Alex F. Roche

- Born October 17, 1921 in Australia
- Spouse: Eileen M. Roche
- M.B. and B.S. in Medicine (1946), Ph.D. in Anatomy (1954), D.Sc. in Child Growth (1966), and M.D. in Internal Medicine (1968) all from the University of Melbourne

Major Employment

- At Wright State University:
  - Fels Professor of Community Health and Pediatrics, School of Medicine: 1990-present
  - University Professor: 1990-1995
  - Head, Division of Human Biology, Department of Community Health: 1984-1992
  - Fels Professor of Pediatrics and Obstetrics and Gynecology: 1977-1990
- Chief, Section of Growth and Genetics; Chief, Section of Fels Longitudinal Study and Families; Chief, Section for the Measurement of Growth and Maturity; Senior Scientist, Fels Research Institute: 1968-1984
- Director, Melbourne University Child Growth Study: 1954-1968

Major Areas of Work

- Cranial facial growth, anatomy, child growth

SRCD Affiliation

- Social Policy Committee member, Program Evaluation Committee Chair, Governing Council member (1975-81), Interdisciplinary Committee member (1977-81) and Chair (1981-83)

SRCD Oral History Interview

Alex Roche

Interviewed by Donna Miles Curry
December 2, 1992

Curry: Good morning, Dr. Roche.

Roche: Good morning.

Curry: I'd like to thank you for agreeing to do this historical interview for SRCD. We'll start off this morning with a general history of some of your background. Could you share with us some of your family background or your childhood; any experiences of interest?

Roche: Well, it's hard to know what's going to be interesting to other people, and it's hard to know to what extent one should keep one's family life private and to what extent one should make it public, but I was born in Australia a long, long time ago—71 years ago as a matter of fact—in a village that at that time had a population of around about 1,500 people about 40 miles from Melbourne, which is a quite large city. I was an only child, and that's sort of more or less the family background. When—the question here on the list that was suggested is where did I grow up. Well, I remained in that village until I was 13 and then I went down to Melbourne to a boarding school where I was—it was really a secondary school, and I was there until I was 18 when I went to the university. Schooling in the village was a little unusual perhaps by American standards. It was a convent school. The total enrollment was around about 40 to 45 all grades combined. I think the nuns who were teaching probably had a sixth grade education, not more than that. Nevertheless a few children in the school went beyond the sixth

grade and went to the eighth grade, which was as far as the school went, and some indeed later went on and were educated further up to about intermediate, which is about two years beyond the eighth grade, equivalent perhaps to a tenth grade here. So that was a strange sort of schooling experience I guess. It was strict on occasions, the opposite on other occasions. There was no organized sport. It was co-educational from a point of view of boys and girls; you can hardly split up a school that's 45 into two schools of 20. It was about a mile and a half from where I lived, so one walked to school and walked back again. Walking to school was fairly brief. One got there as quickly as possible. Coming home took a great deal longer. It took perhaps an hour and a half to come home because there were lots of other things to do on the way. And so that was what schooling was like.

Curry: Great. Do you have any early work experiences that you'd like to share with us?

Roche: It depends on what you mean by early, Donna. The answer is no. I have only had a couple of jobs and my first job was as a resident, the second job was a series of different positions at the University of Melbourne, and then the third job was here in Yellow Springs around Wright State University. So I really have had no work experience until I became a resident.

With regard to military experience, I do have some and it's not quite the same sort of career as you associate with Douglas MacArthur and other people like that. At the end of the first year of the medical course the war was going on, of course, in Europe. Australia was involved in the way since the latter part of 1939 and there was compulsory military training during the summer. So I went away with a university group to an army camp. On the way the news arrived that the Japanese had bombed Pearl Harbor and things were sort of changing rapidly. So in that military camp it looked as if one might not be out again at the end of the summer, this might be a permanent arrangement, and for many people it was, but the army decided to get rid of me and send me back to the university. So I had about two to three months of army experience, which I—during which I was a batman. A batman is a person who does the washing, and makes the beds, and sweeps the floor, and cleans the boots and whatnot for either one or more officers. I had about three to four officers. It was a very easy job; I shared it with another person called Jeff Watson who later became head of the department of statistics at Princeton, so we were the two batmen in that large unit. It wasn't a very big unit—big enough unit anyway. So that was the end of my military experience.

Curry: You never had to go overseas or off the continent?

