

Robert A. Hinde

- Born October 26, 1923 in Norwich, England; died December 23, 2016
- B.A. (1948) University of Cambridge, B.Sc. (1948) University of London, D.Phil. (1950) Oxford University, Sc.D. (1961) University of Cambridge

Major Employment

- Honorary Director, Medical Research Council Unit on the Development & Integration of Behaviour: 1970-1989
- Royal Society Research Professor: 1963-1989
- St. John's College, Cambridge: 1951-1994
- Curator, Ornithological Field Station, Department of Zoology, University of Cambridge: 1950-1964

Major Areas of Work

- Zoology, relationships



SRCD ORAL HISTORY INTERVIEW

Robert A. Hinde
Prof. Emeritus, St. John's College
Cambridge, England

Interviewed by P.J. Turner
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Turner: We'll start with your family background, your childhood experiences. What did your parents do?

Hinde: My father was a doctor. I was the youngest of four children. I was brought up in and lived in Norwich. I went to boarding school at Oundle from the age of 11.

Turner: Is there anything significant about your experiences at school that might be relevant?

Hinde: I think that Oundle School had a very good natural history society of which I was the secretary for a while, and I had two, perhaps three teachers who were very important to me. There was the housemaster and the headmaster who were ornithologists and encouraged and helped me and took me out birdwatching. The wife of the headmaster's son, who came to teach in my last year at Oundle which was the first year of the war, and was very important to me in extending my primarily scientific education and keeping me supplied with books for the next four or five years during the war. To a lesser degree, there was a major who was a retired Indian Army major who changed from shooting tigers to killing butterflies and used to take me on entomological expeditions.

Turner: When you left school, did you go straight to university or did you join the armed forces?

Hinde: I joined the Air Force when I was 17 and I had a brief period driving tea cars to the YMCA and I was called up, I suppose, just when I was 18. Then I went almost immediately on a troop ship to Rhodesia, Zimbabwe, to train as a pilot. I then went to navigator's training in South Africa and came back to fly flying boats in Coastal Command in the Indian Ocean and the Atlantic. I got out of the Air Force in December 1945 and came up to Cambridge in January 1946, so I was able to count the year '45 to '46 as an academic year.

Turner: When you were at Cambridge, were there any particular experiences that were important to your intellectual development?

Hinde: It was a curious time at Cambridge because I was amongst the first ex-servicemen to come up. We all on the whole worked much too hard because we drunk all we wanted to drink during the war. So much of my time at Cambridge was spent working quite hard, and my tutor almost said I was a fool, that I was trying to do better than I deserved. I also spent quite a little time at Cambridge sewage farm, which was a very good place for watching birds. It was a place where many migrants came through. I was fortunate enough to find a bird breeding there which had not bred in UK before and that served to introduce me to other ornithologists working at Cambridge and elsewhere, including W. H. Thorpe who was a lecturer in Cambridge Zoology Department and David Lack who was running the Edward Grey Institute at Oxford. I should have said, I think, that I was studying primarily zoology.

Turner: After your undergraduate days where did you go to do your Ph.D.?

Hinde: I went to work with David Lack at Oxford and did a field study of a bird related to the black-capped chickadee called the great tit in a wood outside Oxford. This was a good time, I spent 18 months watching these birds in a rather lovely wood and was able to finish my Ph.D. in under two years.

Turner: And from there you go right on to—

Hinde: When by good fortune W. H. Thorpe was starting an ornithological field station at Cambridge and asked first Konrad Lorenz and later to a man named Reg Moreau to come and run it. Well, both of them dropped out and so he asked me to come as curator. In 1950 I came back to Cambridge to start the ornithological field station at Madingley. There was just Thorpe and myself and one assistant, and spent most of the first year making bird cages and things like that.

Turner: Can you identify the origins of your interest in child development?

Hinde: This setup that Thorpe made was initially designed to study the relation between instinct and learning. Thorpe chose birds as having well marked instinctive behavior patterns and also marked learning abilities. I worked with birds but I have a debt to Thorpe in that he let me work on what I wanted to work on and didn't coerce me into following his own interests. While I'd been at Oxford, Niko Tinbergen came to Oxford from Holland and I saw a great deal of him while I was there. He was more of a mentor to me than my supervisor, David Lack.

In the mid '50s John Bowlby, the London psychiatrist, was bringing together his ideas on child development. At that time he knew from retrospective evidence only that a period of separation from the mother or disruptive parenting of some sort could produce effects in adolescence, acting-out behavior in adolescence, but he needed experimental evidence. He asked Tinbergen to join his seminar at the Tavistock Clinic in London and Tinbergen felt he was too busy and suggested that I should go instead. That started a relationship with John Bowlby.

