have gotten published, because *Science* is incredibly difficult to get into. It must have an enormous impact in addition to being just impeccable work. So it met those standards, but the reaction was clearly split out there and there were people who challenged it on a whole host of grounds and we responded to those challenges. I guess the best compliment I got was sometime shortly after it was published and I was leaving Harvard. At the time there were three of us who were departing and they had a tradition on the 15th floor of having sort of a gathering and send off and a bit of a retirement or departure celebration—a wine and cheese with everybody gathering in the department. One of the other people leaving at the time, at least resigning, retiring officially before becoming emeritus, was B.F. Skinner, who had just seen the publication and was very complimentary. He was aware of it, so--at the time his awareness was complimentary and it was pleasing to see that he had noticed it and enjoyed it. And so the response is mixed. You might be able to suggest something on it being an historian whether you know of any of the reactions.

Dalton: You know, we have done studies and I think that you have gotten a positive reaction, and it's gotten more positive over time. I think that's the way to look at it. And of course, then there's always been differences of opinion among other developmental scientists about how to interpret McGraw's work in the context of more recent studies. But I think that your studies also do stand alone in many ways. I think that, yes, McGraw's important, but I think that you have taken the state of the art in science further and I think that's really what has been noticed by your colleagues. That would be my impression in looking at the history I've looked at. So I think that we are here to talk about McGraw's connection, but I think that obviously you have improved her paradigm. Her paradigm was very large, and yesterday William Damon, in the symposium I had chaired, ran up the flag for McGraw and said that her work was still unparalleled today, and I think that's a little exaggerated. I think that she did see far into the future in terms of brain behavior and mind relationships, but certainly she did not possess the knowledge that we have today of the brain and how it works. And I think that that makes a difference about how we think about people historically, that their contribution was real important in pushing forward a lot of inquiry that should have gone forward, and I think that's the way to look at that. But let me ask you this. Did you ever meet Myrtle McGraw personally?

Zelazo: No, but I did exchange some letters with her and that was fascinating and pleasing to me, because I guess I hadn't at that point thought of contacting her. But she did and it was just very pleasant and I was pleased. And I think the main point there--

Dalton: She contacted you first?

Zelazo: She sent a letter and--

Dalton: In the early '70s?

Zelazo: Yeah, I think that--yeah. And I think the bottom line was that she, in the letter, made some reference to the fact that the twins that she studied, Jimmy and Johnny, were not identical. So then she felt she wasn't sure whether Johnny's advantage was purely experiential. So looking back, I mean, I'm always fascinated by that work, because it's so unambiguously early training, and unambiguously successful. We tried to address this question in a recent paper I just published with Michael Weiss on infant swimming, which is something that she studied almost solely. I think it's the only known study on infant swimming that I know of, and that investigates general cognition and development over the first year. We try to point out when we look at this question of this apparent contradiction in her publication about whether she falls into the nature or nurture camp. I think there's evidence of a bi-directional thing going on here and she's studying development. She's just not in one or the other camp. But she clearly came across to some people, I suppose, as a nativist and not encouraging the role of learning in early motor development. Yet the work between Johnny and Jimmy was just unambiguous in my opinion. So we looked at some of same kinds of questions using swimming as the behavior where we could train the responses. Yet, she didn't do much. She did with Johnny but the intervention research lost out to the larger samples I think in terms of history, to the larger samples in observation and the biases that prevailed at the time. This is where I said we started, that the view was simply that children developed and if it's simply unfolding of the genes and that's it then it's game over. There is nothing to ask about genetics. It is a boring kind of question and still prevails. It prevailed then. So it plays itself out and Esther Thelen was staunchly opposed to its display and prevalence in terms of early motor development or perceptual development.

Dalton: In your studies of autism it obviously is extremely helpful to think that we know what normal motor development is so we can spot the signs of abnormal development as indicators of some brain based disorder. Is that a correct way of looking at this?