Roche: No, no, no. I was left alone.

Curry: Did you have any early adult experiences that were important to your intellectual development?

Roche: No more than anybody else has. You know? You go to classes, you try to pass examinations, you read books, you do things of this sort, no specific experiences different from what anyone else would have had.

Curry: No special inspiring teachers or—

Roche: Oh, from that point of view a lot who were uninspiring and really disastrous teachers, but thinking of it now—at the university level I presume we're talking about now—some unusual teachers. And I think perhaps none that inspired me exactly, but there was one I had respect for. Not many others did because his general style of dressing and talking and behaving was unusual, and perhaps you might say a little crude, but I appreciated his intelligence and his honesty, and remained good friends with him for a long time. He is dead now. He became more and more prominent as the years went on, but I don't think he was always as much appreciated as he might have been. Everybody appreciated his intelligence. Nobody argued about that. And he gave me good advice on occasions.

Curry: That's good. What would you say would be your origins in your interest in child development?

Roche: None.

Curry: None?

Roche: And no interest in child development in the sense that—

Curry: Per se.

Roche: —people usually think about it. My interest grew out of the fact that I—after I completed a residency, I went into the department of anatomy and I went in there because I wanted to be a surgeon and a common way of training to be a surgeon in order to pass the first part of the necessary examinations was to spend a year, or perhaps two years, in the anatomy department demonstrating. But during that period I decided I didn't want to be a surgeon and I would prefer to be an anatomist. The major reason was that the work was so easy and I wouldn't have to do much. So that led me to considering what I would do as an anatomist other than teach and examine and those sorts of things one needed to have some research interests. This was gross anatomy, so there's not many opportunities. One opportunity is history of medicine, but that I didn't want to make a career of, and another seemed to me to be physical aspects of child growth and development and maturation, and that is what I wandered into and have not managed to wander out of. So development for me is physical changes during growth, but I'm also interested in changes that occur in adulthood and old age.

Curry: Did you have any special research mentors or significant colleagues in those—especially those early years?

Roche: No, no, not really. I developed the interest in Melbourne and that was an active interest for around about two years, but it was based on reading and not having anyone much to talk to except a few people in the dental school. And I was interested in cranial facial growth. And then I got a Fulbright and came over here and went to what in those days was Western Reserve University—it's now Case Western University. And there, there had been a very active longitudinal growth study that began in the late '20s and had ended in the late '30s, but some aspects of it were still continuing. The cranial facial part was still continuing. So I find that a very rewarding experience to see what those people had done, but they were not doing very much by the time I got there so that seeing the records and how the study had been organized and so on was very helpful. So that you might perhaps think of those people as mentors, but the major one was already dead when I got there; he had died in 1938 after all, and I didn't get there till '52. But, in a sense, having access to those handwritten records and correspondence and whatnot of Wingate Todd makes him in a sense a mentor, not always to be followed, but to be looked at critically and you pick up some parts and discard other parts. And Holly Broadbent, who had been a very prominent figure—well, he still was at that time a very prominent figure in cranial facial growth. It was fun to have a relationship with him, but he was running a clinical practice and I really didn't see a great deal of him. He came in two day—two half days a week. He was rather overweight and a little elderly at the time, probably not all that old, but I suppose into his early 60s, and he would just sit at the table and go to sleep. But nevertheless he made data and records available to me. And the person who was his chief assistant and I became good friends, and that chief assistant taught me a great deal. So while not having a degree he, in a sense, was a mentor also. Idell Pyle was at Case Western and she is best known for her part in the Greulich Pyle Atlas of skeletal maturation, and she was very helpful in explaining details of the development of the atlas and so on. So in a sense she was a mentor.

Curry: This time I thought we'd talk a little bit more about some of your research work. You've kind of already mentioned what your primary interest was at the beginning of your career, was from the area of anatomy. Correct? Would you like to share a little bit more about how you would trace maybe your research over the years, your areas that you focused on?