John Bowlby was an extraordinary eclectic man. This is especially remarkable since he was a psychoanalyst. There was a seminar that used to meet in the Tavistock Clinic every week consisting of a Freudian analyst, a Jungian, a Skinnerian, a Piagetian, myself as an animal chap, various psychiatric social workers—just a small group of about a dozen of us, entirely theoretically heterogeneous but with a common interest in a problem, the nature of parent-offspring relationships. That was very important for me. Bowlby was picking what was useful to him from different people's brains and this led up to his books on attachment.

Towards the end of the '50s he helped me set up a monkey colony at Cambridge to study the effects of separation experimentally. I was lucky in having help to set up that colony; first of all for Yvette Spencer-Booth, who was very sensitive to the individualities of the monkeys, and subsequently Thelma

Rowell. I worked with them for a number of years until Yvette tragically died young of cancer. Then her place was taken by Linda—God, I've forgotten her name—Linda McGinnis who had been a research student. During that time we did experiments on the effects of separating baby monkeys from their mothers. We found that short periods of separation, a week or two weeks, could produce effects that you could measure two years later. However, the effects varied very much between individuals and, to put it crudely, the better the mother-infant relationship, the less the effects of separation. I felt that I could get my correlation coefficients higher if I could measure the mother-child relationship properly, so I started to read about measuring relationships.

As a biologist I naively thought that I had to read in psychology to find out how to measure relationships. I read for a number of years in psychology, psychiatry, anthropology, sociology, and anything else I could lay my hands on and found—to be frank, was bitterly disappointed in what I found about relationships. There seemed to be no scientific work on human relationships, except for unsystematic clinical material. So I decided that I'd study relationships. One way to find out about something is to write a book about it, so I did my best to synthesize what information I could get in a book called *Understanding Relationships* which was published in 1979. My view then and my view now is that we need to build a science of human relationships which is based on a descriptive base which takes account of the various processes whereby interactions affect interactions within relationships and applies them to the various types of relationships that we find in human societies.

Also in the mid '70s I decided to transfer my total work to children and I started studying preschool children. First with the help of Rosemary Roper and later working in collaboration with my second wife, Joan Stevenson-Hinde. At that time Judy Dunn came and joined my MRC unit and, around that time, and she also was a great stimulus to me. I decided that if I wanted to study relationships the thing to do was to study the first relationships that children form outside the home. There was already a good deal of work on babies and I thought I'd work on a slightly older age group, so I decided to study how the home relationships affected the children's social behavior in preschool. And that's kept me busy up to the late '80s. I'm sorry I'm talking too much.

Turner: That's all right. What do you think are the particular strengths of your research and theoretical approach?

Hinde: Well, let me go back first and say two things that I left out. In the early '70s Louis Leaeay asked Bill Thorpe to look after a student of his who he was establishing to study chimpanzees in Africa and so passed that on to me, and that led to my supervising the work of Jane Goodall and subsequently other students in the Gombe Stream National Park in Tanzania and also to supervising Diane Fossey, who was studying gorilla in Rwanda, and other students who worked at their camps and also to various students who worked on baboons and elephants and other species of monkeys. This was a happy time because it gave me a chance to visit Africa quite a lot. It broadened my perspectives a great deal. I learned a lot about interindividual relationships from these monkeys. Anyway it was a lot of fun. Now what was it you asked me?

Turner: The particular strengths of your research and theoretical contributions.

Hinde: Such a pretentious question. What am I going to say to it? I suppose that I started life as a biologist. I've always wanted to build bridgeheads between disciplines. I dabbled in physiology for a long time. In the early days at Madingley I studied the interrelations between the environmental stimuli and the hormonal state in the behavior of canaries coming into breeding condition and nest building. So that was a certain amount of physiological work. I gave that up because the physiology became more complicated than I could understand and I was more interested in the monkey work. I suppose that the work that I've done with children can be called psychological work, developmental psychology. I'm also interested in building bridgeheads with anthropology and I am currently involved, it so happens at this moment, in writing a paper in collaboration with an archaeologist on the nature of ritual, trying to bring the biological and the social science approaches to ritual together. Of course, I've been involved a little bit in psychiatry through my contacts with John Bowlby and the work on the child relationships

and so on. I would have thought such strength as I've had has really been involved in putting these different disciplines together. In the early days, I was fortunate in being academically a slightly angry young man and being able to yap at the heels of the great men like Lorenz and become one of his critics, because I disagreed with his energy models of motivation and that sort of thing. It was that that really established me and the experimental work related to it as an ornithologist in the '50s.