Zelazo: Yes. There's a developmental psychopathology perspective that is important to take. And that's not the prevailing view. Again, it's a separate story in many ways, the whole issue of contemporary thinking in autism research. But essentially I think that the majority of people hold the same assumption that they held about early motor development in the '60s and '70s, that is that there is no room for learning, for experience even, that this is genetic unfolding. Except now that there is some good basic research, and I think some good people are beginning to acknowledge that there may be a genetic component, but genes need not be expressed. The work by Michael Meany, I think, addresses that question very powerfully. So hopefully this area of autism is going to open up a bit to outside views, but unfortunately the developmental psychopathology perspective of autism is an outside view presently for two reasons. One is that developmental psychopathology is a new discipline, it's only about 20 years old or so, 30 maybe, but that's relatively young as disciplines go. And for some unknown reason autism has not been studied well from that perspective. That's where I think our work is going to eventually have an impact. But it doesn't have any at this moment. So there really is a strong bias toward viewing autism as a biological phenomenon that simply unfolds. We have some very interesting work to challenge that. We had, for example, the majority of children with autism who were not talking ...to talk. Usually, 35-45% of the children with autism end up not talking. We can get many of them to catch up intellectually, about 75% of the children in our studies. Usually 75% of the children with autism end up mentally retarded. I think that's acquired mental retardation to a large extent, not total, and that that's going to be shown over the years. So I think there are many, many points here. We can routinely get children who are not using objects appropriately to use them appropriately. This is at the Montreal Autism Center that I set up with Erwin Newmark and Caroline Reid. So these kinds of things are not accepted yet, but if you can make these kinds of profound changes in a child and those characteristics are eliminated, the characteristics of autism no longer exist, then need you hang onto the label? Or more fundamentally, what is this thing called autism? So I think it's very exciting that autism is a problem that developmental psychopathology can address and make a major contribution to. And not only that, but that it can show the role of developmental psychology in the acquisition and treatment, in the

etiology and treatment of autism. I don't think there has been much receptivity to that point of view, unfortunately, not even by psychologists, who seem to constantly go on the assumption that this is entirely a biological phenomenon. And I think from an intellectual perspective that one should at least entertain the possibility that it is an acquired phenomenon, not necessarily without a biological basis or some kind of biological component, but that it's not solely a biological phenomenon.

Dalton: But it creates a self reinforcing source of behaviors that then further damage the brain that may have been damaged to some extent with poor connections and makes them even poorer. You can think of it that way.

Zelazo: Exactly. Remember, I started off saying that the research shows today in the fundamental neurosciences that we are wiring and rewiring our brain with experience. But more fundamentally, I think that we have some work that addresses the development of the lateralized readiness potential for the stepping response. I think the children wire their brain early on. Conceptually you can see that easily. Nobody can talk for the child. Nobody can walk for the child. And you will not walk unless you try to walk. I suspect that that's where we are fine-tuning our balance. It's not easy to demonstrate that, but with a new MRI technique perhaps that will be possible eventually to demonstrate.

Dalton: Well, one of the questions about McGraw, McGraw clearly had an interest in mind/brain behavior relationships. What do you find most useful or compelling regarding her ideas about infant awareness and perception? And this question leads into another aspect of her work that's important--