Roche: Well, I'd say that it's never been focused, is perhaps one thing to say. It's always been eclectic. And at first, when I was starting to develop research interests, perhaps is the right way to put it, in the anatomy department I had a completely wrong attitude. And nobody told me it was the wrong attitude. I thought of questions and typically I would go to bones, sometimes the dissecting room, but typically to bones to answer the questions, and I would make observations and write them down. And when I got to the stage where I thought I knew the answer then I stopped. Nothing was written up, nothing was analyzed in a statistical fashion, I didn't read the literature. I just answered my own question and then passed on to something else. So that went on for about two years when there was no focus whatsoever. And that changed after I came back from the Fulbright when I was then heavily involved in organizing a growth study. It was said that you couldn't do this in Australia, this is something that you can do only with crazy Americans. No one will cooperate in it. Well, I put a lot of effort into recruiting people, and we actually had very little money. We had $600 to start the study and to run it and fortunately had $600 the next year also, but it's not a large amount of money. So we ran this longitudinal growth study and it grew very successful really in terms of people coming in when they're asked to come in, and I was successful in terms of getting volunteer help from two or three dentists and one doctor and a nutritionist from the biochemistry department and things of that sort. So one put it together without a financial expense or without much financial expense, but it meant a lot of work from my point of view. I had some attitudes that were wrong, you know, I've had many wrong attitudes over the years, and I felt that we ought to write letters to the parents every time the children come and letters to their doctor and to their dentist about what had been found in the physical examination that the child had and the dental examination that the child had. And of course, I felt you had to have copies of those letters, so I had to write them all by hand, because I didn't have a secretary, and then write them again and make copies, because there were no photocopiers in those days. So that meant—in itself was quite a lot of work. The other problem in beginning a longitudinal study is that if you're going to analyze the data in a serial fashion you have to wait quite a while before you've got enough serial data. So there were no publications. Now, fortunately in this department there was no pressure to publish, so although I graduated in medicine in 1946, I had one publication in '54, because when I was here as a Fulbright scholar I was told, It would be a good thing if you published something during the year. So I did write something. And then I did not publish again till 1959 when there were one or two, and then the publications started to come. There were two reasons for the delay, I think, one—three reasons. One is that you need a fair amount of data before you can publish anything. The second reason is I was busy writing all those letters to doctors and dentists, et cetera. And the third reason was that the head of the department wanted to be either—or insisted on being either the first author or at least the second author on anything that was published. And I went along with that for the first couple of publications, but then didn't feel that this was too fair since he hadn't thought of the question, he hadn't collected the data, he hadn't read any of the literature, he wouldn't read the draft, you know, he just wanted his name on it. So that slowed me down considerably and I didn't try very hard to publish anything.

Curry: Would you like to continue and talk a little bit more about how other influences were present on your research?

Roche: Yes. I think the very fact of working in an anatomy department and teaching gross anatomy definitely had an influence. In particular I was teaching dental students, so that turned me towards cranial facial growth. I have several publications in that area. Also, it turned me towards bones, because bones are very common around anatomy departments and they're clean and nice to handle and you don't have to go into a smelly dissecting room and so on. So there was work dealing with bones. It led me into a set of questions that were interesting and some of them are potentially of minor importance, but in general they're not—it's not applied information. It's not something that a clinician needs, not something that the general public needs. So after a while one moves away from that, one thinks it's a little bit sterile and it'd be better to work with something that was of more practical value. And the same in a sense applied later when I became involved with studies of children who altered their titles. When I began they were *mongols*, after a while there were *mongoloids*, later on they were *Down syndrome*, and later on they were *Trisomy 21*, but always the same group of children, and these

children with congenital deformities were studied serially. They were institutionalized, which is not so common these days, but in Australia in the ’50s and ’60s almost all these children were institutionalized. So I made many studies of growth and development in such children, but then stopped that around about 1965 because again it seemed to me a rather sterile field. It's nice to be able to describe—analyze now how long their fingers are or something like that, but what good does it do to anybody? So I moved away from that area because I didn't see that it had any practical application or any general significance, but it was something—you could measure something, you could describe something, you could report and people seemed to like the papers. But I became dissatisfied with the area so I moved away from that. So, in a sense, that was a discontinuity in the work if you like. I've not got back to that group of children.

Another discontinuity perhaps is that in Melbourne the study group were supposedly normal children, the general population. In addition, we had what I called a para-clinical group. We would take any child thought to be growing unusually, or maturing in an unusual fashion, who was referred by a physician. So we had many more of those children than we had of the normal children, but we did not have long-term serial records for those. We tried to get them back once a year, but they began at a variety of ages and most of them we only had two years of data, three years of data, four years of data. We spent a lot of time on those children and we made reports to the physician who referred them, but, with the exception of the tall girls, we did not publish anything from that para-clinical set of data. So in a sense that was a wasted effort. On the other hand, it may have helped the clinicians, it may have helped the parents, but it didn't help in terms of publishing or didn't help in terms of the literature, science, whatever.

