

## Brian Hopkins

- Born: 4/26/1940 in Manchester, UK
- Spouse: Janet Hopkins
- Teacher's Certificate (1961), University of London.  
Diploma in Physical Education (1962), St. Mary's  
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### Major Employment:

- Department of Developmental Neurology, Faculty of  
Medicine, University of Groningen - 1975-1986,  
Lecturer/Senior Lecturer
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University Amsterdam - 1986-1995, Professor and Head
- Faculty of Human Movement Sciences, Free University Amsterdam - 1990-1993, Dean

### Major Areas of Work:

- Brain and behavior development in infancy
- Motor development
- Mother-infant communication
- Cross-cultural psychology
- Theory and concepts in developmental psychology and psychopathology
- Research methods and statistical analysis applicable to the study of development

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- Child Development Reviewer

## SRCD ORAL HISTORY INTERVIEW

Brian Hopkins

Interviewed by Nadja Reissland  
In Lancaster, Lancashire, United Kingdom  
August 8, 2008

Reissland: The interviewee for this oral history session is Professor Brian Hopkins, Lancaster University, with the interview being carried out at his home in Lancaster. The interviewer is Nadja Reissland, Durham University, and the date of this interview is the 8<sup>th</sup> of August 2008. First of all, let me thank you for asking me to interview you. Shall we start with a personal note? Is that what you would like to start with?

Hopkins: Yes, I can do that. So a little bit about where I've come from. Well, I was born into an Irish family in Manchester in the UK where I grew up until the age of 18 years. My father ran what we call in England a corner shop, selling everything and anything, but mainly groceries, and my mother was a nursery school teacher. Both parents had completed secondary school education up to the age of 16. As for myself, in terms of my earlier life, I missed a lot of primary school through illnesses, and I nearly died from double pneumonia when I was eight years old. To recover in those days, you were sent to a sanatorium, and antibiotics were not yet available. So, I went to a convalescent home for children near Blackpool and I spent a year there.

**Reissland: Did your parents visit you there?**

Hopkins: Occasionally, but it was difficult for them because it was quite a distance and transport was not easy in those days.

**Reissland: Do you have brothers and sisters?**

Hopkins: Yes, I have a brother and two sisters. I'm the oldest sibling, born at the beginning of the Second World War and lived in the dirty, grimy, and polluted city. It was certainly not a healthy environment for a growing child, and many of my peers suffered the same sort of health problems and some even died from lung-related diseases. It didn't become anything like a healthy place to live until the Clear Air Act was established in the late 1950s. In short, it was a pretty awful place to live, but nevertheless I still retain a lot of affection for Manchester. Anyway, I finished primary school and then went to secondary school, which I hated. I could not stand it. I played truant and--

**Reissland: That was also in Manchester?**

Hopkins: Yes, and for me secondary school was a nightmare. I think I probably had symptoms of ADHD, which in those days would not have been diagnosed. I just couldn't sit still, couldn't pay attention, and I was constantly embroiled in discipline problems because of disruptive behavior. Eventually, I was kicked out at 16 years of age. I did have some academic qualifications, but not very many. I see the protocol asks about military experience. Well, I escaped military service by six months. Looking back, it probably would have been a good thing for me to have completed what was then called National Service in the UK. It would have helped me to develop more self-discipline, I think. But anyway, my earliest work experience mainly revolved helping out in my father's shop.

**Reissland: Did you enjoy that?**

Hopkins: Sometimes, but I felt didn't get paid enough! It was always a source of conflict between my father and myself. After I left school at 16, I did various other jobs: mainly laboring jobs on building sites and things like that, unskilled labor in other words. Then, for some reason, I decided to become a draftsman, and so I sought an apprenticeship to be a draftsman, but I hated it. By that time, I was getting on towards about 18 years of age, and came to the conclusion that I had just about enough qualifications that I'd could go to college to become a PE teacher. And so I went to a teacher's college in London for three years, because really the only thing I could do reasonably well at school was sports. I was no good at anything else, certainly not anything academic

**Reissland: And rugby especially?**

Hopkins: Rugby, yes, I particularly enjoyed rugby and also boxing. Anyway, I went and did my three years at this teacher's college and qualified to become a secondary school PE teacher.

**Reissland: You haven't mentioned your children.**

Hopkins: Well, I haven't got to that yet.

**Reissland: Okay. Then go on. You were working in the shop and then you went to the teacher's college? Is that so?**

Hopkins: Yes.

**Reissland:** And so in which college was that?

Hopkins: St Mary's College, Twickenham, near London. After finishing three years of what for all intents and purposes was a sort of sports scholarship, I came back to Manchester and I taught a few years in a school in the center of the city. What was interesting for me was in the time that I worked there, it became increasingly multicultural. It was a time when the UK began to experience large-scale immigration from the Caribbean Islands and later from the Indian sub-continent. Suddenly, so it seemed at the time, there were all these very 'exotic' children, and they were so different than the homegrown, let's put it English, children, and particularly different were the children who came with their parents from one of the Caribbean islands. They were extremely athletic. Those from India, and later Pakistan, were not really interested in sports, except for the boys' enthusiasm for cricket. I wondered: where does this athleticism shown by the Caribbean children come from? Does it have something to do with child rearing? This was a question I came back to later in life. So, how did I get interested in child development? Well, that was one of the sources.

**Reissland:** You haven't mentioned your wife Janet yet. When did you meet her? Was that around this time?

Hopkins: Are they really interested -

**Reissland:** Well, I am interested in that. You had your Ruby wedding anniversary just recently, didn't you?

Hopkins: Yes, we did after two marriages 40 years previously.

**Reissland:** Two marriages? Why?

Hopkins: To begin with, we had a Catholic one because of me. In those days, you had to get permission if you were marrying somebody who wasn't Catholic. So a week later, we had an Anglican wedding in Janet's home church in Ambleside. That was in June 1968

**Reissland:** Was that during your teacher's training?

Hopkins: No, I'd finished by then. After we got married, we both went to the States.

**Reissland:** What were you doing in the States?

Hopkins: We were both doing master's degrees.

**Reissland:** And where?

Hopkins: University of Illinois, Champaign-Urbana. We had a great time and got an enormous buzz from the intellectual challenge (for me, the first I had experienced). We enjoyed ourselves so much that we planned to stay in the States. I was going to do a PhD there that cut across anthropology and psychology. But Janet then got an exacerbation of a problem she had with her cornea called Terrien's disease, which is quite rare. She felt more comfortable having the operation done by a surgeon that she already had consultations with back in England, so we decided to return to the UK. Janet was actually pursuing a PhD in French, but she didn't complete it as a consequence, unfortunately. We came back to England and I got a job in a teacher's training college, teaching statistics and research methodology. At the same time, I did my PhD on a part-time basis.

**Reissland:** Yes. And where was that?

Hopkins: At the University of Leeds. And I did it on the theme that I picked up when I was teaching. How is it that children from the Caribbean islands, boys in particular, are so athletic? What is it in their background? It took a lot of organization, but I got access to hospitals and I looked at children born to Jamaican parents and compared them to the 'everyday' English child if that's the right way of putting it. And one of the main things I found was that the Jamaican mothers had a system of handling the children, massaging and exercising them like you found -

Reissland: Yes, in India they do this.

