

Hugh Lytton

- Born 9/26/1921 in Nuremberg, Germany; died 4/16/2002
- B.A. Hon. in German (1941) University of London, B.A. Hon. in French (1949) University of London, M.A. in Psychology (1953) University of Liverpool, Ph.D. in Psychology (1966) University of London



Major Employment

- Professor Emeritus of Educational Psychology, University of Calgary: 1988-death
- Professor of Educational Psychology, University of Calgary: 1973-1991
- Lecturer in Educational Psychology, University of Exeter: 1963-1969
- School and Clinical Child Psychologist, Midlothian, Scotland: 1957-1962
- Teacher at secondary and primary schools: 1948-1955

Major Areas of Work

- Socialization, parent-child relations

SRCD Affiliation

- Member

SRCD ORAL HISTORY INTERVIEW

Hugh Lytton

Self-Interview
June 4, 1994

Describe your family background along with any childhood and adolescent experiences that may be of interest. Include the educational and occupational characteristics of your parents. Where were you born, grew up? What was the schooling like? Any military experience? Early work experience?

Lytton: I was born in Nuremberg, Germany, and I come from a lower middle class family. My father was an office clerk and my mother worked from the time that I was three onwards as a secretary, not as an act of liberation and choice, but by necessity, and I must say I remember it with great bitterness and I felt very badly about it. I was growing up a German Jew and my first high school was in Germany, in Nuremberg and there I was brought up with a very classical education learning Latin and Greek and Hebrew on the side. When I was 15 in 1936, Hitler and the Nazis had been in power in Germany for three years and therefore when the opportunity arose for me to continue schooling in England, my parents snapped it up with enthusiasm and I went to school from the age of 15 onwards in England. There my education changed completely as this was more of a technical/scientific kind of school rather than the classic humanistic high school that I attended in Germany. My early university education also occurred in Hull and in Nottingham and my first two BA degrees centered on modern languages, German and French in both of which I obtained an Honors Degree. In fact, I started my university life in October, 1939, just as war had broken out. The war indeed later on interrupted my studies very badly, essentially from 1940 onwards. I later on did war service in the British Army and was in active service in France and Germany from 1944 on. After the war ended I was retained in the Army as an occupying soldier and was released from the Army in early 1947. At that time, I returned to England and finished my education and training as a teacher of modern languages. However, as I realize by having just reread a diary from those years, even then perhaps I wanted to go into psychology to explain myself, as so many people do, to find something out about my own insecurities. Also I was, at that time, particularly interested in language problem, having access to a fluent

vocabulary. I was interested in the kind of psychological processes that went into language production and hence this introduced me into psycholinguistic research at first.