Curry: How did it help the parents? I'm curious.

Roche: Well, one of the things we would do, for instance, is predict adult stature. And since these children—many of these children were short that was presumably helpful. Since we were assessing growth and some of them were predicted to become short adults and it was a time when impure extracts of growth hormone were becoming available it was one guide as to whether they needed to be treated, and a guide as to how effective the treatment was. So it—if you helped the child you helped parents, if you helped the physician you helped the parents. It's an interrelated mix here. You can't help one without the other.

Curry: I see. What do you think of your studies was one of the most significant?

Roche: I always think the last one I—last thing I—

Curry: Have done—

Roche: —write is always the most significant, and then the reviewers see it and then I realize how terrible it is, and then it's rejected and I'm sure it's terrible. I think—I don't know which are the most significant. Perhaps I think the methods—I think the most significant group are the method—ones dealing with methodology, because if they are of any benefit they'll be semi-permanent. They won't be—it's not that they won't be superseded, but they won't be superseded quite as quickly as those that describe relationships. So I think the most significant perhaps are the methodological group, and the methodological group mainly concern the measurement of maturity and the prediction of adult stature and the assessment of growth. And within that group of things there are many papers, and monographs, and books, but I think those are the—that's the area that is most significant.

Curry: Would you like to reflect a little bit more on your research funding experiences, the almighty dollar behind—

Roche: Well, it's come and gone. In Australia I started off with that small amount of money, as mentioned earlier, and that was coming out of departmental funds of obscure origin. After about two to three years I was funded by the National Health and Medical Research Council or something of that

sort, which was the equivalent of NIH in this country, but had much less money available and did not distribute in those days the money in a fashion similar to this country, but rather in a democratic fashion. They felt that each state had to get a fairly equal share, and then within the state every university had to get a fairly equal share and no department could get an undue amount, and then investigators couldn't take too much, so that sometimes the money that came to a person from that source was as low as ten dollars. They had two exceptions. They had a large—there was a large institute in Melbourne and one in Sydney that got special treatment, and they received large amounts of money. Then, when you come after them—I got to the stage of I think being probably the next highest, but I'm not quite certain about that. But I worked up from small amounts, like, around about $1,000 up to about $10,000 a year from National Health and Medical Research Council—that was its name. That was the major source of funding.

Then there was another fund, the Mental Health Research Fund, and from that we—that supported the work that was done with the Trisomy 21 children. That went on merrily for about six years and then that stopped—always for a political reason I think—and that stopped because they established a department of psychiatry at the university, brought in a professor to run it, then found that the dean of the medical school had forgotten about the fact that this person would need a secretary and other forms of support, so he talked the Mental Health Advisory Council, which was a state oper—agency, into transferring the research funds to supporting the professor. That would be supportive research, so we lost those funds. We at one time had money from the Huffield Foundation, and that's about the end of what we got in Melbourne.

Then, coming here, I've had continuous funding from NIH since I came here in one sense, but it's been overlapping. It hasn't always been the same proposal or the same study. And sometimes there have been three or four funded at the same time, but I've not got any funded at the moment because I've handed over such things to Roger Siervogel, because I've had enough of writing grant proposals and competing renewals and non-competing renewals, et cetera, so he now does all that and he's the principal investigator. I'm a co-investigator on several things, but no longer a principal investigator.

Curry: What do you find was your batting average in applying for grants, especially in the States? Did you feel like you had a pretty high success rate?

Roche: Oh yes.

Curry: Now they say it's almost like if—1 in 100, if you write one proposal you'll be lucky.

Roche: I don't know anybody who has written 100—

Curry: —get your foot in the door.

Roche: —but it's not easy now. It's much more, much more difficult now.

Curry: More competitive now?

Roche: Yes.

Curry: Right. Did you ever find—have an agency coming after you to do research saying, "We've got funds, we think you'd be—"

Roche: Yes, I've had that happen.

Curry: "—the right person." That's nice. Great, okay.

Roche: But for minor amounts, not for large amounts.