Hopkins: Yes, you are familiar with its use in that country from your own research. Certainly, it's widespread throughout non-western parts of the world. While I was interested in motor development from having been a PE teacher, I was never happy with the idea of 'sectioning off' motor development. You have motor development, you have social development isolated from that domain, then you have cognitive development and that's isolated, and so on. But actually, you're dealing with the whole child. One of the motivators, if you like, of development of action, control of action, and acting on the world is the ability to control and coordinate movement and posture. Thus, I was interested in how motor development may impinge upon, or have some association or relationship with what we call or classify as cognitive development. I wasn't particularly interested in social development at that time.

Reissland: And emotional development?

Hopkins: Yes, to a limited extent, I suppose. But you know I kept social and emotional development, but not cognitive development, somewhat out of the research. I couldn't cover everything just working on my own. I found that formal handling had a rather specific effect on motor development, and in particular on the acquisition sitting, sitting upright, and to some extent with walking, but it had no effect on the range of Piagetian-based tasks that I gave these children when they were six and twelve months of age.

Reissland: Were you happy with the Piagetian tasks, or did you think they were perhaps not getting at what you wanted to get at?

Hopkins: Some of them were derived from my own ideas and reading for which I distilled tests of object permanence. But there were also tasks drawn from the work of John McVicker Hunt and Sybille Escalona who had devised scales of Piagetian-based tasks for infants. So, it was a mix of my own ideas and those of other people.

Reissland: When you were doing your PhD did you have a mentor? Did you have anybody important who guided you in your research?

Hopkins: No.

Reissland: No? It was just you doing yourself?

Hopkins: Yes. I was doing it part time and traveling around Birmingham on a motorbike to the homes of the infants.

Reissland: So you went to all their houses?

Hopkins: Yes, I went to all the houses. I saw the infants first in the hospital and did some newborn testing there, and then I followed-up in their homes. It took me about three years to gather the data, and I finally finished the PhD in 1976.

**Reissland: Did you videotape them?**

Hopkins: Some, yes. Some I videotaped, but I didn't really have the money. There was very little money around in those days for this sort of research, but occasionally I could borrow with video equipment from the college where I was working at the time. In those days, the video recording machines were heavy and clunking things, but nevertheless I managed to strap one on to the back of the motorbike, but I couldn't use it on a regular basis. I got some shots, some videos of these mothers handling their babies and the routine that they took them through. By this time, we had one child.

**Reissland: What were your conclusions in the PhD? What was the main one?**

Hopkins: The main conclusion was that there was a match between maternal expectations about development and some developmental outcomes. All the Jamaican mothers had expectations about how their children should develop. Such expectations were mediated through this formal handling routine. So they expected their children to sit and walk much earlier than their English counterparts. It was all related to what in West Indian society constituted a sign of politeness, namely, to sit upright in an erect posture. Why? Because it shows that you're paying attention and thus being polite to your visitors or to members of the family. This handling routine was driven by these cultural expectations, and it was so remarkable that these mothers knew exactly when the infant was going to sit, when he was going to walk. The English mothers were a lot more vague about it such things. The other interesting thing was that none of the Jamaican mothers said anything positive about their infants crawling. In fact, they said infants shouldn't crawl. When I asked why, there were various reasons: "Well, that's what monkeys do! That's not what humans do!" and, "A crawling child doesn't have any fear, does not develop a proper set of responses related to fear, so it can get into awkward situations, maybe tip boiling water on itself when crawling," and so forth. Actually, none of these infants had any extended experience with crawling, as far as I could observe.

**Reissland: Right. So they were just sitting and not mobile or being carried by their mothers?**

Hopkins: They were carried a lot by their mothers. And I suppose, they were in playpens and things like that, and thus they must have done some crawling, but their mothers were very attentive to any attempt by the infant to crawl and stop them doing it.

**Reissland: When the baby would try to crawl then they would pick them up and perhaps try to get the infant to walk?**

Hopkins: Yes, they did lots of walking on the body, walking up mother's body and things like that. That was part of the formal handling routine. Once again, they certainly didn't want to encourage their infants to crawl.

**Reissland: Is this then a kind of cultural difference?**

Hopkins: Yes, I suppose so. I could see that there was an effect of this formal handling routine and it was part and parcel of a self-fulfilling prophecy in a way. The mothers had these expectations and the formal handling routine helped them -

**Reissland: To fulfill the expectations?**

Hopkins: - to fulfill the expectations, yes.

**Reissland: You said your first daughter was born by that time.**

Hopkins: Yes, 1972, and the next one came 14 or 15 months later.

Reissland: And the third one?

Hopkins: The third one was born later in 1978 in the Netherlands because after I finished my PhD, we went to the Netherlands for me to take up an appointment in the Department of Developmental Neurology at Groningen University on the invitation of Heinz Prechtl. I worked in Groningen for ten years or so.

Reissland: So what kind of influence did Prechtl have on you?

Hopkins: Oh, an enormous influence. He sharpened up my thinking a lot. But I should point out that there were other people long before him who had an influence on me for the better. I came from a background where there was no real pursuit of the intellect. And so it was really a bit novel to sit down with a book. If I go back a rather long time, my maternal grandfather had a very logical and analytical mind. He was self-taught. He had come from Ireland to England and then proceeded to pull himself up by his own bootstraps. I was aware that he had incredible knowledge of many things political and historical, but especially of Irish history, which we used to talk a lot about. In this way, he really framed my way of thinking, but without me really being aware of it at the time. I suppose what he was imparting a logical linear way of thinking about problems, not a very creative way of thinking, but it has served me quite well in formulating research interests. I should also acknowledge the influence that my wife Janet has had on my adult life. I came from a family that was not academic, and meeting Janet was a cultural shock. She came from a family that had books all over the house, and people who read them, interesting classical stuff, as well as having interesting discussions about them. I used to sit there with eyes open wondering what the hell was going. Yes, Janet played a pivotal role in making me a more cultivated person, although sometimes I fail to live up to her literary (and other) standards. I own her an eternal debt of gratitude for 'civilizing' me. Before I forget, another important influence, years before I met Prechtl, was somebody at the first school I taught at in Manchester. The chaplain or the priest there, Father Guerin, gave me a copy - I don't know why he did it - a copy of Pierre Teilhard de Chardin's book *The Phenomenon of Man*. I don't recollect why he did it, but it was the first book of any intellectual caliber that I had read in my life. So I decided to make it a project for a couple weeks and I tried to get through reading it, because I really liked this priest who had a sharp intellect (as well as a doctorate in philosophy from University College Dublin), and engaging way of talking to me about things other than sport.

Reissland: Was it a Catholic school?

Hopkins: Yes, it was a Catholic school. Reading *The Phenomenon of Man* just blew my mind. It opened up a whole new vista and eventually led me to Charles Darwin. I read his *Origin of Species* book, and it blew my mind even more. I became an enormous fan of Darwin, and went on to read nearly all his works. Consequently, I realized that de Chardin's book contained a lot of hocus pocus, bordering at times on something akin to creationism or mysticism. Anyway, these reading experiences were already fermenting in my mind by the time I went to work with Prechtl. He was a student of Konrad Lorenz, and thus very much trained in ethology. Consequently, Prechtl's research ethos was, if you like, baby watching with the 'hands behind the back' rather than 'hands on the baby'.