Zelazo: Yes. It's not just the role of maturation in early development; it's the role of experience or learning in early motor development. So I think that's really a key thing and she did not get credit for that initially, even though she had the first demonstration, clear demonstration, of the role of experience in early motor development. The other really intriguing points for me and exciting questions that she raised and, as you pointed out, not just alone, but with John Dewey and an incredibly accomplished team of advisors (that's a whole separate and exciting phenomenon to discuss). But I think one of the major contributions to come out of that collaboration is the change that occurs at the end of the first year of life. And she wrote a paper that really struck me, a paper that seemed to be titled *From Reflexive to Instrumental Control*, and I lose sight of that, because I also had a paper that used that title and I'm not sure if I'm characterizing the work on that particular paper or whether it captures the bigger issues that she raised? Probably the bigger issues that she raised, but that early motor behavior is reflexive to a large extent, but something profound happens at the end of the first year and she talked about that, as you know, with respect to the development of judgment. That judgment is something we tried to nail down, that is, what do you mean by judgment, and what is at the heart of the cognitive mechanism that's kicking in, and how does the behavior change, and how does the child's information processing change at the end of the first year? And that's what we tried to address in a series of papers on the cognitive metamorphosis at the end of the first year. Two main points were addressed, one in the first paper and one in the second paper on this topic. It was entitled *The Dawn of Active Thought*, and a debt has to be paid to Jerry Kagan as well. I zeroed in on that phenomenon. But even earlier than Kagan, it is in Myrtle McGraw's work citing the emergence of deliberation, the development of intention may be a better way to express it, the deliberateness of the behavior at the end of the first year. And she described that clearly in words, not necessarily in powerful data. She's got primarily observational data and it's a difficult problem to demonstrate in character other than with broad strokes but she got that in broad strokes. There is a deliberate nature about the infant's behavior at the end of the first year. Kagan referred to it as hypothesis activation. Elizabeth Leonard and I called it The Dawn of Active Thought. This transition begins at around eight or nine months. I published a paper before that on the year old infant in a book edited by Tom Bever where I compiled data to say it's not until 12 months that the process is complete. And it's, to me, more sound to discuss the empirical data, the solid changes and compile a whole bunch of these things that show that the first words are spoken on the conventional scales at 12 months; first steps are taken on the conventional scales at 12 months. We looked at separation distress with Milt Kotelchuck. People talk about it coming in earlier and all but it's not until 12 months that the child declares itself, screams or turns or crawls away or runs away. There is a clear active reaction that declares the response at 12 months. At seven or eight months it's wariness; it's an ambiguous response. It is alertness. But the understanding, the interpretation, is not declared. It's not really consistently declared until about 12 months.

I conducted one study with Richard Kearsley in which we examined functional play. The point there was to show that the motoric demands of functional behavior, which can be regarded as tool use--that functional behavior--the motoric demands are the same to pick up the telephone and put it in your mouth as they are to pick it up and put to

your ear. It's not a motor change that is occurring here; it is a cognitive change. Right? The child is conveying the meaning of your world when he picks up the phone and puts it to his ear rather than to his mouth. In this study we used a handful of children at each of four ages: nine and a half, eleven and a half, thirteen and a half and fifteen and a half months. At nine and a half months 89% of the interactions with the toys consisted of stereotypical behavior: mouthing, waving, fingering, and banging the objects. It wasn't until eleven and a half months that all of the children displayed at least one, often more, functional acts. Right? So there is really a cognitive metamorphosis that occurs and there's all these profound changes, protodeclarative pointing, talking, unaided walking, social referencing, a whole gamut of things that come in at the end of the first year. And paradoxically it involves both specificity and diversity of behavior. The real profound question is what's changing?

Dalton: And behavior takes on meaning and significance?

Zelazo: Yeah.

Dalton: It's not just behaving, reacting, it takes on intentional significance.

Zelazo: It's intentional exactly as Myrtle McGraw and John Dewey hypothesized, right?

Dalton: Yeah.

Zelazo: And I think that single, simple observational study illustrates the cognitive change most clearly. Interestingly, almost all the research that I have done has been experimental with the exception of that study. And it's so unbelievably clear and reliable and implies to me that there is tight genetic programming of the behaviors within the first year until this capacity emerges. The other fascinating part of that is it's analogous--or at least there's some credibility to the abused and misused idea of ontogeny recapitulating phylogeny. That is, if you take the essence of that statement, the essence of the changes at the end of the first year—that transition—what you have is the baby taking his first steps, using his first words and using tools. These have been characteristics that have defined our species in the past. And in a sense in each baby you see ontogeny recapitulating phylogeny. These important changes occur in development, not in terms of gills and fins and those kinds of outrageous notions that were raised but in terms of the essence of the cognitive capabilities.

Dalton: Well, let me move on from these interesting questions and discuss--we're gonna circle around and come back to this topic of consciousness. But I would like to briefly pursue another line of questions having to do with the resurrection of McGraw's life and career that began in the late 1970s with a renewed interest in reviews about early experience and then culminating in her recognition a key figure in John Dewey's attempt to discover the pattern of inquiry in early developmental processes. As it turns out you and many other developmental scientists showed a simultaneous interest in McGraw's work during the 1970s that included Gilbert Gottlieb, Burt Touwen, Lewis Lipsett, Esther Thelen and many others. Could you comment on your contributions and suggest why interest was stirred at this particular point in time? Was it because experience was being accorded a more important role and perhaps because new methods became available to investigate the relationship between motor and perceptual development with more specificity and detail? In essence, did a renewed interest of the historical context and significance of McGraw's work play a role here too? I'm thinking of the historical research published by Gilbert Gottlieb, Lewis Lipsett, myself, Bert Touwen and others. Could you comment on that? Why this particular topic? Was it really that science was advancing and that made it possible to revisit these issues or it was a matter of it still being of intrinsic interest to scientists to pursue these issues?

