

Sir Michael Rutter

- Born 08/15/1933 in the United Kingdom
- M.D. from the University of Birmingham Medical School (1963); Academic Diploma in Psychological Medicine in London, England (1961); MRCS (Member of the Royal College of Surgeons) in England (1955)

Major Employment:

- Institute of Psychiatry, University of London - 1973-1998, Child Psychiatry and Developmental Psychopathology
- Social, Genetic, and Developmental Psychiatry Center - 1994-1998, Honorary Director
- Virginia Commonwealth University - 1998-present, Clinical Professor of Human Genetics

Major Areas of Work:

- Developmental Psychopathology, Conduct Disorders, Genetics of Autism, Antisocial Behavior, Psychosocial Risk

SRCD Affiliation:

- Governing Council Member (1997-2003); President (1999-2001)



SRCD ORAL HISTORY INTERVIEW

Michael Rutter

Interviewed by Judy Dunn
At the Institute of Psychiatry, London, United Kingdom
March 8, 2004

Dunn: The oral history interview with Professor Sir Michael Rutter interviewed by Judy Dunn at the Institute of Psychiatry, March 8th, 2004. So Mike, you come from a family of doctors--grandparents and parents who were doctors. Where did you grow up in England?

Rutter: Well, all over the place I suppose is the answer. I was born in Lebanon when my father was doing a stint out there running a mission hospital in Brumanna, which is in the hills above Beirut. And so I was there until I was about 3 ½, then came back to England living in the Midlands. Then the war came. I went on my seventh birthday literally to the States, and I had four years in the States, or nearly four years, with an American family who neither my parents nor I knew in advance; although there were indirect connections through friends of friends, so we knew quite a lot about them, and they knew quite a lot about us. Then in 1944 I came back to the UK again in the Midlands, and moved around a bit until I came to London in '58 to undertake my psychiatric training.

Dunn: So quite a moveable set of experiences.

Rutter: Yes.

Dunn: Do you think that has made a difference to your interest in early experience and what happens to children?

Rutter: Well, not directly I think. But what it certainly did do is help me appreciate that there are a variety of different styles of family, different cultures, each which had its own strengths and limitations, and that I was reasonably comfortable in moving around (not, I have to say, in terms of fluency with languages, which is not a skill of mine). My time in the United States was a happy time for me. People always think that it must have been a terrible stress leaving my family to go to the States, but I don't actually recall it like that.

Dunn: But you went without your parents?

Rutter: Yes, and I did not see them for nearly four years; that was a major stress for my parents, but not for me. The family I was with did a wonderful job of keeping me in touch with my parents. I know it sounds an odd thing to say, but I never really felt apart from my parents. I simply had two families for four years. My American parents knew a lot about my family because they made an effort to find out. Also, letters came through in a sporadic fashion; I mean, my parents wrote every week and so did I, but with the war on they would come and go. So I felt part of things, and going back in 1944 was a very easy transition for me. Of course the war was still on, although it seemed about to end, but then the rockets started, and it definitely wasn't over for a while. My parents were exactly as I remembered them, and for me it was easy to be with them again. It wasn't so straightforward for my parents, however, because I had grown up and in so doing, had changed. I wasn't the way they remembered me, so much so that my grandmother, who was living with us at the time, on the evening that Priscilla (my sister) and I returned took my mother off to the side and said, "Winifred, do you think we got the right ones back?" I'd gone a quiet well-behaved English child, and I came back a rather assertively American adolescent; so I was different.

Dunn: Yes. That's interesting. And then you went to Birmingham Medical School after completing secondary (high) school in the Midlands and then in York.

Rutter: Yes.

Dunn: What kind of an experience was that? Were there pointers towards your interest in developmental principles and psychiatry early on?

Rutter: Not really, although actually we need to go back a little bit earlier than that. In my time at school between returning from the States and going to medical school, I was brought into contact with psychology by my physics master at school. He alerted me to the workings of Freud at the boarding school I attended in York (in the North of England). I read a fair bit of Freud during my four years in York. I became particularly intrigued by what he wrote about cognitive processes. I, even in those days, was very unimpressed by Freud's developmental theory, which seemed to me to make no sense at all, and I didn't like his rigid way of thinking about things. On the other hand, what he wrote about the errors that people made in their thought processes and ways in which they gave meaning to their experiences, that did ring bells. So I went through a period when I trained myself to wake up after every dream and got a bit sleep deprived doing that. It was very interesting, so what I did was I would write down the dream in a half waking state, and then go back to sleep, and so I did become very interested in that then. And although that led to no direct effects later, the notion of thinking about the importance of what people did with their experiences remained with me and still does. Let me return to your questions about medical school. At the time I went into medical school I was going to be a GP. My father was a GP, or he was until he became a regional medical officer. My grandfather was a GP until he also became a regional medical officer, and that was what I wanted to do. I certainly had no idea of psychiatry and no interest in research at that

time. But then, during my time at medical school I did become very interested in brain/mind relationships, partly through reading Grey Walter's book. I didn't agree with a lot of what he said, but on the other hand, he had interesting ideas about what brain and mind were all about. Through clinical work in neurology, neurosurgery, and psychiatry (I spent quite a bit of extra time in all three) I became very interested in brain and mind interconnections. Midway through my clinical training I decided I did want to do psychiatry. It seemed to me intellectually interesting and challenging. A particular turning point came with an elective I did with Willi Mayer-Gross, who'd been professor at Heidelberg, and then came as a refugee here at the time of the Nazi persecution. He was not well treated I think by the UK, although he landed up with a university post in Birmingham his clinical work was in a pretty dreadful mental hospital. He took medical students for electives, and I went on one of those. My first stands out as an awful hour, but one that he translated into a wonderful thing. His practice was to give each student a patient from one of the backwards, and you had an hour in which to take a history and make a diagnostic assessment. You knew the patient's name and age, and that's it. At the end you had to present the assessment to him. I had this gentleman I couldn't make heads or tails of. He couldn't provide a coherent account of himself, I couldn't elicit a history and I didn't know what to make of his mental state. It was a dreadful hour. I made no sense of what I'd heard and seen, and I thought the only thing to do was to confess my total failure. So I just said, "I'm very sorry, Professor Mayer-Gross, but it was a complete waste of time, I've not got anything out of this, and I've no idea what the problem is." And so he said, "Well let's just see, take me through what happened." I did just that and he showed me that I had actually made all the relevant observations. The patient had hebephrenic schizophrenia with gross thought disorder, but I hadn't understood the significance of what he was showing in his thought processes. Professor Mayer-Gross used all my observations and showed me their significance, translating my failure into success – albeit a pseudo success. He was a wonderful teacher, and he certainly excited my interest in psychiatry. After I spent more time with him, he said I should go to the Maudsley Hospital for training, but that I should train in general medicine and neurology first, which I did. That was, I suppose, about two thirds of the way through medical school. But I'd also become interested in Tom McKeown's teaching during that time. He was professor in, I think, social medicine. In a very unusual way, he had his time teaching medical students in parallel with anatomy, biochemistry, and physiology. Then we learned about public health and epidemiology right from the very beginning, as well as how you applied knowledge in these fields to individual patients. So I became very interested at that time not with developmental issues, but with the impact of the social environment on people's functioning, which had a lasting effect on my thinking.