Reissland: Did you ever meet Lorenz yourself?

Hopkins: No, I never met Lorenz, but I heard a lot about him from Prechtl. Prechtl, to his credit, inculcated in me rigor, scientific rigor, and a really broad-based approach to ontogenetic development, very multidisciplinary. It took me quite some time to get used to his

way of thinking, to accept what he had to say. But he, if I wanted to talk about a single person, was the one who influenced me or molded me most scientifically. But if I go back even much, much earlier, because the protocol here does ask ...

**Reissland:** Yes, that's okay.

Hopkins: ... if I go back even earlier, earlier than I have referred to so far, how did I get interested in development in the first place? I was puzzling about that last night. I spent a lot of my childhood escaping Manchester, living and working on a farm in Ireland, a farm owned by the relatives of my mother. I saw animals copulating. I saw animals being born ...

**Reissland:** How old were you then?

Hopkins: The first time I went I was about four or five years of age.

**Reissland:** Was it a regular happening ...

Hopkins: Yes.

**Reissland:** ... throughout your childhood?

Hopkins: I usually went in the summer for eight weeks at a time, some times longer, because I had severe lung problems. Thus, I could witness not only animals copulating and being born, but growing and developing before my eyes. I was particularly fascinated with pigs, piglets.

**Reissland:** Is that when you kind of slept with the pigs?

Hopkins: I didn't sleep with the pigs, no! But I was fascinated by them, because they seemed quite precocious at birth motorically, and then they just developed really quickly. You could clearly see almost from day-to-day that something in their behavior had changed. And ...

**Reissland:** So would you go to the sow with all these piglets and observe them?

Hopkins: Yes. I'd go into the sty and sit and watch them. I was so engrossed by it that the time seemed to fly by. Then when they became a bit more independent, you'd see them in one of the enclosures running around doing what pigs do -

**Reissland:** Digging?

Hopkins: - digging, rooting with their noses, but also other behaviors that looked for all intents and purposes as forms of play. So watching the piglets definitely awakened an interest in development and perhaps is a reason why many years later that I got hooked on issues of developmental change.

**Reissland:** Developmental change?

Hopkins: Yes. There two main issues in studying development: the origin problem and the change problem. I could witness both of them by observing animals on the farm. Of course, it's an assumption I'm making, and I don't know how true it is or how that experience influenced me. But I like to think it was a formative influence.

**Reissland:** Who introduced you directly to child development? You mentioned that Prechtl was one person, but you are rather broad in your approach? Aren't you?

Hopkins: Yes.

**Reissland:** Was there anyone else similar in importance to PrechtI?

Hopkins: It's difficult to say. Once again, I suppose my experience at school, the first school I taught at, was also something of a starting point ...

**Reissland:** Yes, because of these ...

Hopkins: ... 'exotic' children at the school. The influences on their development must have been so different from their English counterparts. I think that experience triggered or reestablished an interest in development and developmental change.

**Reissland:** So basically it was your own curiosity that drove you to go into this direction?

Hopkins: Yes, I think it probably played an important role in this respect, but once more this is evaluating my past perhaps too idealistically. Whatever the case, I don't think there was any particular figure before PrechtI who pushed me or influenced me or gave me motivation to study child development specifically.

**Reissland:** You stayed working with PrechtI for ten years? Is that right?

Hopkins: Yes. It became increasingly problematic to work with him, because he was very much what you would describe as a supporter of neural determinism - structural development of the central nervous system more or less determines everything about functional development. I began to think about such a standpoint, and came to the conclusion that a child just doesn't have a central nervous system, but also a musculoskeletal system that is very flexible in responding to external demands, and which interacts with the central nervous system. Moreover, the musculoskeletal system is the interface with the external environment. Thus, you have these interacting components and it's got to be much more complex than just this driving force generating change coming from the neuromaturation of the central nervous system. I got into some serious, perhaps heated, discussions with PrechtI about all of this, and as a consequence we became a little bit estranged from each other.

**Reissland:** Because you had different points-of-view?

Hopkins: Yes, because I was developing my thinking in a somewhat different direction to him.

**Reissland:** And you are very strong-minded?

Hopkins: I don't think I was strong minded enough, because I should have pushed my boat out much earlier than I did. I had a sense that I should be looking at development in a different way, in a much broader way. What happened next was interesting for me. I went with the germs of these rough ideas to a NATO Advanced Studies Institute in Maastricht in the Netherlands in 1985. Suddenly everything about development seemed to be based on dynamical systems thinking. Esther Thelen was there, having recently begun her set of revolutionary studies on infant locomotion.

**Reissland:** So you met her then?

Hopkins: Yes. In fact, I met a lot of people who were working or trying to get a handle on what it meant to apply dynamical systems thinking to child development.

**Reissland:** In that NATO institute?



Hopkins: Yes, which was organized by my late friend John Whiting. I came away feeling: Ah, that's really what I've been thinking about, but at the same time incredibly worried that it was too complex and difficult for me to understand it properly. There seemed to be quite a lot of sophisticated calculus involved in some of it. So I worried whether I could, whether I should, get to grips with this way of thinking.

**Reissland: How long were you there?**

Hopkins: It was about a week, if I remember correctly. It was quite intensive, locked up in this hotel almost 24/7. What I heard, dovetailed with my recent thinking about the nature of development, and so when I went back to Groningen I decided that there really wasn't any future for me working with Prechtl. I had my time and benefited from his intellect and scientific rigor. It was time to move on. So I applied for a chair at the Faculty of Human Movement Sciences in the Free University Amsterdam. For some reason or other I was appointed. Here, I came into contact with people who were much better than me, who were far more advanced than me, in terms of understanding what dynamical systems were all about, and one person in particular was Peter Beek. We wrote a couple of articles together, and in essence he gave me tutorials on how to apply dynamical systems ideas to questions about the development of action. It did change the way I did research - I became more experimental. The main thrust of what I started in Amsterdam was a longitudinal study of preterm infants. There were two tranches to this research. One was concerned with relatively healthy preterm infants, but who were growth restricted, yet still relatively healthy; thus they had problems arising from how the musculoskeletal and central nervous systems interacted. It was an interesting issue that I had wanted to tackle for some time. Anyway, these infants rather quickly overcame the problems of growth restriction, but nevertheless they developed more slowly than both their full-term and preterm counterparts who were not compromised in this way. We looked at reaching, and grasping, and the coordination of leg movements using 3-D kinematic recordings, things like that. As I said, there were differences in these respects early on, but they displayed a functional 'catch-up' - I suppose this is the best way to describe it.

**Reissland: Within the first year?**

Hopkins: Yes, in the first year, and we used corrected ages, of course, for the preterm infants.