Before I started my actual teaching career, I had gotten married and we had a family, so my work had to take first place; indeed I taught school in a number of high schools for seven years, but I devoted some of my spare time to, first of all, taking some general courses and doing some work in psychology and then to carving out a thesis for a Master's Degree under Professor Leslie Hearnshaw at the University of Liverpool in England. My Master's thesis was in fact concerned with language learning; that is, with foreign language learning as compared with learning one's mother tongue. Although this was the result of my psycholinguistic interests, I soon became dissatisfied with what I couldn't find out at that time and I now realize that the neurological knowledge and the actual neurologic and brain connections for language production that I was looking for were not known in those days and are only slowly becoming known now. So this linguistic psychology was left behind me. It was a kind of unorthodox entry into psychology, a detour in a way. Shortly afterwards I decided to take up psychology in a practical way, mainly as an educational psychologist as they are called in England, which means school and clinical child psychologists in modern American terms. I was admitted for training to the Tavistock Clinic in London which is very well known for its psychoanalytic outlook and indeed I always think that its reputation increases in proportion to the square of the distance one is away from it. In any case, it was the only full time training I have in psychology and it lasted exactly one year. All of my other training and knowledge in psychology I acquired by part-time study. While I was at the Tavistock Clinic being trained as an educational psychologist we were swamped with psychoanalytic concepts and I'm afraid the psychoanalytic approach, its Freud-driven and authority-based dogmas, offended my academic and scientific tendencies. I kept saying, more or less, that I was having as counterbalance contact with Professor Vernon, who then already began to supervise my Ph.D. I should say that in England clinical and school psychology training is completely separate from ones' Ph.D. which is a research enterprise. The one influence that stayed with me from the Tavistock Clinic that I appreciated was that of John Bowlby who was the director of the Child Department at the time and I do remember having classes and discussion groups where he explained his attachment theory that then was entirely new to me and that has since become so famous. Indeed this theory did influence me perhaps more than any other part of the Tavistock and I do remember also that John Bowlby, who on the whole had a fairly rational mind and to some extent a scientifically oriented mind, found himself in frequent conflict with the more ideology and authority-driven psychiatrists who were his colleagues at the Tavistock Clinic. I worked as a school and clinical child psychologist after my training and I did this work in and around Edinburgh, Scotland, for about six years. Again on the side, I started a Ph.D. degree which had begun earlier under Philip Vernon. My topic then was driven by the practical considerations of a school psychologist, namely I had to organize remedial education for children who were not doing well in school but who were not generally intellectually sufficiently retarded to go into a special school for the mentally handicapped. So my topic for the Ph.D. was the question of selection for remedial education, namely whether it should be based on discrepancy between performance and IQ promise or whether it should ignore IQ and the discrepancy I just mentioned. My results did suggest that in reading one could indeed safely ignore this discrepancy and the children would make just as much progress if IQ was not taken into account in selecting them for remedial education, but in arithmetic it did make a difference. Essentially this said that IQ was not as important a predictor for everything as some people claim. Even Philip Vernon made this point; in fact he was particularly keen to make it.

What are the ages of your interest in child development? What individuals were important to your intellectual development? Who are your research mentors or significant colleagues?

Lytton: I think I was drawn into the study of child development by my experience with psychoanalytic methodology at the Tavistock Clinic. Although I rebelled against it and just about escaped being brain-washed by very irrational views, I did appreciate the importance they attached to direct observation of children and parents. And of course I was also inspired to try and find some non-psychoanalytic reasons for the development of various children's characteristics in particular children's pathologies. At the Tavistock we were in fact required as a part of the course to observe one mother and very young

infant pair and some of us in particular wanted to observe mother nursing her child as this was considered a specially significant experience. At that time, of course, I did not know what I was looking for and ideas of how such observations could be used in the study of children's actual development came much later. Bowlby, I think, was important to the formation of my views and I was very attracted to his attachment theory even though this theory far outran his data. I also appreciated Mary Ainsworth's work particularly on maternal deprivation. This kind of approach indeed helped me a great deal in understanding or thinking that I understood the kinds of maladjustment that I came across in my practice as clinic child psychologist later on.

My early research mentor was Philip Vernon whose integrity and honesty and very careful data-based work I greatly admired and under whom I completed my Ph.D. thesis in 1965. As for people who came later in my life and in my career, Robert Sears, etc., were people whose interest I share and whom I admire, in many ways, in their attempts to understand parent-child relations. I also had a certain amount of common ground with ethologists such as Robert Hinde, Nick Blurton-Jones. It is not that I completely agreed with their theories in general but I certainly found their methods most congenial as they insisted particularly on the direct observation of children and parents and as I thought and still think that any soundly based theory on child development and parental influences on child development must be based on data that come directly from those children and those parents and not from artificial situations such as either surveys, questionnaires, interviews or experiments. This does not mean that I exclude the latter methods altogether.

What political and social events have influenced your research and writing and teaching?

Lytton: Political and social events, I think, did not influence the content of my work, at least I'm persuaded of this. They did influence my actions and above all what I could not do. In my early years in England, after the war and during the war, the fact that I was a refugee from Germany and the war itself and the interruptions that it brought limited my early opportunities and, in fact, kept me away from psychology. Also of course the fact that my life was completely uprooted and overturned by Nazism influenced my attitudes. So my attitudes became rather left leaning. I certainly was against racism for equality between different groups and for the righting of injustices that the more oppressed people of most countries have to bear. Nevertheless, I still was always convinced of the importance of nature; that is, of genetic influences that have an effect on human development.