Dunn: So quite early in medical school you were interested in the links between basic science and clinical conditions. And when you came to the Institute of Psychiatry, one of the people who was an important mentor for you was Aubrey Lewis.

Rutter: Yes.

Dunn: What are the encounters with him that you thought were formative? Was it discussion over individual cases or things that you yourself had noticed like these observations you've made on the first patient you saw?

Rutter: Yes to all of that. He was a wonderful personality and a tremendous teacher, although somewhat frightening. I remained frightened of him throughout my time, but I was always invigorated and excited by his teaching, and I enjoyed being made to think. Although some of the trainees didn't see him like that, he was actually very supportive, and he wanted you to succeed. There was never any competitiveness and there's nothing that pleased him more than you winning an argument with him. It didn't often happen, but it was as much a pleasure to him as it was to me or to anyone else. In terms of interactions in relation to patients, my approach to Aubrey was colored by my previous experience in cardiology where the teacher who impressed me most was Paul Wood; he was the top of the field at that time. In presenting

cases to him, he insisted that you work your way through your history-taking, your observations, and your thought processes. At each point you, as it were, had to present the logic of where you were going with the next step, and he would tie it all together in terms of the underlying physiology as assessed by the technology available at the time. It was really applying science to the individual and as a way of looking at mechanisms that was just awe inspiring. He was wonderful at it. He was usually right, but actually he was as interesting when he was wrong as when he was right because he would force himself (and you) to go back asking why you had landed up with the wrong answer: were your observations wrong, were your inferences wrong and so on. I came away feeling that's really what I would like to be able to do in psychology. Aubrey Lewis would, in a different way, attempt to do the same. He wasn't talking about physiology for the most part, but he would seek to integrate the science with the findings on an individual. That meant that the discussions over individual patients were always interesting, often provoking research questions as well as clinical applications. It was always hypothesis testing from the word go. He was always concerned to bring out the features in common with, as well as those that were different from other patients. You needed to land up, as it were, not only with a diagnosis, but with a prognosis and a set of mechanisms that would lead into treatment. But it was also very individual-oriented. Aubrey was a polymath. If a patient was a member of an obscure religion or was interested in poetry, his eyes lit up because he knew an immense amount about an amazing array of things. I don't remember him coming across a topic that he didn't know about, so that what was individual about patients was as much an interest to him as what they had in common with others. So that was one aspect. The second would be the journal clubs in which he selected the research papers to be considered as well as the student to deal with each paper. Nowadays it's all democratic and students choose for themselves, but the ones that I had from him actually fired my interest immensely because he was better in his judgment than I was. One was a paper on sensory deprivation using experiments that involved putting people into tanks of water and what the lack of simulation did to their thought processes. That set of studies got me interested in deprivation much more generally. The other paper that stands out in my memory was by Buck and Laughton, an epidemiological study looking at the connections between mental illness and physical illness in parents' and illnesses in their children.

Dunn: And that's what you did your doctoral thesis on, is it not?

Rutter: Yes, that's right. Aubrey Lewis was very interesting to talk to on the many issues that their study raised as well as involved in my own research. That played an important role in beginning to shape my career in research. He was extremely challenging on whether you were doing things in the right way, but critically, you had total freedom to disregard his advice. You did that at your peril, but the choice was real. Over the years I did do a few things that he advised against, but that was fine by him and fortunately my decisions turned out to be justified.

Dunn: And he insisted that you should do training in child development rather than child psychiatry, is that correct?

Rutter: Well, he first of all decided I should become a child psychiatrist even though that was not a career choice that I considered up to that time. But he said I should, and of course through my thesis I had become interested in child development and child psychiatry from the research perspective. I hadn't found the clinical work terribly exciting. So I said, "Well I will give it a go," and you're right, I was forbidden to have any training in child psychiatry, and I never have because he thought it would not only be irrelevant, but it would be damaging in the sense of teaching me all sorts of things that weren't so--

Dunn: Wrongheaded?

Rutter: --wrongheaded, and more crucially would discourage my curiosity. Curiosity was the top with Aubrey as it was with me, but I should study child development, and I certainly agreed with that. And we went through a period of discussions about who I should work with, which never really got anywhere. I became rather despairing who to go to for that training until Herb Birch gave a lecture here. He was a charismatic thinker and speaker, and I was attracted by his questioning approach to child development.

Dunn: That decided you on going to the States for a year?

Rutter: Yes. Both Aubrey Lewis and I saw that much of the best work was going on there. However, Aubrey had also put me in touch with various developmental researchers in the UK--Tanner in terms of physical development, Brian Kirman in relation to mental retardation, Jack Tizard in the social psychiatry research unit, and especially Beata Hermelin. But in terms of child development as a whole he felt that the work that was important was in the States, and I think he was right.

Dunn: Yes. So that one year, which I think was 1961-1962 that you spent in New York, had quite a major influence on your career after that.

Rutter: Oh, it certainly did in a whole variety of different ways. Herb Birch, Alex Thomas, and Stella Chess were all wonderfully good to me, very supportive and three very different personalities, so I got very different things from them. I had the 7:00 meetings in the morning with Herb. He was very combative; he had this rare mixture of being combative and very egotistical, but equally very responsive to the other person. I was warned by those working with Herb that he'd never been known to admit he's wrong, but you knew that you had won when he put your arguments as his the next day!

Dunn: That also started your interest, or maybe it had already started, in longitudinal studies, is that right?