Zelazo: Yeah. I think it's hard to pin down exactly what the influence was and what specific influence carried the day, but there were a number of influences and that's what I would like to talk about. My impressions are that the inherent questions that she asked were interesting and they are intellectually compelling--something that you can't say about all substantive questions. I think that certainly plays a major role with the thinkers, and the scientists that you've described who are some of the better thinkers of that period, who also had an interest in early motor development. Another sort of political thing was that--I don't have the exact date--but in the '80s, I guess, we put on a conference in Montreal--the ISIS conference--International Conference on Infant Studies. But I remember doing a tiny amount to stimulate the area of motor development by giving it a separate section on the agenda and prior to that--because I was program chairman--and prior to that it didn't appear on any agenda. So for a small amount of

effort we put it literally on the agenda and did that to try to stimulate more interest in early motor development, because the work in infancy was primarily on perceptual cognitive development. But I have personally an interest in pursuing these lines in parallel, perceptual-cognitive or information processing and early motor development, for a whole variety of reasons. But early motor development was not being accorded enough attention, I thought. Esther Thelen was beginning her career, I think, at about that point and certainly individually and personally did an enormous amount to stimulate motor development after that point. But at least to some extent there was a conscious effort to try to bring it on the agenda, and I think that played its role as well.

The relationship between the two I think--well, there're two points. One is yes, the techniques improved, so now you're asking questions, here's a whole body of really exciting important questions that I wanted to follow and that I think were inadequately addressed or just left unanswered or didn't get completely answered or could certainly be revisited in light of the better procedures that we had available to us during the '80s and '90s as opposed to the '30s and '40s. The science had evolved and the standards were greater and more rigorous, not that the researchers were any better, but their tools had improved and our sophistication as psychologists had advanced. So all those things seemed to play an important role. The parallel, and I think she—McGraw herself—was always interested in that parallel development, the perceptual-cognitive as well as the motor, except doing that work we seemed to know only about the motor development and make inferences about cognition. That was certainly true in the creation of conventional tests of development, that is, creation of the standard tests of infant mental development. The work of Arnold Gesell was heavily genetic and biologically driven, right, the unfolding of the child, and that the motor response basically said it all.

I was interested in flipping that around and that's what was so interesting about the head turning study, that what we used was a profoundly clear motor response in a two-day-old baby. It could turn its head 90 degrees toward a sound. We controlled the information that was put into the baby's head and the discrepancy from what we put in had showed a whole host of things about how the baby functions. But one of the most important things that turned the process on its head is that this baby's motor response was dictated by the information, auditory information, that we put into its head, right, that we exposed the child to. So that, for example, in a study with Michael Weiss, we examined in his dissertation, the role of discrepancy. Is recovery of attention and responsiveness a linear or curvilinear function of the degree of discrepancy from the initial information? Right? So we oriented and habituated the baby to a particular fundamental frequency and then varied the fundamental frequency very systematically following habituation so that there was either no change, a seven percent change, fourteen percent, twenty one percent, or a twenty eight percent change. We were able to ask the two day old baby a question that had foiled researchers at the older ages—foiled them because you couldn't have a single metric to examine the nature of the function. And with the single metric—fundamental frequency—we were able to determine that there's a curvilinear pattern to recovery. That is, information at specifically fourteen and twenty one percent led to more recovery and redundant information or information that varied only by seven percent, or interestingly, twenty eight percent, did not. At the extreme discrepancy, the children oriented and then turned away. Another beautiful feature of the head turning response was that it was bi-directional so that we could see that babies behave at no change as though they were bored and they continued to not only not turn, and therefore show habituation, but because of the bi-directional nature of the response we showed that they weren't really habituating in the traditional sense. They were actively turning away, and the same is true at twenty eight percent discrepancy. So it's such a profoundly important paper because it shows that in the neonate two days of age, moderately discrepant information was most appealing and led to most attention, longer sustained attention, than redundant or very discrepant information. This is likely a characteristic of the organism at birth and I believe that.