Curry: Great. We'll shift gears a little bit and talk about some of the places that you've worked and some institutions. Would you like to talk about some of those? I'm curious—

Roche: You'd like to know what the institutions were like or—you don't want just a list—

Curry: —no, that's kind of listed in your—

Roche: —and so on?

Curry: —in your résumé pretty much. But—

Roche: Well, I suppose we could start with the hospital, which was Saint Vincent's Hospital in Melbourne, where I worked as a resident from 1946 to part of 1948. It was a period of stretching out the medical course. You see, the medical course in Australia—and it may have changed now, I'm out of touch, but in those days was a six-year course. You went straight into it from high school and the first year was physics and chemistry and zoology and botany. And then, after that, you started anatomy and physiology and so on. Now, during the war—I did the medical course during the war. During the war it was collapsed into approximately five years by greatly reducing the vacations and pushing other things together a little bit, too. In addition, many of the instructors were in the armed forces, so that a lot of the instructing was done by retired general practitioners who really were long retired from textbooks and were really not much help, but were doing the best that they could. So that it was not a medical course under the ideal circumstances and it was also done under a threat of going into the army. If you failed in one examination at any time without a very good excuse, you were in the army. So the class got small. It decreased in size very rapidly at the beginning. After that, it didn't decrease very much. The survivors after the first two years or so did in fact survive for the six years with rare exceptions.

Life in the hospital was, again, I think a little bit different from being a resident here. There was much less emphasis on education and much bigger emphasis on service, so one was on duty for very long hours. I had two weekends off out of five—they started at midday Saturday and ended at 6:00 Sunday—and one evening a week off, but the rest of the time you had to stay within the hospital, and you were not always on duty, but you were sometimes on duty, sometimes first call, sometimes second call, sometimes allowed to sleep. But it—they were long days and very, very boring. That's when I found out I didn't like medical practice. The hospital turned me off medical practice. The other thing I found out was the back biting that goes on within the hospital and the sticking knives into people's backs and so on and whatnot was all very distressing. It was not a happy environment really. So that basically I was bored while I was in the hospital, tired and bored and glad to get out of them. So that's one institution.

The next institution was the anatomy department at Melbourne. Now, that was—you'll start to think that I don't like any institution where I work and you'll wonder what I'm going to say about Wright State. But the anatomy department at Melbourne was again a place for the indolent. The typical working day began at 9:00, but this is for the staff now. I'm not talking about students. And people would get there perhaps about ten past 9:00, but then it was necessary to take off your hat and coat, and talk to people about what the traffic had been like, and they'd get to sitting down at a desk by about 9:30. Then, about 10:15 it was time to accumulate in a room to have tea—it wasn't coffee, it was tea—and start drinking tea. And of course, you wouldn't go if you had a lecture from 10:00-11:00, but for those who didn't have any fixed activity at that time they would accumulate and drink tea for about half an hour. Then, about a quarter to 12:00 it's time to get—start getting ready to stop work for lunch, so lunch is theoretically from 12:00-1:00, but it's more like a quarter to 12:00 till a quarter past 1:00. Then, at 3:00 you accumulate again to drink tea for half an hour. Then, at a quarter to 5:00 a big bell goes off, a very loud one, and this is to warn students in the dissecting room that they've got to be out by 5:00, so they all start coming out and washing their hands and all that sort of thing. And that's the time when the staff start putting on their coats and getting ready to drive home. So really, it's a very short working day. I probably followed that pattern for about two years within the anatomy

department, but then—and then had the Fulbright, and then came back and developed a sort of work ethic I guess. I would get there a little early, and I—after about three to four years I gave up on drinking tea in a communal fashion. I drank coffee at my desk and I still left at—on the tick of 5:00, because that was a critical decision. We lived about eight miles from the anatomy department and if I left at 5:00 I could drive home in 20 minutes. If I left at five past 5:00 it would take 45 minutes, so I did leave at 5:00. That was the end of my working day at the anatomy department, but then I worked at home after dinner. I was working hard at this stage, working at weekends and so on. There was one year even when I worked every day—

Curry: Oh my goodness.

Roche: —and by that I mean 365 days.