Rutter: Yes, it did play a major part in doing that through the New York longitudinal study, but also through the people they put me in touch with. The three of them between them seemed to know anybody who was anybody in the United States, and so that's when I met Lee Robins, Jerry Kagan, Ben Pasamanick, and Hilda Knobloch, all of them concerned with longitudinal studies in different sorts of ways. There was also the epidemiologist Ernie Gruenberg. I spent quite a bit of time meeting these people and, indeed, with almost all of them kept in touch thereafter. In terms of longitudinal studies and in addition to the New York group, Lee Robins had the most impact; I was very excited about what she was doing.

Dunn: And then you came back to England to the Institute of Psychiatry and were part of the MRC Social Psychiatry Unit, so that was with Jack Tizard and the Isle of Wight studies. Have I got the timing right?

Rutter: The Isle of Wight studies came a bit later.

Dunn: Right.

Rutter: Yes. It was a wonderful group of individuals in the University. Aubrey Lewis had a very broad concept of social psychiatry so that people like me, Beate Hermelin, and Neil O'Connor were involved with cognitive studies of handicapped children. Jack Tizard was an epidemiologist, George Brown studied social institutions at that time with John Wing, but there was also Peter Venables studying the physiology of schizophrenia; it was a broad range of science.

Dunn: So that's a tradition that you've developed all your professional life with the interdisciplinary developmental psychology, isn't it?

Rutter: It is. I think it's the right approach, and it certainly appealed to me. And the questioning style, which I mentioned already with Aubrey, played a major role so that, I mean, virtually all of those people were appointed by Aubrey pre-PhD level, and yet virtually all of them became world leaders. The "failures" were those who simply became University Vice-Chancellors or Presidents of the Royal College. Jack Tizard on one occasion asked Aubrey how he did it; Aubrey thought for a moment and said, "Well, of course the people have got to be bright, but I take the awkward ones the others won't have." Jack wasn't terribly pleased with that answer, but what Aubrey went on to explain was that he wanted people who would challenge the theories of the day and have the know-how on how to do that successfully. That would have been characteristic of all of the people in the University. They were all iconoclasts, but not wasting time on sterile battles.

Dunn: Right.

Rutter: They did research to tackle head-on really important questions. I think it was a wonderful place to develop a research career. The Isle of Wight study arose out of an approach to Jack Tizard by the Department of Education (as it then was), in which they posed the straightforward question as to whether the association between physical disability and educational functioning that Cyril Burt had found in his epidemiological studies at the turn of the last century still applied. Jack, at a fairly early point in the discussions, brought me in. Basically, what we said was, "Yes, we can answer that question, but actually there is an opportunity here to do something much more interesting. An appropriately planned epidemiological study could both be informative on planning services and informative about risk factors for educational and mental disorders with the possibility of providing leads on causal mechanisms." In those days (you couldn't do this today) there was a dialogue with the Government Department and, to cut a long story short, we obtained the funding to undertake this broader, more ambitious study. It wasn't originally going to be on the Isle of Wight. The pilot study was to be there, but we planned to do the main study in the much larger region of Wessex. But as soon as Jack and I were on the Isle of Wight we realized that with a population of about 100,000, it was the right size for what he had in mind; and it had water all around so that the boundaries were clearly defined and it should be reasonably straightforward to determine who was in and who was out. We had a wonderful group of administrators there who were very positive, and so we thought, "Let's forget Wessex; this is the place where we should be." Jack was extraordinarily generous in giving me the opportunity of taking leadership on that. I learned a lot from him; he was a mentor as well as becoming a close friend. It was a very exciting study for many reasons. The scientific findings were striking; they were relevant for the planning of services, and the survey involved an integration with policy and practice from the outset. In addition, the research was undertaken with a splendid group of colleagues, most of whom had worked with either Jack or myself. We worked together as a cohesive, happy team meeting every evening from Sunday night to Thursday night, following which we went through all the research protocols completed during the day. My ordinary work had to be completed during the Fridays and Saturdays when I returned to London. It was very hard work, but great fun and very rewarding.

Dunn: And it's particularly exciting that you've been able to follow them up now some 30 years later?

Rutter: Yes, they're in their mid-40s now. The follow-up was led by my colleague Barbara Maughan.

Dunn: Forties, yes. So thinking about the training issues, do you think that not having been formally trained as a child psychiatrist has been in any way a handicap for you?

Rutter: Not really. Obviously I had a lot of learning to do both on clinical child psychiatry and on clinical science, but there were a lot of people from whom I could learn that both within the department and outside it. And it was set up in a way in which it was very easy to do that. Running my team, but being paid as a trainee, which is the way things worked in those days, worked very well. It would not have worked if I had been in an isolated setting, but of course that did not apply to the Maudsley Hospital and the Institute of Psychiatry where there were a lot of experienced people who were very helpful. It was a tradition that if you didn't know something, you went to somebody who did. So there was no expectation you had to have all the answers.

Dunn: Right. Was what was happening politically and socially in the 1950's and 1960's important in shaping the sorts of questions you were asking and the sorts of mechanisms that you were exploring?

Rutter: Not really, other than in a background way. Let me go back a step. On the social side, of course, the general election in 1945 was what I came back to from the States, and that was a very exciting time. There was the determination of the Labor party that they would not repeat the errors of the first World War, and the conviction that this really would be an opportunity to try to put things right; this accompanied by the very influential Beveridge Report made this a time for a peaceful revolution. It led to the setting up of the National Health Service, a major change in education and the setting up of the welfare state. All of that colored my thinking. I was not very political in a direct sense; I never have been. I'm too questioning for that. But my father, as a doctor, was a supporter of the Health Service, and I grew up on that. One of the experiences as a medical student that fed my views on services was going round with the health visitor (as part of my medical student training) in the very deprived area of Balsall Heath. My father had a general practice in a predominantly working class area in the Midlands, and as a boy I went round with him. Accordingly, I was well familiar with poor living conditions. Nevertheless, I'd never seen anything like Balsall Heath where we went to homes where there was excrement in corners, there was no furniture, there was a pot on the stove that had obviously been there for weeks; it was awful. And so I became very interested through this clinical experience, as well as through Tom McKeown's teaching, in the realm of social factors, not only in the origins of disease, but in the implications of practice. If you're sending somebody home from the hospital to conditions like that, you absolutely had to take on board what was involved. So there was that in the background. There was John Bowlby, of course, and I've mentioned the sensory deprivation studies. These led to my becoming interested in Bowlby's writings. And it was a time, of course, when there was a revolution that Bowlby brought about in terms of conditions in hospitals and in children's homes and of the residential settings. That was not political with a capital P, but it was certainly an important issue that played a major role in my thinking and indirectly led to my book on maternal deprivation.