I think that this process of optimal recovery to moderate discrepancy is, as Jerome Kagan says in one of his more recent books, *Surprise, Uncertainty and Mental Structures*, that discrepancy, moderate discrepancy, is the engine of change in man. He extended that from perceptual cognitive experiences to more symbolic kinds of experiences including language. So I believe that that's a really seminal characteristic of the human condition and he made that case very profoundly in that book. But this experiment does what you can't do easily at any other age, because the organism is too sophisticated and will habituate too quickly. But the two day old baby can attend to changes along a single dimension—allowing a single metric—without becoming excessively disinterested.

Dalton: Then how do you react to the people--there are developmental psychologists who argue that you can split apart motor and perceptual development, that the two can be disassociated and you could look at one without the other, especially perception? People argue we have these perceptual abilities, hidden abilities

right at birth, before birth. How do you react to that? Because it appears--what you're telling me and describing to me about discrepancy is that there has be a simultaneous aspect of behavior and recognition that goes on here in not just one or the other. What's your thought about that?

Zelazo: Yeah. I think that there are advantages to separating these things, but ultimately we have an integrated organism and that it--it can be misleading to artificially separate these things out. Because we can often forget about going back and reassembling the organism, and then we make generalizations based on a bisected organism basically. Our work on the head turning response does that very nicely. We--as I said, what we determined was that the neonate will automatically create mental representations of the stimuli that he or she experiences. We put the information in and control the degree of change from what the infant already knows—from what we just put in—and show that he or she is capable of being motivated either to approach or to avoid the discrepant stimulus. The new information influences his or her behavior based on the prior experiences that we experimentally manipulated.

Dalton: So we're embodied minds--a brain that's in a body that's in a world experiencing the world.

Zelazo: I like the embodied mind notion.

Dalton: --you know, talking about mind, there's one other area of interest that I wanted to pursue with you, a couple remaining areas of interest. And one area of mutual interest that I would like to pursue with you this afternoon that I believe holds great promise in shedding light on the relationship between mind/brain behavior, and that is the phenomenon of consciousness. You and your son, Philip David, are among a handful of development psychologists squarely confronting the issue of whether infants possess consciousness. Here also McGraw appears to have had an early influence in getting you started on this question. Is that correct?

Zelazo: I think it is. And in terms of McGraw's contribution on both counts. Yes, I'm interested in it with my son and he's profoundly interested in the study of consciousness, but I translated or tend to translate McGraw's work more concretely, which is also the way it's manifested in this paper that I did with my son on the emergence of consciousness. The work I described with head turning and with motor development really has very fascinating parallels. Some findings for cognitive and motor development more closely parallel each other than one would imagine, if you extract the essence of what the infant's mind is like and what is it capable of. And I started to say something about that earlier. The neonate can create mental representations, that can be recognized as stimuli that can be experienced, that can be approached, and be avoided but the neonate doesn't seem to have an awareness of itself, and certainly does not possess the capacity to recall events.

Break in sound

Zelazo: There's one other point that I would like to make. In my studies of the first year I relied heavily on the work of Les Cohen, who studied visual processing, but who came to a similar conclusion. That is, over the first year, you seem to get more of the same in terms of cognitive development. It's not so much that we suddenly make some dramatically different development during the first year. The baby embellishes what he or she has. Dramatic change in ability does not occur until the end of the first year where McGraw spoke of judgment occurring. Les Cohen's work, very careful, brilliant research on visual information processing—really comes to a similar conclusion and it has its parallel in motor development. You have, as the functional play study with Richard Kearsley showed, you have 89% of the toy contacts at nine and a half months consisting of the same behaviors—stereotypical, mouthing, waving, fingering and banging of the objects indiscriminately. It's not until two months later that the child starts to behave as if there was some awareness of the whole world as we see it. So he picks the telephone up, puts it to his ear and then babbles, puts the hat on his head, a whole host of things that are very specific hypotheses about the world as Jerry Kagan would say, or it's the dawn of active thought, where you start generating ideas and you can start thinking about things that happen.