What were your primary interests in child development at the beginning of your career?

Lytton: My interest, I think, was often determined by practical problems in my clinical work. So for instance, I became interested in remediation of reading difficulties, how this could be made most effective, how children could be selected for remedial classes and various ramifications of reading difficulties. I had also been interested, even before I started this particular work, in the question of creativity or divergent thinking, how such a category of thinking differs from analytical intelligence that is tested by IQ. I wrote a book on the topic of creativity in 1970.

What continuities in your work are most significant?

Lytton: I think my main, underlying and longtime interest has always been the relations between parents and children and the influence that the parents have on their children through their rearing techniques and socialization practices. I realized, however, from the start that this influence is not a one way street, that children also influence their parents by their natural gives; in other words, by genetic influences that are present within them and by the kinds of reactions that they present to their parents.

What shifts have occurred and what events were responsible?

Lytton: I think the main shift in my life occurred through my move to Calgary in Canada which happened in 1969 when I was nearly 48 years old, so basically late in life. It was only then that I was

able to pursue research consistently and on a fairly modest but at least consistent scale with the help of research funding from the Social Sciences and Humanities Research Council of Canada. This research from the start in Calgary centered on parent-child relations and the area, of course, had been of interest to me before and I thought about how one could investigate this at a concrete level and in a useful way, but the actual work in the area with the help of funding and research assistants only started in 1970.

Reflect on the strengths and weaknesses of your research and theoretical contributions, the impact of your work and its current status.

Lytton: I think the strength of my work has lain mainly in its careful methodology, in the accuracy in data collection and analysis and, I hope, the accuracy of reporting other people's findings. In fact, I am a little obsessional and being obsessed I think helps in research. Another strength was that I was willing to adopt new methodologies and pioneer them. The ongoing purpose in my research has also, I think, meant a certain strength in it and if one wants to express this ongoing purpose grandiosely, one could say that I've always had the idea in mind that I should elucidate the interaction between environment and genes, that is discover things about genetic influence working in concert with environmental factors in children's development.

The weakness of my work, I think, has lain in the fact that it was often too data bound and that I was so afraid of not giving an accurate reflection of what I found or what other people found that I constantly ran into saying, "Yes, this on the one hand, but on the other hand the other is true as well," and I was too timid to draw wider conclusions, more sweeping conclusions and this I suppose is my personality at work. I think people have generally been interested more in my methodology, direct observation of children and parents more than in my ideas, and hence I found that my earlier work in the 1970s on direct observation of parent-child interaction was received with a greater interest than the subsequent follow-up which did not have this particular aspect as part of it anymore. Meanwhile, I have moved on in methods, to meta-analysis as a matter of fact, and as regards to the impact of my work I think what has been very interesting and also disillusioning to me has been the impact of an article about three years ago that was published in *Psychological Bulletin* and therefore very widely circulated. The article dealt with meta-analysis, that is, quantitative integration of studies that investigated differential rearing of boys versus girls, i.e. differential rearing based on sex, and the overall outcome was that parents make really very little systematic differences, almost none I would say. Now this article of course goes against people's ingrained ideas and common sense and hence it has been cited relatively little, and what I find even more disillusioning was the fact that when I do find it cited, sometimes in manuscripts that are submitted to me for review, I find that people ignore its main message or distort its main message and simply get out of it what they want to get out of it, namely the one area which displays some sex based differentiation, namely the encouragement of sex typed activities at play or household chores and so forth.

What unpublished manuscripts best represent your thinking about child development and which of your studies seem most significant?