**Reissland: Did you look at intellectual development as well?**

Hopkins: The only such measure we used, and it wasn't all that interesting for me, but was insisted upon by the pediatricians, was the Bayley Scales of Infant Development. There were 16 people working on this project and the pediatricians felt the need to include these scales in order to extract information about age-related developmental levels

**Reissland: And what did you find?**

Hopkins: We found marked differences, particularly in the first year, because for some of these preterm infants, especially those growth restricted, their movements were so poorly coordinated that you couldn't score them. Thus, you realized that the Bayley Scale of Psychomotor Development was more motor than psycho, because these infants had these considerable constraints on postural control and thus on the coordination of movement. Consequently, they had difficulties in reaching out and grasping objects, which made scoring their performances fraught with difficulties. But they did catch up subsequently in terms of Bayley scores and other measures relating to motor control and coordination.

**Reissland: How long did you follow them up for?**

Hopkins: We followed them up until two years of (corrected) age. The follow-up should have continued until school age, but by this time I had left the Free University and somebody else took over responsibility for the developmental research. But that never really materialized in the way I hoped. One member of the team did collect data on the children until 7 years of age as part of a PhD. She has so far never completed analyzing the data. I'm still waiting hopefully, but I don't think anything will happen with this particular set of outcomes. The second tranche of research within the same program involved a change from reasonably health preterm infants to the ones with brain damage as verified by means of MRI. These preterm infants were born quite young, less than 30 weeks in most cases. Quite a few of them had bilateral white matter lesions, referred to as periventricular leucomalacia. This disease affects that part of the corticospinal tract that runs down the spinal cord and innervates the muscles of the legs.

**Reissland: Does this disease have an effect on the ability to walk?**

Hopkins: Yes. The evidence is that most of them with this disease develop spastic diplegia. So I decided to focus on that group of infants, with the help of a couple of PhD students. What I was interested in was coordination, again from a dynamical systems point-of-view. We decided to concentrate on spontaneous leg movements, but not exclusively, with the infants lying on their backs and supported in a specially designed chair. Based on 3-D movement registrations, we found that these preterm infants at corrected ages had a form of coordination that was typical, or expected, of a healthy full-term newborn. In other words, there was little differentiation in the movements of the joints; they all moved *en bloc*. And so if you did cross-correlations between the hip, the knee and the ankle joints, then the coefficients were very high. They were also high in the control group of full-term healthy infants. But by about 12 weeks or shortly after 12 weeks corrected age, a sort of watershed occurs in postnatal development. At this age, the healthy infants showed a lot of joint differentiation, and so the cross correlations just went down to about zero. With most of the preterm infants who had periventricular leucomalacia, their correlations remained high after this age. Every one of them with high correlations beyond 3 months acquired spastic diplegia.

**Reissland: Is it used now as a test?**

Hopkins: I think some people are using it as a means of developing it as an instrument of early detection. Jan Piek at Curtin University of Technology in Australia is particularly well known for her work in this respect.

**Reissland: How did you get from that to fetal research?**

Hopkins: Well, that's interesting. Hanneke de Vries is an obstetrician working at the Free University Hospital in Amsterdam.

**Reissland: Did you meet her when you started your study there?**

Hopkins: No, I knew Hanneke already from my time in Groningen. We had research contact when she worked in the University Hospital there. And we both came to Amsterdam at more or less the same time. I was getting more and more interested at that time in expressions of laterality, behavior referred to functional asymmetries. I was wondering whether traits like handedness might already be in some way set down, or laid down, prenatally. I went to see Hanneke about studying functional asymmetries in human fetuses. OK, where did we start? Well to begin with, let's look at head position we decided, because there is a theoretical perspective that says head position is preferentially maintained to the right in most newborns. Most newborns will turn their head and maintain to the right, and as a consequence acquire a preference for viewing the right hand relative to the left. It's a long-standing theory, but not a very strong one in my view with regard to explaining how a hand preference is acquired.

George Michel, now at the University of North Carolina Greensboro, has carried out important work in testing the theory, and he has shown that there is a relationship between newborn head position and later hand preference.

**Reissland:** What about the position of the head in the womb?

Hopkins: Hanneke and I, together with other colleagues, got started on a longitudinal study with healthy fetuses from 12 weeks up till term.

**Reissland:** How often did you scan them?

Hopkins: Ultrasound scanning was carried out every two weeks from 12 weeks onwards until term age. It was a bit tricky at times to know whether you were looking the head to right or left of the torso of the fetus.

**Reissland:** Yes, especially when they grow with age, I suppose?

Hopkins: Yes, that's right. But anyway, with the decisive help of the obstetrician Herman van Geijn at the Free University Hospital, an ultrasound scanning technique was developed for detecting whether the head was to the right or to the left of the fetus's torso. We found that a stable head position preference was a rather late occurring event in human prenatal development. It was only after about 36 weeks there was a clear preference for the head to be turned and maintained to the right in the majority of fetuses.

**Reissland:** Did it depend on the womb environment?

Hopkins: A good question. In short, it didn't. We looked for asymmetries in the uterus and found no relationships to head position. So, we assumed that the acquisition of head position preference had some sort of neural bias. Fetuses maintain their preferred head position for some 2 to 3 months after birth. We didn't follow up in that study to look at what hand preference they developed. We just had not got the resources to do this. In another study, we found no evidence for preferential sucking on the right or left thumb as Hepper and colleagues at Queens University Belfast have reported.

**Reissland:** It's difficult in 2-D to actually see whether the hand or finger is in the mouth?

Hopkins: Yes, it is.

**Reissland:** And also which hand?

Hopkins: Yes, very difficult. We concluded that really the only reliable and consistent expression of laterality, behavioral laterality in a healthy human fetus, is head position. That is a postural bias and it can impose itself on movement expressions both before and after birth.

**Reissland:** Were there any political or social influences on your research, teaching and writing?

Hopkins: Mmm ... I don't think so, but let me just think. I haven't given any serious consideration to this particular question. Well, we were in the States at the time of the Vietnam War protest, the shooting of Robert Kennedy, and the Kent State shootings, each one a big shock for me. But I really don't think there's any political influence as far as I was concerned. Also, the Troubles in Northern Ireland started about the same time and that was very upsetting for me. Let's come back to this issue later as I am beginning to sense other related points are starting to go through my mind.

**Reissland:** But what about your cross-cultural research?

Hopkins: I never interpreted it as having had a political dimension to it. But maybe -

**Reissland:** But what about poor families you came across and the different interpretations of what constitutes good childcare and rearing?

Hopkins: I must admit to finding it a strange question, but perhaps I am being naive. I suppose you can be influenced by such factors. For example, if you look at the practice of science in the former USSR, that was very much determined by -

**Reissland:** By politics.

Hopkins: - by the politics of the Communist Party and the political beliefs of its leaders influenced what was appropriate in science or not. But I didn't feel those sorts of extreme influences on my work. I need to think about it again.

**Reissland:** The development of your ideas about fetal and infant research basically came from Prechtl?

Hopkins: Some of it, at least in the beginning.

**Reissland:** And then going on to dynamical systems?

Hopkins: Yes.