In terms of work with my son, what we did was characterize the mental world of the infant. As I have just said, there's more of the same, during the first year, but it doesn't really diverge and become more specific, paradoxically, until towards the end of the first year. Then the phenomenon of recursive consciousness seems to kick in, which gives the child enough capacity, cognitive capacity, that I like to think is driven by increased speed of processing, that allows two signals to be present in one's awareness or consciousness at the same time, so that a new idea like a telephone to the ear rather than to the mouth can override the prepotent reflexive behavior of putting the object in

the mouth. So the idea reverberating in the infant's head along with the prepotent response provides the child with the capacity to execute that idea and frees himself from the reflexive repertoire that he has. That process plays out in dozens of ways and accounts for this capacity to be both specific—more specific—and more general rather than just doing the same thing in every situation. My son, Philip David, labeled the infant's mental state during the first year minimal consciousness but identification of the child's capacity was based on our work on head turning largely and other research like Les Cohen's over the first year. This work pretty much characterizes the safe conclusions that one could make about the baby's ability. Right? They're the minimal conclusions. They are the parsimonious conclusions that you can make about characterizing a child's world. And I think that they are valid and they're certainly based on very tight research. The change at the first year, as I said, is really significant to me and one that validates McGraw's most interesting contribution and it hadn't been given the attention or credit that it deserved—even today. There are a number of people who talk about social development and talk about profound changes at the end of the first year, but they're not crediting McGraw's early efforts to deal with this problem. So from that point of view, yes, I think history has an enormously important role and we should be respecting these contributions.

Dalton: You know, are developmental psychologists avoiding discussing--using the term consciousness in the work or is there more acceptance of that in your perception from a few years ago? There are some other researchers that are talking about consciousness, but I don't see a lot of people focusing on consciousness. Is it still a term that they consider too elusive or what is your thought on that?

Zelazo: Yeah, I think there still is a lot of resistance. It takes courage to talk about consciousness in a scientifically reasonable way. Well, that too is paradoxical, because it takes courage to talk about it in developmental psychology, but as you well know, there are people like Gerald Edelman and other Nobel Prize laureates, others like Roger Sperry and Francis Crick, who also talked about consciousness and studied it empirically. Consciousness is the next really significant scientific frontier. Why developmental psychologists should be shy about doing this, I don't know. But I think they still are. There's a little bit more acceptance, but not a whole lot.

Dalton: Because there are more neuroscientists that are talking about consciousness.

Zelazo: Yeah, exactly.

Dalton: Developmental psychologists--

Both speaking at once

Zelazo: Exactly, people like Herbert Jasper. You made reference to, I think, Jasper and look at Jasper. He had been pursuing the study of consciousness I think the majority of his life and produced one of the early phenomenal books compiling a group of prestigious neuroscientists as he seemed to be able to do with his own prestige.

Dalton: Were you personally familiar with Herbert Jaspers?

Zelazo: Yes, and with Donald Hebb, and Wilder Penfield, for example. I was fascinated by their research and theorizing at the time. There was a very impressive body of people doing that kind of work in the 1950's and 1960's. I was interested in early experience. It started way back then. I worked for some time with Eugene Bernard, who had studied with W. Grey Walter, who along with Herbert Jaspers is credited with the development of the EEG. He was a neurophysiologist, psychophysiological and sparked my interest in early experience, but also from a brain-based perspective. So I guess the interest was there early on, and I was familiar with and very impressed by *The Organization of Behavior*, Donald Hebb's book, and the people that he interacted with at McGill University. He was a very impressive neuroscientist at the time. That's one of the appealing features of Montreal and McGill in particular. There were just a number of very good people studying both psychology and medicine there.

Dalton: So you really had a critical mass of scientists there to do something important--

Zelazo: Oh yes.

Dalton: --in this area of consciousness. I guess Herbert Jaspers had led or helped organize conferences in the '50s through the late '70s on brain mechanisms and consciousness. And I think that the last conference that was really in that genre was one that you attended at McGill University, 1997 I think.

Zelazo: Yeah.

Dalton: I think it led to a publication of a book that Herbert Jaspers co-edited. I believe he was 90 years old at the time--

Zelazo: Yeah. He was.