Dunn: That had an enormous impact, I think, on not just academics, but broad social policies. It had a huge impact when it was published in 1972--terrifically important book.

Rutter: That brought in another key mentor, Robert Hinde, who I had never worked with. I met Robert at a meeting a year or two before completing the book. I hadn't really got to know him, but I certainly recognized that he was somebody who could be very helpful on this. So I sent him the draft manuscript. And as you know from Robert, I got a typical Robert epistle back again with about 17 pages of comments. Once I got used to all the criticisms, I realized it actually was enormously flattering that he'd taken the trouble to go through it line by line. He was extremely constructive in his suggestions, put me in touch with areas that I had not read adequately about, and provided a new perspective on things. And since then I've interacted with him on various things. I don't know that he recognizes it, but certainly he had a major impact on my thinking.

Dunn: As you describe these various encounters with people and getting to know them and their ideas, it all sounds fairly straightforward that there's a lot of continuities in your interests and in your intellectual approach to understanding links between, say, experiences and outcome. Do you think there've been major shifts in your approach, or is it more the continuities that stand in your sort of intellectual approach?

Rutter: Oh, both I think. Certainly there are continuities. I mean, in a sense I'm still working in the same areas that I started on for my doctorate, although I'm doing it in a different sort of way. The issue of the importance of individual differences, of course, came up with temperament in the New York experience. I did actually my first twin study there. It was a small pilot study, but it was actually a very interesting one, and one of the pairs that I was interviewing was a pair of monozygotic twin girls who'd been separated at birth and brought up in different homes and who didn't get to know about each other's existence until adolescence. I remember my interviews with them very well because they were extraordinarily similar in their temperamental patterns as measured in the ways that we were using at that time. And yet the effects were quite different. Both of the girls had very irregular patterns, were quite non-adaptable, and rather difficult in some ways. Nevertheless, one of them fitted very smoothly into a well-functioning family and was behaving and developing in a manner that was not at all noteworthy. The other one was quite different; it is well exemplified by the second interview that I had with her when she was about three years of age. She answered the door, and her opening words to me were, "Guess what? I had them all up again last night." So here we have remarkably similar temperamental patterns, but the effect had been quite different. So that brought me into genetics in a kind of way. The twin study wasn't a serious genetic investigation, but it certainly fired my interest in genetics. It provided a continuity with what I'm doing now, but then it went into the background, if you like, until the opportunity for the twin study of autism came up. That certainly excited my interest in a major way and made me aware that I had to learn much more about genetics if I was going to be able to do anything of note. That constituted a major shift in my research. A further relevant shift, with regard to my involvement in genetics, came with my first involvement with the Virginia twin study. I've forgotten exactly when that was, but it must have been in the late '80s. Initially I came in because they wanted a consultation on various measurement issues, but then I became embroiled in it in a much more major way. As a result, I began to think about genetics in a much more systematic way, but also I developed a new way of thinking.

Dunn: Right. And has that led you to think that you were going down wrong tracks before? I mean, did it lead to any sort of revision of your ideas, for example, in autism?

Rutter: Yes. I published a paper back in '66 I think it was in which I said that it seemed unlikely that genetic factors would play a major role in autism. It was no sooner than I'd written that paper and had it published that I realized I was wrong. Accordingly, a year later I wrote another paper in which I pointed out why it was wrong in that I had paid attention to the absolute level of autism in siblings rather than the relative level compared with the general population. Curiously, the proper geneticists made the same error and were slower to correct than I was, so I felt quite pleased that I, as it were, had seen the error of my ways. I was wrong indirectly, and I quickly put it right. I've been wrong on several other things, and indeed, I've written on the importance of sometimes being wrong. The way I initially put it in a tongue in cheek way was that if you simply confirm what you already thought you knew, you've learned nothing. It's when you're forced to recognize you were wrong and that you've then got to think anew that the exciting part of research begins. Another example of the same kind would be that in the deprivation book, but also in later papers about the negligible effects of experiences in early life that were independent of the later environment. That is still empirically correct, although there are some important exceptions. But it took me a year or two to recognize that this was the wrong way of thinking about it because it assumes that the later environment is independent of the earlier environment; obviously it is not. Accordingly,

the effects of early experiences that are independent of the later environment are one thing, but they tend to have a cumulative effect because they actually shape the later environment. That realization changed my way of thinking about development, not through any one finding, but simply through a growing awareness of how development operated.

Dunn: Yes. I'm supposed to ask you which of your written contributions reflect what you think are the most important contributions you've made. I don't know if there's any particular paper or book that you feel captures those. I think your book on *Developing Minds* is an amazing summary of some of these issues about developmental continuities, processes, and mechanisms.

Rutter: Oh, thank you. Yes, well I--that's one of the things I've written with which I was most pleased. And of course, it was a joint thing with my wife who brought a different perspective into it. So that was interesting at a personal level, and it worked very well.

Dunn: Let's look back on some of your other work. You have written so much that in posing this question I thought, "How can Mike come up with an answer to that one?" There's been so much. Still it's interesting that, personally, you feel that book brought together a lot of your ideas. Also, your book on maternal deprivation has to be one of the best pieces of your writing.

Rutter: I agree. On a different aspect of my research I would also pick out my paper looking at the two-way interplay between research and clinical practice in autism.

Dunn: And then there's the schooling work, which we haven't discussed so far. That provides another whole dimension, doesn't it?

Rutter: Yes, it does. That's another study that I enjoyed enormously for a whole variety of reasons. One reason was that supposedly it was known at the time that you couldn't study schooling. I've always enjoyed having things that people said either shouldn't be done or couldn't be done and showing not only that they could, but that they should. Jack and I had originally wanted to do the study of schooling on the Isle of Wight. Although we were primarily studying families rather than schools, we found that the rates of both reading difficulties and emotional/behavior disturbances varied enormously by school. And so the question had to be was that because the schools had differential intakes, or was it the effect of the schools on pupil progress? At the time, the Isle of Wight authorities, who'd been enormously helpful in everything else, couldn't see the point of this, and so we didn't do it there. But the same issue came up in our findings in London, and the turning point there was a meeting that we had with the teachers who'd helped us with the survey in which I was mentioning, amongst other things, the differences among schools. In the discussion session after my talk, there was this one lady who gave me a very hard time and with what seemed to be hostile questions.

Dunn: A teacher?