**Reissland:** Have you moved on from that?

Hopkins: I suppose not, although I increasingly fascinated by the theoretical vista offered by embodied cognition, which is 'dynamical' in the sense we are talking about (and dovetails with my earlier thoughts about central and musculoskeletal systems interacting over ontogenetic time in the development of action). The thing with making progress with dynamical systems thinking is that I'm not a mathematician. I have a basic grasp of differential calculus. I can solve simple differential equations, but I think if you're really going to progress you have to be able to model development as a non-linear process. Then, you need to be able to solve complex non-linear equations, and to apply them to the neglected issue of developmental transitions. Now, I did get a bit of a grasp on the relevant maths when we attempted to use catastrophe theory, which is one branch of dynamical systems theory, to study transitions in the development of reaching and grasping.

**Reissland:** You said something about the critical times of change. So is that something which you think has to come from the data, or is that something which you have said is like a watershed at 12 weeks, but which is something to be found in the literature?

Hopkins: Mmm ...

**Reissland:** How did you arrive at the notion of a 'watershed'? Was that from other people or does it come from your own observations and your own data?

Hopkins: Well, it was around in the literature, that's for sure. The two-to- three month functional transformation is something that increasingly fascinated me. When I did get involved a little bit in research on mother-infant interaction, I could see a major transformation occurring in infants between two to three months of age. The essence of that transformation was in a sense captured by the mothers who said that "At last, my infant has become human," with the advent of social smiling, cooing vocalizations, and attention to the

mother's face. It is actually one of my regrets that I didn't pursue the development of mother-infant interaction further, because I think I was probably one of the first people to look at the role of changes and effects in postural control, or motor control more generally, in the context of such interaction.

**Reissland:** It was actually expectations that mothers had about the development of their infants that played a role in this aspect of development?

Hopkins: Well, now you are referring to the West Indian mothers, and not to the Dutch mothers and their infants who I studied in a face-to-face interaction situation.

**Reissland:** Yes, but what about the English mothers compared to the West Indian ones? If you compare the two groups, the expectations were different?

Hopkins: They were different, because what was mediated by expectations of outcomes was strikingly different for the two groups -

**Reissland:** OK, but to come back to the two-to-three month transformation, was this a change that came from your own data and your own observations rather than what you see in the literature?

Hopkins: If I understand your question properly, then yes, I suppose it's both sources. Eventually you become convinced by your own observations that it really is a worthwhile phenomenon to pursue further by implementing your own ideas about what is actually undergoing qualitative change at this age. Slowly, but surely, it dawned on me that I needed criteria for pinpointing when such transformations or transitions took place. And so by the time I came to Amsterdam, I had become interested in catastrophe theory, and in particular so-called catastrophe flags, which seemed to offer the possibility of indicating when a transition is starting or is underway for an individual infant. So, for example, you might record a rather sudden increase in the variability of a particular function about a particular age, a flag referred to as 'anomalous variance'. To give a further example, you may detect a sudden jump (another flag) from what an infant can't do and to what is now possible to achieve, which may then be followed by another sudden jump such that the action appears to 'regress'. There are all sorts of odd, in some ways counterintuitive, changes during transitional periods that are captured by this approach. We used the theory to study the development of infant reaching and grasping, and in a couple of papers showed it to be a good fit to a cusp catastrophe model. What I learnt from this experience is that in the study of child development, and especially the development of action, pinpointing and understanding transitions, when they take place and how they take place, requires an eclectic, multidisciplinary strategy.

**Reissland:** If you reflect on the strengths and weaknesses of your research and your theoretical contributions, is this one the kind of impact you have had?

Hopkins: Possibly, but I don't want to make a case for it as I was 'standing on the shoulders of giants', to quote Issac Newton, in trying to apply dynamical systems thinking to infant development.

**Reissland:** Do other people follow in your footsteps in this and other respects?

Hopkins: Yes, possibly in terms of some publications. I think that one that is in a sense a legacy is chapter I wrote together with Louise Rönquist for a book on sensorimotor development edited by Francesca Simion and George Butterworth and published in 1998. That's where I tried to put down my ideas on development of laterality. It's been cited a few times, but more rewarding was that when I went to conferences some of the leading researchers in this particular area told me how much they benefited from this chapter.

Reissland: It would be nice then if that chapter was in the public domain?

Hopkins: Yes, as it's a book chapter, and so not many people are going to read it. .

Reissland: This is what I mean.

Hopkins: Anyway, without belaboring the point, I think that chapter is a legacy of one strand of my thinking about child development. The other is a more clinical approach, stemming from a long-standing interest I have in trying to develop tools for the detection of infants at risk (An interest undoubtedly inherited from PrechtI). I tried to bring this all together in a book edited by Jaan Valsiner and Kevin Connolly, thus yet another chapter. The chapter in that book is entitled *Developmental Disorders: An Action Based Account*. I devoted 30-odd pages trying to tell people what I'd done and what I thought was a good approach to developing tools for early detection.

Reissland: So what did you say?

Hopkins: In a nutshell, that you need observations of spontaneous movements, as well as some form of experimentation. With such observations, you can get an idea of what's wrong, but to understand the underlying determinants of it you have to devise ways of manipulating things such as posture, and in this way you begin to discern what has been wrong with this particular individual infant. That I feel is from the more applied, clinical, side of my research, and as such is another legacy.

Reissland: And from the more theoretical side?

Hopkins: On the more theoretical side, where I really put my heart and soul into it, was an attempt to outline what I thought were the benefits of adopting a perspective derived from dynamical systems thinking. This came in the form of yet another chapter in the *Handbook of Brain Behavior in Human Development* edited by Alex Kalverboer and Albert Gramsbergen. It's a massive tome, published by a very greedy publisher in 2003.

Reissland: They didn't give you any money?

Hopkins: No not that, but rather they charged a lot of money for the book. It was one of the most expensive books I've ever come across. Consequently, it hasn't had a wide circulation -

Reissland: Oh, right.

Hopkins: - which for me was unfortunate, as well as for the other contributors to the book, because I really tried to spell out what I thought was the value of adopting a dynamical systems perspective. It's all here in *Understanding Motor Development: Insights of Dynamical Systems Perspectives*. Another legacy, if you like.

Reissland: What benefit did you get from writing this paper?

Hopkins: I found that particular chapter useful in my teaching. Now, my swan song in terms of legacies - something that took five years to complete and finished just before I had to retire.

Reissland: What was that?

Hopkins: Editing the *Cambridge Encyclopedia of Child Development*, published by Cambridge University Press, which came out in 2005 (followed later by an electronic version that you can put on your desktop).

Reissland: Oh, right.

Hopkins: I have a couple of chapters of my own in the book, a book in which I tried to be as comprehensive and multidisciplinary as possible about ontogenetic development, with up to 100 contributors in total. The underlying motif of the book is to avoid segmenting development into different areas, but rather to think of it as consisting of overlapping associations between what are, after all, artificially created domains with heuristic, if not realistic, value. I think it's a reasonable source of reference, and graduate students who I teach have found it useful. I would like to think that academics would also get some benefit from it as well.

Reissland: We have covered the background to your primary research interests. You have referred to the published manuscripts that best represent your work. Is there one, perhaps not mentioned so far, that you feel gave an impetus to your research since you left Groningen?