Turner: Can you say which published articles or books best represent your thinking about child development? Or which of your studies seemed the most significant to you?

Hinde: I don't think I've done really anything very significant in child development. I started to write a textbook on animal behavior jointly with Niko Tinbergen in the '60s, I suppose it was. And Niko had to drop out almost straight away. That book came to fruition, first edition 1966, second 1970, and was, I think, very influential in animal behavior at the time. A lot of the thinking in that I've transferred to work on child development.

I've not written any books specifically about child development, though really primarily because my real interest is in adult relationships rather than in children. I suppose one of the things I've done is to help foster the interest in children's relationships as opposed to their interactions. I should say that I value what I've actually done here should it be misunderstood. It's one of the nice things that I think happens in academic life, at the same time as I was getting interested in relationships so were a number of other people and there's been a flowering of intense interest in the nature of relationships over the last 20 years or so. Though, in my view, a lot of that work has been rather hit and miss, and it needs pulling together to give it the shape of an integrated body of knowledge.

One of the findings that Joan Stevenson-Hinde and I made quite early on was that working with these four-year-old children there was a differential effect of shyness in girls and boys, although girls and boys were almost equally shy. Shy girls got on better with their parents and with their siblings and at school than non-shy girls, whereas with shy boys exactly the opposite was the case. They got on worse at school than non-shy boys and at home. This clearly related to the value systems of the parents and that gave me an interest in the nature of culture and the influence of parental value systems on bringing up children.

The things I've been writing the last ten years or so have been in part concerned with trying to produce an integration between the biologically stable characteristics that are virtually pan-cultural, or *relatively stable characteristics*, the culture from an anthropological point of view and the psychology of development. Sorry, I'm not keeping to the questions.

Turner: That's all right. Have your research and writing been influenced by any particular political and social events?

Hinde: After the war I was happy to come back to Cambridge and sort of bathe in the glamour that was attached at that time to people who'd been pilots in the RAF and so on. My brother was killed in the war; my best friend was killed in the war; many of my contemporaries at school were killed in the war. That undoubtedly has had a great impact on me. In spite of this glamour I think everybody in my generation came away from the war with a certain feeling of guilt at surviving, because of all those who didn't. This slowly came to the top of my mind and I think it was really Vietnam that made me fully conscious of it. I then became active in the campaign for nuclear disarmament. I have actually written quite a lot about war, the nature of war, the false view that it's related to or is caused by individual aggressiveness. I held very strongly to the view that war causes aggression rather than is caused by aggression. I was a vice president of an organization called Ex-Serviceman's CND—CND means Campaign for Nuclear Disarmament—which attempted to take the hippie image out of CND, the hippie image that is always given by the media, and we used to go along with medals and gray-striped suits and ties and whatnot and parade around the cenotaph and that sort of thing. I've organized a conference on the basis of war as an institution and I'm organizing another one this year on a similar theme, sponsored by UNESCO, which will bring together psychiatrists, anthropologists, psychologists,

lawyers, theologians, you name it to speak about the different forces that support war as an institution. But I don't think that politics has influenced my academic work in any other way. Quite a lot of my energy, especially in the last ten years, has been given to writing about war and cooperation and so on.

Turner: OK. Could you comment on your experiences with research funding bodies over the years? For instance, have you been involved in shaping research funding policy?

Hinde: I've been very lucky in that I was made a Royal Society Research Professor in 1963, which secured my salary and had one very curious rule attached to it, which was that you weren't allowed to do anything that you didn't like doing, like teaching or administration or anything of that sort. So I've been fortunate in only having to teach what I wanted to teach and doing virtually no administration. I later also had an MRC unit, Medical Research Council unit, which was small, involved just a few senior colleagues and assistants and that also enabled us to do more or less what we liked. I have sat on MRC Neuroscience committees and on various other committees for the old DSIR organizations, but I've not played a big part in formulating policy.

Turner: Could you briefly describe your role with the MRC, what objectives you pursued or achievements or frustrations you encountered?

Hinde: I think I covered that in campaign research. I haven't played an administrative role in the MRC.

Turner: What about your experiences as a teacher of child development research, for example, in training research workers and supervising Ph.D. students?

Hinde: Well, we've had a number of Ph.D. students working on child development projects through the years that I've been working in child development. The most distinguished, who did much of her work before she got here was, of course, Judy Dunn. I've not taught child development at the undergraduate level, except for giving a few odd lectures each year. My career has been primarily in research and secondarily in teaching, except for the graduate students I've supervised, and that's quite a large number.