Lytton: I think that my book on parent-child socialization represents a solid kind of contribution on the actual interactions that go on in daily living between parents and their two-year-old children and show how parents do influence children to some extent. That is a book that is not very exciting, full of method and full of details and fails to deliver a strong message. More recent work that does have a stronger tendency and a clear message is, first of all, the meta-analysis that I mentioned earlier on parents' differential rearing of boys and girls and then also a review article on child and parent effects in boys' conduct disorder. This article in *Developmental Psychology* tries to reinterpret the evidence in the literature. The literature has generally attributed the development of conduct disorder and extreme aggression in children to parents' misdeeds, essentially to parents lack of warmth, rejections, harsh punishments, inconsistency and to social factors such as poverty or also social structural factors such as divorced families. What I tried to do was to show that these factors simply play a part; such a view is one sided in that it leaves out of account the large part that is played by genetic factors,

temperament and early biological factors that are present in the child. I have also tried to clarify my ideas on the relative influences of parent socialization and child inborn factors, overall, how also the two different kinds of factors are seen as more or less important in different designs of the investigation. This thinking has been published in an internal University of Calgary publication but is not very well formed and I am hoping to revise it for further publication later.

What is most wrong-headed about my contributions?

Lytton: I can't think of anything that I would now consider completely wrong-headed. Perhaps this is rather delusional; nevertheless this is how it is, but I do think that my early publications were immature both in their conceptual thinking and also because they were not statistically very sophisticated. My statistical adaptation and really training took place mainly after I came to Calgary.

Reflect on your experiences with the research funding operators over the years, including your participation in shaping research funding policy as implementation, securing support for your own work, etc.

Lytton: My experience with research funding agencies has been almost entirely with Canadian funding agencies and in particular with the former Canada Council and later the Social Sciences and Humanities Research Council of Canada. I have had very good treatment from them and I found that their treatment of applications was fair and on the whole generous. There are certain smaller irritants and dysfunctional aspects of this, for instance, the council was rather restrictive in not allowing investigators to make their own informed decisions on how to allocate moneys within the grant to different tasks. And above all not to allow registered research through graduate students, to be both graduate students and research assistants at the same time. This I think had a stultifying effect as the people most concerned with the investigators were not allowed to work for the project itself. These policies I think have now been relaxed and therefore improved. I was also a member of Social Science Research Council's Committee on Strategic Research into Family and the Socialization of Children. This is one of several Strategic Research Committees and in them we certainly discussed research policy, although my own part was minute. It seemed to me at the time that the aims of this particular Strategic Research Committee were vague and they tended to favor fragmented research all over the place and they did not attract very good applications. The general attitude was everybody must be allowed to play a part in this and hence the committee simply sat back and waited for applications to come in. They did not play an active, stimulating role in trying to elicit programs that would have a certain purpose. From this point of view I feel that the advent of some new research consortium such as the Catherine and John MacArthur Foundation has been of great benefit for the advancement of knowledge. Not necessarily for individual scholars who may not receive funding from them, as I have not, but with their problematic research and the stimulation of a whole sequence of research studies by knowledgeable people, people known to be able to produce meaningful and solid research has certainly made for a revolution in research in a way. I think it has given us more of a chance of making a dent in our vast ignorance in the social science area and child development in particular.

The institutions that I have worked in.

Lytton: I started my professional career in 1948 as a teacher in mainly high schools and followed this up again by training in school and clinical child psychology. My first job in psychology was as a school and clinical child psychologist in Edinburgh, Scotland, from 1957 to 1962. I then was recruited by the University of Exeter, England, where I was the equivalent of an assistant and then associate professor in educational psychology. In 1969 I moved to Calgary where I was first associate and then full professor from 1969 onward to the time that I retired, first partially in 1988 and then fully in 1991.

Describe your experiences as a teacher of child development research and training of research workers. What courses have you taught? Comment on the tension between teaching and research in the field of child development.