Hopkins: Yes there is one, and I had forgotten all about it. This was a publication in the *Journal of Child Psychology and Psychiatry*, published in 1983. Here, I tried to show the relevance of studying motor control in the context of how early mother-infant interaction or communication develops, something I have mentioned previously in this interview. Although I wasn't aware of at the time, I can see now that the paper contained the seeds of some thinking about development in terms of dynamical systems.

Reissland: Concerning research funding over the years would you like to comment on your participation in shaping research funding policy, implementation, study sections, councils?

Hopkins: In the Netherlands, I got a few grants that helped to keep projects going. It was not easy to do so. Because it's such a small country, you submit a grant application to a national research organization, and then you have to discuss it in a group situation, you have to defend it, which I suppose is a fair way of doing it. But it took an enormous amount of time to do it all.

Reissland: More than getting funding from in the UK?

Hopkins: Yes. When I came back to England, I did not have a great deal of success. In the UK, I think there is somewhat of a resistance to dynamical systems approaches in psychology, but more particularly in developmental psychology. The few funding applications I made didn't really gel well with the Zeitgeist in UK Psychology, I guess.

Reissland: So do you think there is (or was) a kind of political agenda that certain people who are in power have got the money and therefore can direct how research is conducted and thus funded?

Hopkins: In my more 'down' moments, I came to conclusion that things could have gone better, but yes, I began to suspect there was something of a 'plot' against the acceptance of dynamical systems thinking in issues relating to developmental psychology, or something like that. In retrospect, I don't think that's true.

Reissland: Is the Zeitgeist not yet there?

Hopkins: Oh, I don't know whether attitudes have really changed in that respect. Certainly, with some individuals there continues to be some degree of opposition, at least to the way I interpret dynamical systems theory applications to development - perhaps it is not 'cognitive' enough? Anyway, I shouldn't complain, as I have been able to get research done. For

example, I got money from a Sweden foundation to work with Louise Rönqvist for a year in Umeå, which proved to be quite productive in terms of joint publications. All told, I've had enough financial support to keep me going research-wise and to keep me productive in terms of publications. If you want to talk about being involved and having an influence on research in child development since I came back to Lancaster, then two things seem relevant in this respect. One was that I continued to edit the journal *Infant and Child Development*, of which I was a co-founder. In addition, I took on the job of writing the application to establish the Lancaster University Centre for Research in Human Development. Finally, the application delivered the finances, and we now have a new dedicated building, which has ten labs in it. I saw the building through from beginning to end, and in process had some interesting interactions with the architects and builders about the design of the building - sometimes frustrating and other times quite rewarding. By the time it was all completed, I was on the point of retirement, so I couldn't actually benefit from all that effort myself.

**Reissland:** That's where you left a legacy then for other people?

Hopkins: I'd like to think that Lancaster Psychology could benefit from it. It was a big investment on my part, and together with being Head of Department at the time, it did cut across my own research time. Now I think of it, one other thing that may have had some small influence on the way research money was allocated was that I was a member of an assessment committee for research in Psychology in the Netherlands during 1998 to 1999. By that time, I had left the Free University. I was one of the four foreign members of the committee. During that year, the committee met up to visit and evaluate almost 70 programs of research in Psychology in nearly all Dutch universities. I had responsibility for those programs concerned with child development. It was a rather massive exercise. Once we had evaluated a program, we had to sit in front of its appointed representatives and tell them what we thought of their research in no uncertain terms - typically Dutch! I like to think it did have a slight positive effect on improving the quality of psychological research in the Netherlands, especially that pertaining to child development.

**Reissland:** We have talked about the various places where you have worked and the positions you held went. You were a professor in Amsterdam as well weren't you?

Hopkins: Yes. I think we should have brought this up earlier ...

**Reissland:** And Head of Department as well?

Hopkins: Yes and Dean of the Faculty as well. Is that enough on my employment record?

**Reissland:** To summarize, which institutions have you worked in, dates, and positions and so. Do you remember all your dates and positions?

Hopkins: I don't. I haven't written them down, and they are stored as a CV somewhere on my computer. OK, if you want to know about jobs I had before I went to Groningen. Well, after the job I had at the teacher's college near Birmingham, I completed my PhD, and worked as a Research Fellow at the University of Manchester on the development of infants with Down's syndrome.

**Reissland:** And then?

Hopkins: Then I got a job offer to go to the Netherlands. I collected data on Down's syndrome infants, but with taking up the position in Groningen, I never participated in writing them up or in publishing papers relevant to the project. Something of a regret, but still working on 'soft' money and having a young family, I felt I could not turn down the offer of a permanent position.



**Reissland:** So then you went to the Netherlands?

Hopkins: I went to the Netherlands in 1974 as lecturer to work with Heinz Prechtl in the Department of Developmental Neurology at the University Hospital in Groningen. And in '85/86, I went to the Free University Amsterdam where I was offered a chair.

**Reissland:** When did you become Head of Department there?

Hopkins: Straightaway. Two years later, I became Dean, which was a bit of a disaster for me, because I wasn't banking on that happening so soon. It soaked up a lot of time I could devote to research because I had to combine it with being Head of Department at the same time. I didn't enjoy that particular experience lasting three years at all.

**Reissland:** Is that the reason why you then decided to come to Lancaster?

Hopkins: It was building up. I had five years in the Free University where research-wise I just really enjoyed myself. It took about three or four years to get established. I thought though, is it really going to work get this program of research on the development of preterm infants set up? Fortunately, I was lucky in establishing good working relationships with pediatricians, obstetricians, radiologists, child neurologists, psychologists, and physicists, as well as having a few really good and committed PhD students.

**Reissland:** How many people in total?

Hopkins: Sixteen, as I mentioned previously. As the data started to come in, I thought we are going to have big problems with the analysis for various reasons such as participant attrition - surprisingly (or perhaps not so surprising) more with healthy full-term infants rather than with the preterm infants. Anyway, thanks to hard work, especially by the PhD students, it all came together, and we had about five years of just analyzing the data and publishing them, you know, just a wonderful time.

**Reissland:** With regard to the data, perhaps we can say that other researchers have reported similar problems collecting data involving preterm infants?

Hopkins: Yes, of course.

**Reissland:** So, even though it's difficult to collect such data, it's still worthwhile--

Hopkins: I think if you're going to do something like this where you want to provide some instruments or tools for early detection, then you have to do longitudinal studies, and you have to go along with all the problems preterm infants have. They can become ill, drop out, and even can come back in again. It's a difficult form of research, but very, very rewarding.

**Reissland:** How many preterm infants did you manage to recruit, do you remember?

Hopkins: I can't remember exactly. There weren't massive samples as per epidemiological research. The first project we had about 30/40-odd, and the second one about the same number.

**Reissland:** So the 40-odd you mention took about three years to assemble?

Hopkins: Yes, given the rather demanding selection criteria, it took about a three years in each case.

**Reissland:** To put into perspective how difficult such research can be is necessary because the general public don't recognize that.

Hopkins: I think you may be right. Anyway, my successors, if you like, in Amsterdam, just didn't want to get involved in this sort of research.