Rutter: Yes, a head teacher. At the end of the meeting she came up to me and said, "You absolutely cannot leave that finding there. It's important to us as teachers to know whether this is a function of differential intakes or the effects of the schools on their children. What about you and I setting up a working group of teachers and researchers together and planning such a study?" So once I got over the shock at finding what I thought was a hostile questioner actually wasn't, we did exactly that and it led on to the study. Initially, people said it couldn't be done. Firstly, you wouldn't be able to study the things in the way you wanted and second, the schools would never agree. She assured us that the schools would agree. She took it through the Union, and she took it through the negotiating channels. We actually had a teachers' strike in the middle of the study, and she persuaded her colleagues that they should refuse to do their ordinary duties at school, but they should complete what was needed for the

research. So she was just wonderful, as were the other teachers. The study itself was exciting because it came out with some very interesting findings that created quite a stir at the time. The academics were very critical, so one distinguished academic, who will remain nameless, actually wrote a paper that he distributed to every department of psychology in the country pointing out that this result was flawed and could not be true.

Dunn: On the basis of the statistical approach?

Rutter: He argued that we hadn't taken account of other things unspecified, and he added various statistical objections. In the event, subsequent studies have confirmed our findings and shown that, if anything, we rather underestimated the school effect rather than the reverse. The response of schools, interestingly, was very positive. I was worried that, for example, our finding that past class size was unrelated to outcome would be seen as damaging to schools. I had to give a talk to a group of about 2,000 people and teachers in the States at their annual Union meeting. To their eternal credit, they didn't respond like that. What they did was to take up very positively the point I had made that this is a liberating thing, and that what one needed to be concerned about is not the average class size, but where a reduction of class size really would make a difference. That provided a freedom to do things that has not been present hitherto. Ultimately the academics realized that was actually an important point, but it took rather longer. We were also criticized by left wing sociologists on the grounds that because we said that schools in inner city deprived areas could do something to improve things, we were actually interfering with the revolution that was needed to bring about more radical change. But in the end that faded away.

Dunn: Another question that SRCD would like your views on is your experiences with research funding apparatuses over the years, and whether you've had a role in shaping policies about funding research as well as getting support for your own work and implementing scientific ideas in practice. Once the Child Psychiatry Unit was formed here, then you obviously were an enormously influential figure in how child development research proceeded, I would have thought.

Rutter: Yes. I need to give a slightly complicated answer to this. First, in relation to the funding for my own research I'm aware that most of the things that I've done that have worked really well would not get funded today. So that, for example, in relation to the school study, I was not an educationalist; I had no track record of research in schools other than the work we'd done on units for autistic children, and there was no theory of a grand kind. There were specific hypotheses, there was a strategy that I was pretty sure would work, and I had a good track record for developing measures where they were needed. But if the proposal had gone out for review in the usual research council way, the work would not have been funded (as the hostile response from academia when we published our findings showed). Also, in the 1960's Government Departments would engage in dialogue with researchers. Thus, the Isle of Wight Study grew out of our discussions between policy makers and Jack Tizard and myself as researchers.

Dunn: And were more flexible--

Rutter: More flexible, yes. And the same would apply to many of the other things I've done. So the longitudinal follow-up study that Dick Harrington did on depression was initially slammed by reviewers. We applied to a private foundation, a charity, and on the one and only occasion in my life it seemed to me the referees just totally misunderstood what it was about. So I wrote a detailed critique with a letter saying that I realize it would make no difference, and I know the rules of the game and will apply for funding somewhere else, but actually the referees' arguments were not only wrong, but they would lead to the opposite prediction to the one they put forward. To my surprise, they sent this letter to the reviewers who agreed with me and said that they had been mistaken, and I got funds; that couldn't happen today. So

I worry about the conservatism of funding. I have been lucky in the time that I did my work that there were ways round that, and I got funded to do things that would not get supported today.

Dunn: It's an important issue, isn't it? Because most of your research required large scale, long-term funding, and it's not a question of your being able to go off and do it anyway.

Rutter: My first grant was a five-year grant. I don't know what it cost, but we had four full-time people on it, so it was fairly expensive. I would not get funded today to do that.

Dunn: So even though you are now, or have been in a position in like, in your role in the Wellcome Trust of enormous importance in funding, you haven't been able to shift this conservatism--

Rutter: I've tried with some limited success. I'll have to give one other experience because it was, even now looking back on it many years later, a very painful one. When I came back from the States I was encouraged to apply for a Unit, and there had been a report from the Department of Health that a Unit in child psychiatry was needed. So I did put in an application, and the reviews were very positive, but raised a few very specific queries, all of which were very easily answerable. And so it went back and got turned down flat. The answer came back that it wasn't even worth considering as a project grant; that was very surprising. The head of MRC asked to see me and said that he was worried that this really did seem a pretty bizarre outcome. They'd not used referees on the reapplication because it seemed so obvious that there were only a few specific points and because the referees had supported the application in the first instance. He commented that, of course, he could not change the panel decision, but he advised me to put in for a program grant. We talked a bit about what that might comprise. Subsequently I discovered that when the program grant went out for refereeing, one of the questions put to referees was should this be a program, or should it be a project, or should it be a Unit. The reviewers said it should be a Unit and so having applied for a program grant, I got awarded a Unit. It should have been a totally positive experience, and of course I was delighted to be granted a Unit. Nevertheless, it was a very bruising experience because it was obvious there had to have been somebody who put the knife in. Many years later, after the key individual died, I learned that this was in fact the case. Even now it is very painful recalling that someone resented me so much that he would do that. In the event, when I learned who it was (which really I shouldn't have found out), it helped because I had seen him do exactly the same thing in the same unfair way to other academics.

Dunn: Let me return to your influence on funding, more generally, quite apart from your own research. You have had key positions in the running of more of the organizations funding developmental research in the UK, have you not?