Dalton: --or even older.

Zelazo: Yes. Jaspers assembled an outstanding cast of characters. They actually held the conference at the University of Montreal. Jaspers had moved from McGill University to the University of Montreal. He was at the Neurological Institute with Penfield at McGill University, but then set up this Mind Institute at the Université de Montréal, so that's where it was held. It was a fascinating, very impressive group of people, because Francis Crick's lab was represented, Gerald Edelman's lab, David Hubel was there and Michael Gazzaniga, who studied with Roger Sperry. We were the only psychologists, my son and I, so it was an honor to be included in such a high-powered interdisciplinary effort. It was incredibly stimulating.

Dalton: Yeah. So it looked like a very interesting meeting.

Zelazo: Yeah.

Dalton: Very interesting publication that came out of that conference with a wonderful little historical introduction from Herbert Jaspers--

Zelazo: Yeah.

Dalton: --that I appreciated. You know, I've done some research about this with Bernard Baars that led to the publication of a chapter in a book I co-edited with Rand Evans on the revival of the scientific study of consciousness. While doing the research I discovered in an autobiographical statement by Herbert Jaspers that his hero was John Dewey, the famous American philosopher and co-founder of pragmatism.

Both Speaking at once

Dalton: --that led him to be interested in studying the brain in relation to experience. And I thought I'd pass that on to you, because that stood out.

Zelazo: Yeah. The other thing is too that he was from the U.S. even though he spent most of his time in—or much of his time—in Canada and Montreal in particular, he was American. I am from the U.S., but I've spent the last 23 years in Montreal.

Dalton: Okay. Well, I think we've really covered the spectrum here. In talking about the study of consciousness and, of course, we don't quite have the concentration of mass of intelligence that we did in the '50s. This early interest in the brain and consciousness was really unusual, because that was just on the heels of behaviorism and yet they did publish a lot of interesting things that really provided the momentum that we see today in neuroscience, in the neurosciences. But we do have smaller scale organizations, like the Association of the Scientific Study of Consciousness that are now going into--the Society is now going into its 11th year of existence. And each year they've appointed a distinguished president, someone who's well known in the field of neuroscience. And these small little efforts are really important to keep going. I only hope that developmental scientists can avail themselves of these resources and consider collaborations that would be useful, at least--even in the context of a meeting or something like that.

Zelazo: Well, I don't know why there's such reticence in developmental psychology to study consciousness. I don't know. There's a strong nativist resurgence in developmental psychology that's happening now. I don't know if that is part of the reason why there is reluctance to study the development of consciousness. Where do we let this lie? I don't know. But I think that--

Dalton: Maybe there's still a lingering heuristic orientation that we still have to look at and make behavior the major focus of what we're doing. Of course, we still have--developmental psychologists who have produced enormous amounts of research focusing on behavior and that's significant But maybe there needs to be--well, a bigger awareness of the tools that are available and maybe those tools are becoming easier to understand how to use from a neuroscience perspective. But the technological complexity, that's a barrier. There are imaging studies that some psychologists are drawing upon. But--

Zelazo: Well, I think the study of levels of consciousness that Philip David is pursuing is going to be helpful in terms of encouraging people to look at consciousness. I think the model that emerges from—first of all from the early stuff that we've done and what he's done subsequently—is a sound empirical approach for studying the development of consciousness. In the first year, you see, the baby is not unconscious. The baby is simply not self aware and doesn't seem capable of generating hypotheses or ideas, that is, recall of associations. So, there's some work being done on those levels. I think a good contribution of the model done at later ages is to show the continued development of increasing abstraction. As you go along you seem to get more and more removed from the concrete, or have the capacity to be more removed from the concrete. I think that's where the year old infant or the baby under a year old begins. There's not a lot of evidence that it's even aware of itself, but it is aware of the stuff—the stimuli—and can recognize those stimuli that it has experienced. That's not the same as saying the baby is unconscious. Right? So I think the important thing here is—one important thing—is that consciousness itself is developing. That gives us—developmental psychologists—a profound role in the study of consciousness. Most other things that we're studying develop. Consciousness is also a developmental phenomenon, so you would think that there would be more interest. I don't think that many people in developmental psychology, with the exception of Philip David and yourself, show an interest in consciousness or are even aware of this chapter in Jasper's book. But I think at some point maybe they will become more aware. Philip David is studying the role of recursive consciousness and the recursive phenomenon with older children. I think it might start to get more and more accessible and more and more interesting.