**Reissland:** Why do you think?

Hopkins: You don't get quick publications from it. You have to wait quite a long time, and then they come in a splurge. What happened to me in the Netherlands in becoming a (reluctant) Dean was I got increasingly involved in bureaucracy, which acted as a sort of block on creative thinking, a block on being able to continue to be research active. Somehow, I have never talked about this experience before, and in a sense it is now a bit of relief to do so. Anyway, after about ten years in Amsterdam where I had five just fantastic years of being involved in research, I decided it was time to give myself a break. During this time, I really benefited scientifically from communications I had with people such as Peter Beek. But it proved to be detrimental for my family life being so absorbed with research. I was so engrossed with it all. I had my own little bailiwick. But the bureaucracy began to wear me down, and then added to that the Dutch government came up with the idea of establishing research schools. So, externally validated research programs such as my own had to become amalgamated into the quite large inter-university research schools. I actually had to participate in setting one up in Amsterdam together with a group in Nijmegen when I was Dean. Initially, I had some enthusiasm, and then I realized how much bureaucracy was going to come, layer upon layer of bureaucracy, and I'm going to have people dictating to me what I can do and what I can't do in terms of research. Thus, in a sense the move from research programs to research schools was politically motivated. We come back, therefore, to politics in research, something I wasn't really aware of until I thought this change through. I decided I've had enough of this, I need to get out to refresh my mind. I'm getting stale with all this bureaucracy, which is only likely to increase. So, I applied for a position at Lancaster and I got it.

**Reissland:** Were you Head of Department right away here too?

Hopkins: No, thank God. I had about three years of little bureaucracy and I got quite productive doing research. I had a lot of PhD students too from Amsterdam and a couple in Lancaster. These were three years where I was reestablishing myself research wise, and then suddenly the person who was Head of the Department decided to up sticks and leave. I was voted, between inverted commas, to become the next Head of Department, so I had to take that over for three and a half years. Fortunately, I had wonderful secretarial help, and consequently the job wasn't as onerous as in Amsterdam.

**Reissland:** Right, but then you took on the task of getting this child development centre set up?

Hopkins: Yes. I really put my heart and soul into that.

**Reissland:** We haven't really talked about your experience as a teacher of child development. What kind of courses do you teach? Do you still teach now?

Hopkins: Yes.

**Reissland:** Is there a tension between teaching and research, do you think?

Hopkins: Yes, there is tension in the sense that you don't just stand in front of a class and teach, which I quite enjoy. It also involves setting examinations, marking them, and all the

hooah that surrounds third year examinations in English universities, for example. There's lots of administration to it, so you feel that sucking away the time you can devote to research. And it does become a tension in that respect, but that somehow you have to deal with it in such a way as to ensure you have some time for productive research. What I've always tried to do, but not entirely successfully, is where possible, to build my teaching around my own research. Then you're much more motivated about your teaching, I experienced.

**Reissland:** Yes, and the students become more motivated as well, don't they?

Hopkins: Yes, I think so. I've taught prenatal development, and early postnatal development of perception and action. Also neurophysiology of subcortical structures, mainly the basal ganglia and cerebellum. I think for undergraduate students of psychology, these are 'killer' topics, up there with statistics.

**Reissland:** They can't see the relevance?

Hopkins: Yes. You can point out the relevance and how important, for example, the basal ganglia are for many everyday actions. There's a lot of functions that you can allocate to the cerebral cortex that are replicated in the basal ganglia. Thus, you have, in a sense, this redundancy in the substrates of a range of functions typically ascribed to the cortex. The basal ganglia (and for that matter the cerebellum) is a terribly important structure in the brain, and I try to motivate students to appreciate this, but most undergraduates are really not interested in neuroanatomy and neurophysiology, except for the purpose of answering compulsory exam questions. In addition, as for my own interests, little is known about the development of the basal ganglia in the human, and the same can be said for the cerebellum, I think.

**Reissland:** And in scanning?

Hopkins: Yes, if by that you mean brain imaging.

**Reissland:** Yes.

Hopkins: And also in the two visual systems and mirror neurons. Certainly, you get some students interested in that sort of cortical 'stuff'. Teaching wise, I was landed with the sub-cortical 'stuff', and I began to ask myself "Is this terribly relevant for undergraduate students of psychology?" Nevertheless, I enjoyed teaching it, if only because it kept me up-to-date with recent findings on the functions of the basal ganglia and cerebellum. Increasingly I got involved in a course that I really grew to like: a third year course on history and concepts of psychology. As a consequence, I've become quite keen on studying the historical background to research in child development, going back to the 18<sup>th</sup> century. I really enjoy that and I get a big kick out of teaching about the history of psychology. It's not just about child development, but I try to bring that in when possible.

**Reissland:** What is your experience with the SRCD? When did you join it?

Hopkins: I've never been a member. I'm not a great joiner of organizations. I've never been to one of the Society's biannual meetings. I had intended to do so a couple of times, but it clashed with workshops or smaller conferences where I'd been invited to talk. So somehow those things conspired against me going to SRCD meetings.

**Reissland:** What was the earliest contact you had with the Society, and with whom in the Society?

Hopkins: Adopting a loose definition of 'contact', I've done a lot of reviewing for *Child Development* over the past 10 to 15 years, which is one of the key journals in the area. In my

view, the time that the journal was under the editorship of Marc Bornstein was very important. Why? Because he really opened the journal up to Europeans - he actively encouraged Europeans to submit articles to it. Prior to him becoming editor, Europeans, in the UK, but more especially in France, thought "We don't stand a chance of getting published in this journal." Europeans tended to feel up to the time of Marc Bornstein a little bit suspicious of *Child Development*, that they wouldn't get a fair deal, that their submissions would be fobbed off for comments to PhD students of the academics who had been asked to review in the first place.

**Reissland:** OK. Can you now comment on the history of the field during the years that you have participated? In terms of major continuities or discontinuities and events related to these? Have things changed over time?

Hopkins: Yes, I think the field has changed a lot since I started in the early 70s.

**Reissland:** Have they changed in your opinion for the better or the worse?

Hopkins: For the better in some respects, but one has to be careful making such judgments in other respects. What I've experienced in my time is that Piaget is still here, very much alive, despite the fact that some have predicted the death of Piagetian theory over the years. It hasn't happened. It's been refined, re-modified, particularly for the period of infancy. If you take research on newborn and infant imitation, for example, it has shown that Piaget's original ideas in this respect had to be modified. But this theory is still very much with us, so that's a form of continuity in the way we are talking about. In the time that I've been research active, there's been an increasing interest, a growing interest, in Vygotsky's social-cultural theory. And I think some parts of it are really interesting in ways that Piaget's edifice is not - the zone of proximal development and the related concept of scaffolding being the most obvious examples.

**Reissland:** You saw the value of Vygotsky's theory in this respect already at the beginning of your research on mother-infant interaction?

Hopkins: Yes, exactly.

**Reissland:** The mothers were scaffolding their babies -

Hopkins: I didn't actually call it 'scaffolding' at the time, but recognized something like it. Jerry Bruner in the States was a very important person in terms of bringing Vygotsky into mainstream developmental psychology, as later were the writings of Jaan Valsiner. And, then of course, there is the impact of dynamical systems thinking, which stems from about the middle '80s. Without a doubt, a major person who had an enormous impact on research in child development through introducing and applying such thinking to the field, was Esther Thelen. While she was a very clever experimenter, she had what I suppose is the ideal way of doing research: first observing in the old ethology sense, get an ethogram of what an infant does at particular ages in particular contexts, and then derive from such observations related experimentation. She did all of this and did it brilliantly.