Rutter: Yes, that is true. In the 1970's I was on the Educational Research Board of the Social Sciences Research Council (the only medic on the SSRC at that time); I chaired the Neurosciences Board and the Medical Research Council in the early 1990s; I joined the Nuffield Foundation as a Trustee in 1992; I became a Trustee of the Jacobs Foundation in the late 1990s; and I became a Trustee of the Wellcome Trust in '96; during the last five years I've been deputy chairman. One of the issues in all of these places has been to try to ensure that there wasn't too conservative an approach. If you got really good ideas from somebody whose track records suggested that they knew how to do it and could do it, you should take risks. If you're not funding risky research you are, to my mind, failing. And the definition of risky means, as it were, that some of them must fail; if they all succeed then it's not risky. I think that, at the top of all these organizations, that message has been taken on board. But panels, when they're funding only 20% of the applications that come in, are conservative, I have to say. What I've tried to do at the Wellcome Trust, through other people because the decision-making on this is not mine, is to have mechanisms where you can deal with studies of that kind and, of course,

the Wellcome Trust has funded some very risky things. For example, the sequencing of the human genome, if you like, would be one of those. There was a recognition that John Sulston's ideas on this were right. He had the know-how to do it, and we realized that it would actually make a difference to what was possible in genetic research. Now, of course, everybody sees that as the obvious thing to have done; it didn't seem obvious at the time. At less global levels there are many other examples of risky research. One of the dilemmas is that all funding bodies have to take strategic decisions on what they're going to fund, and they need to set up mechanisms on how to do so. So for example, at the Wellcome Trust, in relation to functional genomics, we recognized that this needed to involve collaborations that would often have to be international in order to do what was needed. Accordingly, we had to have a mechanism for being able to fund research in the United States and in mainland Europe, which hitherto had been outside our scope and so we did that. That kind of strategic thinking is very important, but what one must also do is to do it in a way in which, having set up the initiative, you are then responsive to the creative ideas of individual scientists. I'm very much against committees deciding it should be done this way or that way. I think the history of science indicates that's very rarely helpful. So there has been only some minimal success in that connection. I think that in terms of interdisciplinary approaches I have won through rather more successfully. Of course, the Wellcome Trust pioneered four-year research programs, PhD programs and those, I think, have been a great success. They've been taken on elsewhere and that is good. They've been successful for many reasons, but amongst others they recognize the value of people in the first year of their PhD having a range of experiences that bring them outside their narrow training up to that point. And, of course, in the SGDP Center here that's been very much what's been done.

Dunn: A comment on the center would be appropriate here, I think, because in terms of institutional formation this has been great. This was absolutely your idea in the first instance, and it has grown incredibly successfully, I think.

Rutter: Yes, it has; I'm very pleased with that. To come back to something we were talking about much earlier, this sort of interdisciplinary integration has been seen throughout as a means to an end and not an objective in its own right.

Dunn: Right.

Rutter: As the Center has appreciated, it's not that interdisciplinary collaboration is good in its own right, but rather that you need a range of research approaches in order to solve problems. This brings together the social, the developmental, and the genetic, and it seemed to me obvious that the crisis in social psychiatry at the time all this was set up demanded that. The history of social psychiatry in the UK has been very strong. Wonderful things had been done, but it hit a plateau. There was a failure to take on board all sorts of things, but especially the failure to take on board the role of genetics, and it seemed to me the time was right to do that. In order to do that it was necessary to recruit people from outside the UK. At the Institute of Psychiatry we had some very good people to serve as the nucleus for the Center. But in terms of all three areas, genetics, social, and developmental, we had to have new people, and so one of the challenging aspects of this was the international recruitment. It began, of course, with your recruitment and that of Robert Plomin. That was actually just before the Center was funded, so strictly speaking it wasn't part of it, but it was, to my mind, closely integrated with it, and of course, the two of you played a major role in the development of the Center. But then other recruitments followed: Terrie Moffitt, Avshalom Caspi, Francesca Happe, Ian Craig, and others. That was a very exciting period. It was interesting again because initially the MRC group that reviewed this clearly took the view that we couldn't recruit at that level. They assumed that we were talking about senior lecturers (associate professor) positions at best. It seemed to me that we wanted the world's best (which we did), and we should try for the world's best and only accept something less than that if we were forced to. Well, in the end, we actually got the world's best in a number of areas. It's

interesting that that approach to recruitment has become part of MRC policy as it was also part of Wellcome Trust policy. It creates problems, of course, and it means that everybody's not equal. There were salary differentials, there were resource differentials, and of course it puts pressure on the funding of research. That is one of the downsides. If you have top-level recruitment the quality of research goes up, and therefore it's harder for second level researchers to get funds. And that has been a reality. I'm unapologetic about it because I think raising the quality of research is what it's all about, and the new ways of bringing these three areas of science together could not have happened without it.

Dunn: Another area that I think SRCD is interested in is the relation of research experiences to applied issues in child development. And obviously with your domains of interest, such as autism and the other development psychological areas, that's obviously a very relevant dimension. Do you have any comments on your experiences with bridging the gap?

Rutter: I've always been interested in doing that. I suppose that people wouldn't see me as an applied researcher in a straightforward sort of way, but really of all the things I've done, whether on maternal deprivation, or schools, or autism, or genetics for that matter, have been concerned with practical applications, and I have wanted to work in ways that provided bridges with practitioners. The school is the most obvious example where that research was absolutely at the top of the agenda, but it would apply to all the others. In the autism field it meant that I recognized really from the very beginning that if the research enterprise was going to work, one had to have a really good clinical enterprise that went with it. And so I put a lot of energies into building up a good clinical service; it paid off. It was good that one had a good clinical service, but it also meant that the families that felt helped were very willing to help with our research, and so that became a corporate enterprise.

Dunn: That was true for the Romanian adoptee research too, wasn't it?

Rutter: There's a two-way traffic by which research shapes what you do in clinical work, but clinical work also shapes your approach to research. And that two-way traffic has been very important throughout my career. One of the concerns I have about the Center would be that, for very good reasons, it wasn't directly involved with clinical services. One of the challenges to ensure that the Center's integration within the Institute of Psychiatry as a whole ensures that those still develop and that there are really good people who are developing clinical services in research oriented ways. Most of the people in the Center don't function clinically in quite that way, and it will be important to have interactions with people outside the Center who do. Yes, indeed. The follow-up of children from professionals depriving institutions who were adopted into UK families was initially funded by the Department of Health to answer policy and practice questions. However, it was clear from the outset that it also provided the opportunity to tackle fundamental developmental questions of theoretical, as well as practical, importance. There have been gains because the researchers have been freed up to develop their sort of research that they were hired to do, and that's been terrific. I wouldn't want to change that.

But we must not lose sight of the needs to build links between the research and its clinical application. One of the problems in the UK is that the way in which the health service has developed means that it's much more difficult, if not impossible, to set up research clinics of the kind that I have done in the past. Of course, in the United States the system is very different, but there are the same sort of problems and tensions.

Dunn: Could we turn to your experiences with SRCD itself? When did you first join?