Dalton: As this interview draws to a close I just wanted to give you one last opportunity if you had any other thoughts that you wanted to share. I know that this is going to be one segment of your interview and there'll be another segment that'll focus on your earlier life and earlier career before you even went to graduate school. Here we wanted to focus on a very important subject of your relationship--intellectual relationship to Myrtle McGraw and the scientific tradition that it represented. And if you have any further thoughts--

Zelazo: Well, there're probably two points here. One is that I've always taken an experimental orientation with the exception of what I call the functional play study. It strikes me that there was too much reliance on observational research in early child development initially. I know that there's a lot of experimental work out there, but somehow I always feel as though there isn't adequate appreciation for the power of taking an experimental orientation to the study in development. I suppose some people feel that we can't manipulate developmental variables like the difficulties involved with studying the transition at the end of the first year of life. What do you manipulate if it's just development, I suppose? And in a sense there are things you can't do. We've taken the approach of facilitating development, which is benign generally relative to depriving development. I like the model applied in animal studies of deprivation. Removing or prohibiting something from happening, such as the seminal work on the visual system in which diffuse light was provided but not patterned stimulation used a deprivation kind of approach. But a facilitative approach is something that still lets you know that you are manipulating the key variables in the development of that particular domain. So I think that the stimulation of development can allow you to study development experimentally as in our neonatal stepping research. I've done it, because I find it most satisfactory and I think we get more understanding of positive mechanisms and that we are dealing with the right variables. I get frustrated by too much correlational work, because we never know what variables are causal; we are not constrained. Right?

A second point is that there is a tendency to treat biological variables as more credible than psychological variables. I know this is an interest of yours and a topic included in your own recent book [Early Experience, the Brain and

Consciousness: An Historical and Interdisciplinary Synthesis], co-authored with Victor Bergenn, about to be published But that's one of the problems even with the fascinating new techniques of brain imaging and the study of brain mechanisms in development. The neuroscientists sometimes behave as though they are telling us more precisely and more precisely than they are what variables are crucial. For example, in one recent book—and you have parallels in your book—Jerry Kagan's book on *Argument for Mind*, he talks about an auditory signal lighting up something like 24 different sites in the brain, some of which are totally irrelevant, that is may not be necessary for the act performed—I suppose not irrelevant, but not necessary for the behavior that's being studied. And the difficulty with the illusion involved in this process of measuring brain images is that it does not give you a causal relation in itself. Brain measures may reflect electrical brain activity or increased blood flow, but that does not necessarily mean that it is causal and it may not even be necessary. And where in the stream of brain activity associated with the behaviors that you are observing is it most important? Right? So these are incredibly glamorous techniques, but I'm not convinced that they give us as much even as the experimental manipulation of variables. Right? And even that can be reduced to somewhat of an illusion, but I think the illusion is even bigger and more compelling in some of this work on the brain based phenomena. And I think that's one of the really good things that I liked about your book, cuz it's throwing out a caution--any statement about the limitations of brain imaging techniques—that there is an illusory component to this as well. Brain imaging appears to be a phenomenon that can override caution because it's so glamorous, so expensive, and so attractive in its own right. It's fascinating to see parts of the brain light up and this is what is so alluring. There are ways that we can use that technique, but I think it's really got to be pursued with the kind caution that you and Kagan and others have called for.

Dalton: Yes, I agree with you, Phil. I think it's too alluring to the wrong people I think--

Zelazo: Yes.

Dalton: --or looking for quick answers and quick solutions and so here's exactly where we can identify the relation between that brain function and your behavior. And it's, of course, not that simple, as you point out. But anyway, I guess we'll wrap up our conversation for today. I want to thank you very much for being with me in Boston. I had to fly further than you did from California, but I really enjoyed our conversation and I look forward to —your completing the other part of your interview and we'll get these things transcribed and out there.

Zelazo: Great. Thank you very much, Tom.

Dalton: You're welcome.

End of Interview