**Reissland:** Did you work with her?

Hopkins: No, I never worked with her, but I knew her and met her at conferences and events like that. She was a tough lady.

**Reissland:** Was she?

Hopkins: Yes, in the positive sense: a lot of scientific rigor and telling criticisms, especially about representational concepts washing around in cognitive psychology in the 1980s and 1990s. Of course, she did not achieve all of this single handedly, because there were other significant people involved in promoting dynamical approaches to studying child development, but she really brought dynamical systems thinking to the study of child development in a big way. So, the work of Thelen and like-minded researchers brought about a definite change in child development research, starting about the middle of the 1980s. Perhaps the biggest changes to have taken place in studying child development in the last 30 years or so would be more technological than theoretical in nature. Thus, it has involved the incorporation of brain imaging techniques into studying infant perception and forms of cognition. Not just MRI, functional or structural, but also multi-electrode EEGs, so-called geodesic forms of recording brain waves, in what is now referred to as developmental cognitive neuroscience. And not to forget, ultrasound recordings to study fetal behavior have become increasingly evident, the first research applications starting about the middle of the 1970s.

**Reissland: But there are still not that many people doing it in order study fetal behavior?**

Hopkins: No, which is perhaps a little surprising. In psychology, there's certainly a few people applying ultrasound recordings to study fetal development. The resolution of the scanners has improved considerably in recent years so you can see a lot more and the images are clearer than say even 10 years ago. All told, fetal ultrasound scanning has amounted to an enormous technological breakthrough, and as such has opened up a whole new vista for understanding the origins of human development. I think that developmental psychology, and the field of child development more generally, should pay more attention to the recent research in this area. Then, there is also the more recent advent of 3-D and 4-D sonography, which still has a few problems to overcome before it can be used in the same ways as 2-D recordings have for studying fetal behavior (e.g., the sampling rate of these innovations is somewhat restrictive). Together with improvements in scanner resolution, these innovations should encourage more people to do research on the development of fetal behavior in humans. Turning to postnatal development, another thing that has had impact, at least for me personally is motion or movement registration systems in 3-D. I've used them in my own research, but their usage to study infant action started with Claes von Hofsten in Sweden in the late 70s, early 80s. He was really the pioneer in this respect and it's taken off since then. It's turned out to be quite a major enterprise in the study of development of action. Sometime during the 1990s, passive marker movement registration systems became available that didn't require a lot of expertise to make them operational for your own specific research questions. Related to 'easier-to-use' registration systems, another important influence to have an impact on the study of action in the context of child development was the emergence of more user-friendly software such as Matlab for analyzing signals.

**Reissland: Do you use Matlab?**

Hopkins: Yes, I have. But one of my shortcomings is that I have never really been able to find the time to be able to program proper in Matlab. I wish - maybe I will ...

**Reissland: You can do it now, couldn't you?**

Hopkins: - maybe I'll have time now that I am retired. Anyway, these are the sorts of changes that I have experienced as breakthroughs in the study of child development over the years.

**Reissland: What are your hopes and fears for the future of the field?**

Hopkins: That's a difficult one. Maybe my hopes - one major hope at least - is to have a really well-articulated theoretical background that has to do with the development of brain-behavior relationships. It's not just got to be about cortical

development, but needs also to encompass sub-cortical structures, and be particularly relevant to the development of action, which has been the main focus of my research over the years. A big ask, I suppose. There are theories and texts around that begin to portray something of the approach to brain-behavior development I would like to see, but almost all concern cortical structures and functions, and ignore the basal ganglia and cerebellum, for example. Staying with the neocortex though, one finds relatively little attention paid to the development of the corticospinal tracts, which are supposed to be very strongly implicated in development of action, especially in the acquisition of prehensile abilities. Compared to the rhesus monkey, we really don't know a great deal about the development of these important descending tracts in the human, and how damage to them can give rise to cerebral palsy. This situation is gradually being rectified though the important programs of neuroscience research carried out by Janet Eyre and Roger Lemon in the UK, for example. I think students of child development, and by this I mean those who consider themselves to be primarily developmental psychologists with an interest in the development of action, should keep themselves informed and up-to-date with the this sort of research.

**Reissland:** Any other hopes?

Hopkins: More and better research on human prenatal development of movement and posture, and their developing interrelationships. Perhaps another is really for somebody to write a first-rate book, a comprehensive book, on the history of research in child development.

**Reissland:** That's the topic for you?

Hopkins: Possibly, but I don't have the training to write an historical text. It should not only be the history of child development study from a US point-of-view, but it should also account for and evaluate the contributions of Europeans - not just Piaget and Vygotsky, but also on pioneers who came before them like Wilhelm Thierry Preyer. I'm missing something like that.

**Reissland:** And what are your fears?

Hopkins: My fears stem from how brain imaging has become such an important tool in research on child development. I have had recourse to use it in my own research in the form of structural MRI. Its implementation has grown very rapidly in the last 10 or so years, and in some ways justifiably. But I get the feeling, shared by quite a few colleagues, that if you haven't managed to incorporate some technically impressive brain imaging into your research, then you're not going to stand much of a chance of getting published in the really top, high impact, journals. My real fear, however, is that with a growing emphasis on brain imaging at the expense of studying development of natural actions in everyday environments, we're going to lose a crucial perspective (and insights) on what constitutes normal or atypical development. We still need the 'hands-behind-the-back-and-observe' approach to the study of child development, but alas it looks like it is a dying trend. Another fear? Well, based on your questioning earlier, I'm beginning to wonder about the issue of whether there is any political interference with research that has had a bearing on my own research. As I said, I've never really given it serious thought before this interview, naïve perhaps on my part. I would like to think that there has been no or little political dimension in or political influence on my research. But if I think about what's going on in this country and maybe it's the same in the States with regard to research that impinges on child development/wellbeing, then I get the feeling that politicians and policy makers have been increasingly interfering with what constitutes 'appropriate' research. Certainly, in the UK, we have increasing political attention being paid to the problem of poverty, and thus what to do about children being brought up in impoverished environments, broadly defined. We're the most tested nation of school children in Europe, if not the world, at the moment, from nursery school onwards. A good source of employment for child psychologists perhaps, but one that gives me concern because of its potential impact on the practice of parenting. Successful scholastic results are associated with

this way of parenting we are informed, thus potentially giving rise to divisive opinions (each supported by research findings) about what is 'good' parenting and what is 'poor' parenting in what is now an ethnically very diverse country. Thus, yes, the potential for political interference does worry me, and yes it's ultimately going to have some effect on the way funding is dished out. Your research has to provide solutions to resolving child poverty, you have to address parents not doing their job properly, you have to educate parents to be parents ... All of this is a very risky road to go down for researchers in child development. Somehow, I feel I am getting a little out of my depth with the direction of this discussion ... Have we run out of tape time? I think that is about it

**Reissland:** Thank you very much for making the time to record your thoughts.

Hopkins: Thank you for taking on the job of interviewer.

**End of Interview**