Rutter: I joined, I think, in '83. I thought of joining much earlier than that because I first became interested in SRCD back in the early 1960's. Alex, Stella, and Herb all suggested that I should join, and various other people I met did the same. I didn't partly because the journal

was available in the library anyway. I was on a very low salary, and therefore even the relatively modest sum required seemed high. Moreover, the cost differential of going to meetings according to whether you were a member was trivial; in fact, I don't even know there was a differential at that time.

Dunn: Do you remember the first meeting you went to?

Rutter: No, I don't. I started going to meetings of SRCD long before I was a member. It quickly became my favorite organization partly because I knew a lot of people there and partly because the quality of the meetings was high; the research quality was good, and the way that it was organized was good. My contacts with psychologists in the States have always been stronger than with psychiatry, although I have both and profit from both. Even now, although SRCD has become a vast organization, it still, in a curious way, has more of a family feel to it.

Dunn: It does, yes, I agree.

Rutter: I hope we don't ever lose that. So I was a bit slow to join and so the first meetings I attended would have been in the '60s.

Dunn: Yes, right. And have you been involved in organizing scientific meetings or participating in the scientific meetings since the '80s in that case?

Rutter: Oh, yes.

Dunn: Yes. And the publications?

Rutter: Yes, I've done reviewing. There are ways in which the changes over time in SRCD have got a curious mixture of what I would see as the very positive and somewhat less positive. On the positive side, the first meetings I went to had much the same impact on me as the journal. That's to say it involved tremendously good research, but actually it didn't seem to matter very much. So it was largely cross-sectional. It was dealing with privileged populations of one sort or another; it wasn't concerned with psychopathology for the most part, wasn't concerned with applied issues, and it really just wasn't very interesting in terms of the areas that I think are exciting. That has changed very considerably. There are now a lot of all longitudinal studies. The interest in psychopathology has certainly grown enormously and so has involvement with applied issues. What is less positive is that it has curiously, for reasons I don't quite understand, become less interdisciplinary so that the statistics on the proportion of the membership who are not psychologists has gone down.

Dunn: That is interesting.

Rutter: Now, I'm not sure that it means quite what it seems to me because the influence within SRCD of non-psychologists is actually very strong and, I think, appropriately so. The aspect that hasn't improved is the involvement of child psychiatrists. The number of child psychiatrists involved is abysmally low, and that bothers me. The other changes are ones that are happening, although I would like to see them happen a little bit more, so that the biology of development was a bit slow to come on board and genetics initially got a pretty hostile reception.

Dunn: It certainly did, yes.

Rutter: I well remember Sandra Scarr's presidential address. The distinguished psychologist sitting next to me, who shall remain nameless, was incandescent. I thought she was going to have a stroke on the spot; fortunately, she didn't. That has changed. There's not a lot of genetics in the journals and there's not a lot at the meetings, but there is more. Imaging

doesn't have much of an impact so that the revolution in the way cognitive psychologists are making use of imaging to understand mental mechanisms has had occasional representation, but not much. And the exciting developments in animal research, again, have a very small impact. It would be people like Robert Hinde and Steve Suomi, who certainly played a very active role and continue to do so, but much of the experimental work because both Robert and Steve use experimental thinking in their work even though they're using semi-naturalistic settings to study things. The more laboratory style work by people like Bill Greenough and Michael Meaney doesn't have much of an impact. Equally, one of the main issues that concerns me is what psychosocial experiences do to the organism has almost no place in either the journals or the meetings.

Dunn: That is a gap, isn't it?

Rutter: It is, indeed.

Dunn: During your time as President were there particular problems or issues that surfaced that you became involved with?

Rutter: There were issues of various kinds; most of them concerned personalities and matters of detail. It struck me as, for the most part, a very harmonious organization, so it was a nice group to be a part of. The fact that there is a generally well-functioning central office made a big difference. And so it was a very interesting and challenging area in some respects, but not challenging in a negative sort of way. I tried, unsuccessfully, to get them to recognize that it is an international organization by having at least one meeting outside North America, but for a variety of practical reasons that didn't happen.

Dunn: Yes, still waiting for that to happen.

Rutter: We're still waiting for that to happen. There are also some practical reasons why it's difficult, but the fact is that developmentalists in Europe would see SRCD as their prime organization. I know there is a "home rule" for Europe groups as well, of which I'm rather out of sympathy I have to say, but SRCD is international.

Dunn: Absolutely.

Rutter: And I think it would help if that was recognized by having a meeting outside North America.

Dunn: So the last area is really something that I think we've talked quite a lot about already, which is the history of the field while you've been participating in it and whether you think there have been major discontinuities or new directions that have been particularly important, and whether your views concerning the importance of these different issues has changed?

Rutter: Well, I'm reasonably optimistic about the future. I think that there are new ways of thinking, and it's been fun to be part of some of that so that I think the whole issue of gene-environment interplay is forcing changes in the ways of thinking about both genetics mechanisms and environmental risk mediation mechanisms. I see that as a major growth area, and I think it will change the way one thinks about child development; I think it will change practice. Exactly how? Well, that depends on how the results come out, but I think the old idea that there was very little in the way of gene-environment interaction has gone. There are various reasons why people wrongly drew that conclusion, but it's clear from not only the quantitative genetics, but more especially the molecular genetics and not just in psychopathology and in psychology, but in medicine as a whole that all that's been changed. The old debates about how much is genetic and how much is environmental are dead and

buried. They haven't quite died away, but it's not the way to ask the question. The answers are interestingly troubling, as it were, in ways that I find exciting. But they're troubling to some geneticists because they are being forced to recognize that much of genetics operates through the environment, and that's not the way they were brought up to think about it. But I think they're going to have to think about it like that. It's uncomfortable for psychosocial researchers because genetic effects are all pervasive, and it's very difficult to find environmental effects that aren't influenced by the genes in one way or another, but it seems to me all of that makes sense. I think that sorting out the details is going to be really exciting, and I hope I'm around long enough to play some role in that. A second area though, which interests me equally, is the effect of experiences on brain development and the sort of issues that this question raises in terms of biological programming. There's some really good experimental work going on in this field now and mainly in North America rather than the UK, but some here. The issues from the Romania study have been really challenging in showing that the effect of early experiences are as marked at 11 as they were at 6.

Dunn: It's a very striking finding.