Lytton: As a teacher of graduate students I taught mainly future school psychologists, counselors or mental health clinical psychologists, that is, I taught practitioners. I hardly taught any research workers in child psychology or child development at all and this is one source of regret for me. As a teacher of undergraduate students I taught mainly intending teachers. The courses that I taught were for undergraduate and for graduate students. They were, in later years, almost all devoted to child development up to adolescence and they were devoted more specifically to social child development in the graduate courses. In addition, I taught a course in research design and methods which I hoped clarified the general aims of research and the general approaches to research to students. As regards to tension between teaching and research, I found none, except for how to divide one's time. That is, the only tension was in myself knowing and deciding how much time I could devote to research and how much time I could devote to teaching.

Describe your experiences in so called applied child development research or on applied work. Comment also on your role in putting theory into practice.

Lytton: Almost none of my research has in fact been experimental, that is it has all been done in the field on real life problems either in the home or sometimes by testing in schools. If I refer to applied child development research as research that is meant to find solutions to practical problems then my research, a lot of my research has gone into such areas. One is the research on reading retardation and the remediation of reading retardation. Another area that I touched on has been how to encourage creativity in children and more recently I have begun to look into the relations between family factors and conduct disorder. I have also been interested in day care, although I have not done any work in it. My comment would be mainly that, as everybody knows, it's quite difficult to apply any rigor in research like this; nevertheless, I think that we should certainly attempt to be as rigorous and as concrete in detail as we can and as accurate and as documented as we can. That's the research areas that really do make an impact. I also feel that in these areas ideology perhaps plays a greater part than in the many more abstract research areas. As regard to applied work, I of course tried to apply some psychological knowledge when I worked as a school and clinical child psychologist. I'm not sure that I applied many theories directly. I think I was inspired at that time in the '50s by vague theories of play oriented and child centered education. Therefore I probably was a kind of progressive influence in Scottish education since Scottish education has never stopped emphasizing the basics and has been called appallingly efficient. Reading is taught from the first day of school when children enter school even at age five. Other theories that influenced my work I suppose was Attachment Theory as I mentioned earlier, which explained some phenomena, or so it seemed to me, but it did not provide any remedies. Behavior modification, on the other hand, had not reached me yet in the '50s.

When did you join SRCD? What were your earliest contacts with the Society and with whom?

Lytton: My first contact with the Society was in 1967 when I came to the US from the UK on a tour of various counseling and child development centers at the time that I was interested in both counseling and child development. I attended the meeting in New York in that year, invited and introduced by Paul Mussen. I'm afraid I remember very little about it now apart from the fact that I first met Diana Baumrind at that time. I actually joined the Society myself only when I came to Canada in 1969 or thereabouts.

Describe the history of your participation in the scientific meetings and publications of the Society or in other aspects of the work at the Society.

Lytton: I have of course published in the Society's journal but by no means as often as I would have wished as many other people will no doubt say. I have not been on the editorial board of *Child Development* but I have been on the editorial board of *Developmental Psychology* where I worked under Marion Radke-Yarrow. This was a good and interesting experience and I also found it quite interesting to see many articles come in when I was a casual editorial consultant after my service ended on the editorial board. I don't think I shaped the policy of any journal, or any general publication policy. However, I do want to say in the light of some experiences that I have had in more

recent years I think that it is very important for editors to be on the alert for rather dubious material. That is, material that contains doubtful data, doubtful because it is not clear whether they were obtained accurately and honestly. This is a problem that applies I think more to the less prestigious journals but it is a problem that I have also come across in being a reviewer for *Developmental Psychology*.

Has there been in a change in the nature of publications?