Turner: Do you have any experience in so-called applied child development research?

Hinde: No.

Turner: We'll go on now to your experiences with SRCD in particular. When did you join SRCD?

Hinde: I honestly can't remember. About ten years ago or so.

Turner: And what were your earliest contacts with the Society and with whom in particular?

Hinde: I've simply been to SRCD meetings. I've never been in very close contact with SRCD.

Turner: And could you describe the first biennial meeting you attended?

Hinde: Oh, Jesus. I'd have to ask Joan. I can't remember when it was. I suppose it was—no, I really can't remember. I was persuaded to stand as president at the last election, and heartily and unbelievably thankful that Glen Elder was elected, because I should absolutely hate to do the job.

Turner: Have you been involved in any other aspects of the SRCD, for example, in publications of the Society?

Hinde: No, no.

Turner: Do you think that any important changes have occurred in SRCD during the time—

Hinde: I'm not competent to comment.

Turner: Could you comment a little—I think we've covered most of this but—on the history of your field during the years that you've participated in it?

Hinde: Well, I'm—as will be apparent from what I've already said, I've dabbled in different fields. And I don't feel as though I have been in child development long enough to give a profound answer to that question. What I would say is that psychology as a whole has achieved respectability by trying to ape physics and by the use of statistical methods. I think both of those things have been overdone. Psychology's quite a different sort of science than physics, something we ethologists have been saying for years. The statistical techniques the norms developer in themselves can carry attention away from the complexity of individual people and lead one to forget that individuals function as integrated wholes actively acting upon, as well as being acted upon, actively acting on their environment as well as being acted on by it. I'm pleased to see a growing recognition of the usefulness of the ethological four whys, namely the whys of causation, development, function, evolution. Functional and evolutionary aspects are beginning to come into the consideration of issues in child development. I think that's a very healthy sign. I don't like the suggestions that, so often made by editors, that papers should be theory oriented and should pose questions that they then proceed to solve. A lot of that sort of thing is phony. Some of the most interesting issues come from observation, ordered and consistent observation, which then leads to hypotheses.

Turner: What are your hopes for the future of the field?

Hinde: Well I hope that it will increasingly—I don't know what you mean by the field in this context. What I feel as though I've given my life to is really trying to integrate these different sciences and I very much hope that there will be a closer integration, especially, between the various behavioral sciences. I think, to give just one example, the cross-cultural and developmental psychologists look at the role of differences and norms and values in institutions between different cultures, but the way in which those norms and values operate are exactly the same as the way in which the norms and values that are idiosyncratic to individuals operate. And yet the one is studied by sociologists and anthropologists and the other by developmental psychologists and social psychologists. There's a great need to bring these different behavioral sciences together.

Turner: And, finally, is there anything about your personal interests and your family interests which may have had a bearing on your scientific contributions?

Hinde: Well I think that my natural history interests led me to biological interests which led to psychological interests in the way in which I've described. Undoubtedly having children of my own, and I have six by two marriages, made a great deal of difference to my interest in children. I don't think I have anything more to comment there.

Turner: Is there anything else that you'd like to add?

Hinde: I think if there was ever a golden age for research, I caught it. I got into research in 1950 and I got out of empirical research effectively in 1989, and that was just a wonderful time. So I've been fortunate in that respect. I've been very fortunate, as I've mentioned, both in school teachers who influenced me at that stage and in falling into exactly the right job for me after the war, in having Timbergen to help me and influence me when I was a graduate student and in the immediately succeeding years, in having Thorpe as a boss who was a very patient person who allowed me to do what I wanted to do, having had a wonderful series of colleagues and students here, especially being able to work with Pat Bateson over so many years, and having learned so much from women colleagues about the importance of individual differences—since my role with Jane Goodall was to teach her to research systematically and she taught me just as much about the individuality of the chimpanzees she was

watching—and I think I learned a great deal from Thelma Rowell, Diane Fossey, Jane and various others that have really quite changed my perspective on my own work. I've also been lucky in having easy funding, getting the Roth Society Research Professorship relatively early on, which I didn't even apply for, and a sympathetic Medical Research Council. It was days when it was relatively easy to get funds and my heart goes out to good students nowadays who find it sort of impossible even to get jobs.

Finally I would like to say that I was extremely grateful to the American Psychological Association over the award of the G. Stanley Hall medal this year. I realize that I am joining an extremely distinguished group of recipients of this medal and I am very grateful to the developmental section for thinking of me in this context. The award means a great deal to me.

[end of interview]