Rutter: Yes, and I would not have predicted that, but neither would anybody else I think. And that really does mean you've got to think and ask what is happening for that to be so. But you've also got to take on board the huge individual differences in outcome. The old style programming notions talked about things as if they were inevitable and universal. Our finding is that they appear to be neither inevitable nor universal challenges all of that. Now I don't know quite what the mechanisms are in all of that, but I see that as a major growth area and the answer in terms of the Romanian study, I think, will be in terms of effects on the brain in one way or another. But if one's looking at other situations, the effects are as likely to lie in cognitive mechanisms--the way one thinks about experiences, the kind of models you build of what has happened to you, and the development of styles of interpersonal interaction that serve to create environments that subsequently have effects on you. In other words, there are many different ways in which experiences may bring about longer term consequences, and I see that as a major growth area. I see the understanding of the relationships between brain and mind as clinically important.

Dunn: Where do you begin?

Rutter: Brain imaging is currently the most valuable tool. It does not provide all the answers, but it provides a splendid beginning. I'm cognizant of the fact that at the moment all it tells you is where mind activities are taking place. It's not really correct to say it's seeing the brain in action. It sort of is, but only indirectly. Spectroscopy does a bit more than that, but it's still a fair way off. But looking at, for example, the brain processes that are concerned in compensatory mechanisms, whether you're determining mechanisms in relation to brain injury (why young children who develop a lesion that knocks out the whole of one hemisphere, don't land up without language) or whether one's looking at compensation in terms of autistic individuals who lack mentalizing skills, but yet find a way, as it were, to solve mentalizing problems, compensating mechanisms need attention. Similar issues arise in relation to dyslexia, so I see that as an interesting area. Now, at the moment, that's not what child psychiatrists are mainly looking at, and it's not what developmental researchers are mainly looking at. So I hope they're moving in those directions because I think that, although they involve really difficult research questions, they are tractable and of huge practical importance.

Dunn: Yes, and very exciting.

Rutter: Yes.

Dunn: So absolutely the last question SRCRD asks us about is whether you have any comments on your own family experiences. I'm thinking of you as a grandfather of twins.

Do they have any bearing on the scientific issues you've been most interested in, or the way you studied them, or your own contributions?

Rutter: Not in a very direct way, but in indirect ways. We talked earlier about my experiences as a boy growing up and the effect that had. That meant that until my biological father died a few weeks ago, I had four parents in their 90's--two social parents in the States and two biological parents here. My family experiences growing up, as I mentioned, were happy ones. My father was not a researcher, but he certainly had an amazing intellect, and so I grew up having discussions about things that played a major role in my thinking. Both my father, but more especially my grandfather, did things in ways that were unconventional. So my grandfather, for example, decided that although he was a pacifist (he was a Quaker), it was necessary during the war to work with ambulance services within the armed forces as well as with Friends Ambulance Unit. He got the MBE for doing that work, but within Quakers people weren't sure that was right. But he was a strong-minded person, and so I grew up with an ethos, as it were, of being expected to think things out for yourself and to stand up for what you thought was important. My father was a very unfrontational person, amazingly so really. During the whole of his lifetime I never saw him lose his temper, and I never recall him saying a hostile remark about anybody.

Dunn: Amazing.

Rutter: There's nobody else I can say that of, but he was obstinate. At school one of the regulations at that time was that you had to engage in all the sort of races in the annual sports. He was forced to run the mile and thought that was wrong in principle. And he wasn't going to do it, but he was told he had no choice. So he did it his way; he walked and ran not a step. This kind of stubbornness was only a trivial issue, but it applied more broadly as something I grew up with, and I guess my being a non-joiner politically--although I've always been interested in political issues, I've never joined a political party--is part of that. In my research, similarly, I had not joined the club for psychoanalysis, attachment, biology, or for that matter genetics really. I'm a sort of an insider/outsider in relation to all of those, and my family, I'm sure, played a part in that.

Dunn: What about having your own children?

Rutter: Oh, well, that certainly was influential in that I enjoyed my children, as indeed I enjoy them now as adults; and I enjoy my grandchildren now as young children. And thinking about them as individuals certainly influenced me in thinking about people. I never applied my psychology in bringing up children. They were never research subjects and I don't think they ever thought of me as an expert in child development.

Dunn: Isn't it interesting; the same applied to me. It's like two different worlds.

Rutter: It used to irritate my children, two of whom were more or less in the same sort of field, that they were expected to be experts on my research, which of course they weren't. They knew a little bit about it, but that wasn't what we talked about in the family at home.

Dunn: No.

Rutter: Having two grandchildren as twins certainly did not bring me into genetics because I came into genetics well before that, but their interaction and the way they thought about things did cause me to think a lot. And it was very striking that they saw people as coming in pairs. It was bothersome to them if I came without Marjorie or Marjorie came on her own; everybody was supposed to be with somebody else in a pair. And they were also interesting in that sex typing came very early on. Now their parents are very untraditional people, but for example I remember when James must have been about four, I suppose. I was saying something

about tennis and he said, "Girls don't play tennis. They play badminton." I realized that children make classifications from the word go, regardless of anyone's teaching.

Dunn: Absolutely.

Rutter: Our two year old granddaughter, who Marjorie looks after for a day and a half a week, is classifying all the time. She has her theories, as it were, and she discusses them with you. It's immensely interesting. And it was the same with Rachel, who's now five. When she was about four we had this discussion in the car as to what you had to have to be a family, and she was saying that she thought you didn't have to have more than two, but you couldn't be a family with only one person. You could have a family of husband and wife or whatever, but you couldn't have a family of one; a family had to involve being closely involved with at least one other person. I thought, well, that's spot on. So yes, my family has played a very important part in my life and still does. The transition to grandparenthood, I think, was absolutely wonderful. Curiously, I think that I learned more about child development with my grandchildren than with my children. Maybe it's just that you don't have them every day, and therefore you see the changes more readily. Perhaps, too, their discussions are very thought-provoking in ways that, in the hurly burly of dealing with day-to-day issues, don't arise in quite the same way with your own children.

Dunn: Well, I think we've covered everything they wanted us to, so thank you very much.

Rutter: Thank you.

Those who inspired and were influenced by Michael Rutter:

Mentors

Stella Chess
Herb Birch
Willi Mayer-Gross
Robert Hinde
Aubrey Lewis
Alex Thomas
Jack Tizard

Colleagues

George Brown
Ernie Bruenberg
Avshalom Caspi
Ian Craig
Judy Dunn
Francesca Happe
Beata Hermelin
Jerry Kagan
Brian Kirman
Hilda Knobloch
Terrie Moffitt
Neil O'Connor
Ben Pasamanick
Robert Plomin
Lee Robins
Sandra Scarr
Steve Suomi

Michael Tanner
Jack Tizard
Peter Venables
John Wing