Lytton: Without having done a full scientific empirical study, I would say that publications have become more rigorous over the years. First of all, they've become more sophisticated in their method-collection technology as well as in their statistical analysis, and the latter I think is due not only to training in statistics but also simply to the presence and availability of the computer. One should remember that in the '50s for instance I had to crank out correlations and factor analysis on a crank-handled calculator that wasn't even electrically driven. The second change, I think have been more observational studies of children in the journals and proportionately less experimental ones, but we must remember that in absolute numbers there no doubt were more experimental studies than previously over the years simply due to the explosion in the numbers of good studies. My third observation about change in publications is that in an impressionistic way I noticed waves of studies triggered by a given investigative method that had been invented and that had come into vogue. An example is the studies trying to replicate Piaget and asking whether the age at which children attain conservation, for instance, cannot be lowered and the process accelerated. The second example is the rate of studies into the security of children's attachment which took off like a rocket with the development of the strange situation by Mary Ainsworth. I suppose this is normal science in a way as defined by Kuhn, namely people explore the boundaries of a phenomenon. They dot the Is, they cross the Ts and they use accepted methods until some new well accepted method comes into public focus; above all a problem that has a method that can fairly rapidly be learned and applied. I also want to say that qualitative research as it has recently sprouted is to a large extent, in my opinion, destructive of any advance in knowledge apart from simply being an adjunct to quantitative research where of course this is very necessary. But qualitative research as such has barely touched the prestigious child development journals that I have been in contact with.

Comment on the history of the field during the years that you have participated in it. Have your views concerning the importance of various issues changed over the years and how?

Lytton: Part of the changes in the field I have discussed in the answer to the previous question on developments in publications. There are some other points I can make. I feel I am in a predominantly female field of developmental psychology and hence I feel that I'm engaged in cross-sex activity almost like cross-sex play which does me good and probably says something about my personality. I feel it has become more female dominated over the years and positions of influence, for instance editorships, are nowadays more occupied by females as indeed befits their numbers in the area. Another point is there has been a movement away from purely environmental influence and a greater recognition of the pervasive influence of biology and genes which is becoming more pronounced almost every month. I would say that I myself at first put greater emphasis on environmental influences. For instance, I followed Bowlby's view that delinquency as psychopathology arises from maternal deprivation in early childhood. I now make more allowance for possible genetic factors in the dysfunctional families that lead to the child being deprived of mother's presence, attention and affections and hence lead to this possible delinquency. Overall my views haven't changed much. I feel vindicated by movement towards my views, but I feel less optimistic or perhaps less naive about the possibility of proving empirically any psychological position.

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

Lytton: I see more how preconceived notions sway all of us and guide the bigger picture and conclusions once we have gone away from our data sheets and are more free to speculate and give way to our preconceptions. I think proof of any psychological relationships and causations will come more

from molecular biology than from psychology. Nevertheless psychology's level of explanation at the level of behavior is something that has to be maintained. It's different from that of the biological sciences but psychological theories and explanations, if they want to be viable, cannot be at variance with biological findings.

Please tell us something about your personal interests and your family, especially the ways in which they may have had a bearing on your scientific interests and contributions and your applied contributions.

Lytton: My son is a molecular biologist and my daughter is a psychologist; that is, she is an educational and clinical child psychologist in England the same way that I was, hence I very much uphold the intersection between the two fields. I think my children have influenced me very much in the sense that their development has interested me and I have learned simply from their unfolding. I am quite happy with both of them, but the same as I do not blame parents for the misdeeds and deviancy and delinquency of their children, I do not want to take credit for the good development of my children. This too is something that is often outside the control of parents. I would say that I have had enormous support from my wife who, although she has great intellectual potential, in my view, has in fact been satisfied, at least in practice, although not in her mind, with looking after the family and not seeking a career of her own. This has been partly due to the kind of generation that we grew up in and the kind of expectations that were held of mothers and fathers at that time. But it is not only that, we also both believed that it was important for our children to be brought up in the loving environment that their mother can create. I myself was personally influenced in fact by the rather negative, bitter memories of my own mother going out to work and I wanted to avoid the same kind of situation in my family. In addition, my late entry into the psychological profession because of my personal history and the war has meant that I had to work hard in order to gain a foothold in the profession which meant that I had very little time for devoting myself to other family tasks as indeed my son and my son-in-law do and this has put a certain restriction on my wife's ability to strike out on her own. These family relationships may have influenced to some extent my views on mother-care and other-care. I do see the usefulness and the inevitability of day care, for instance, either in other families by baby sitters or in groups. However, I have some doubts about whether some of the benefits that have been widely written about were not discovered mainly by psychologist mothers who themselves had to take advantage of day care in some form.