Curry: What special project were you working—

Roche: Yes, I was trying—

Curry: —was that the longitudinal or—

Roche: —that was—no, that was the doctor of science. See doctor of science in an Australian university is different from a PhD. It's a much more advanced degree than a PhD. And it's something that physicists and chemists tend to get instead of a PhD, but medical people don't and I was the first medical graduate to get a doctor of science in around about 24, 25 years, whereas in the Australian context, a PhD essentially means that you're capable of doing independent research. A doctor of science degree means that you have contributed significantly to human knowledge and understanding, and that's more difficult. So when I had had the PhD for some time and I had a fair number of publications I went and saw the teacher, he was a professor, head of the department really of physiology whom I referred to without being specific earlier, and gave him I think the publications that I had written—perhaps just a list of them, but I think the publications—for his advice about whether I could use this as a basis for a doctor of science. And he—it must have been the publications I think, because he read them, and I went and saw him a week later or so, and he said, "It's no d** good." He was very blunt, you know. "It's no d** good," he said, "you're just boring little holes all over the place that are very good, but they're not joined up in any fashion and you've got to connect them up somehow. And, you know, it's going to take a long time, and if you come back and see me in about five or six years and tell me what's happened then I'll give you advice again," sort of thing. So I worked very hard for the next two years and then went back and saw him, and he said, "This is fine," you know, "this is—this shouldn't be any problem," you know, so I got a doctor of science.

Curry: Dr. Roche, how did the growth studies that you worked on fit into the other research or other goings on of the anatomy department?

Roche: Well, the anatomy department really was oriented towards teaching. There were lots of students by the time you count the fact that medical students did anatomy for one and two-thirds years, and then there were other sorts of students such as nurses, dentists, physiotherapists, et cetera, there were around about 1,200 students altogether. So teaching was a major part of the department's activities. There was some other research bound up with neuroanatomy, really neurophysiology, and that was about all there was from a research point of view. So the growth unit didn't really fit in very nicely at all. Whether you think of it topographically or whether you think of it from a point of view of content—we were placed just outside the dissecting room for a start. We—it was—the anatomy department was a closed building. You were not allowed in unless you had a reason to be there, because there was an attitude that the general public shouldn't be looking at what's going on in the dissecting room. But nevertheless, we had mothers and infants and children coming, and I might say here that they came without any financial rewards, without having their travel costs paid or anything else, because we didn't have money. But nevertheless, they did come, so that from that point of view it was unusual in an anatomy department, unusual to have physical examinations going on, dental

examinations, to be taking head—standardized head radiographs called cephalometric radiographs. They were taken in another building. They had to walk over to this other building, be escorted over and brought back again. So all in all it was a strange sort of set of activities to be occurring in an anatomy department. The other thing that was strange about it perhaps in comparison with the American experience here is that in those days Australia was a very restricted society from a point of view of nudity or sex or pornography or anything like that. Playboy couldn't be sold for arguments sake. Nevertheless, all those measurements, and we took photographs in the growth unit, were all done nude, and I measured the boys and the girls, I stopped measuring the girls when they were around 12, 13 years of age. And I don't know whether I should be proud or embarrassed to say that some of the girls objected to me dropping out and a female physician taking over, but it did in fact happen. But now, in this country, in the growth studies we do in Yellow Springs despite the very different environment with regard to whether Playboy can be sold and things of that nature, those children are never nude, and they wear minimal clothing however, and they're examined one at a time for privacy reasons. In Australia we had them all together, which had advantages and disadvantages. We had about six to eight at a time, there were mixed sexes, but they were all children who had birthdays at about the same time. And that had one advantage that a year later—their early examinations were at six month intervals, but the next set of examinations approximately the same eight would come in together. So those mothers got to know each other; the kids got to know each other. But we had kids in dressing gowns wandering all over the anatomy department, because not all—we were not in a close knit set of rooms somewhere 50 yards down the passage, and somewhere along here—and some were to the right, some to the left. So it must have been confusing I think for the students to see these little kids wandering around in dressing gowns, but they were no actual complaints about it. We lost a child one day there.

Curry: Oh!

Roche: We lost a three year old who wandered out the door onto the university grounds and then across a major road where there were streetcars running up and down the road, and finished up 200-300 yards the other side of the road where a pedestrian stopped the child and took the child to the police station. And we didn't know where the child was, but eventually somebody thought of ringing the police station, and that's where the child was. So the end of the story was satisfactory, but it was worrying while it occurred. So that was rather what the relationship was in the anatomy department. I think the growth unit was rather a misfit, but it was in one sense the best place for it to be, because it was physical structure that we were looking at, so it was logical from that point of view. For it to have been in, let us say, physiology would have meant you'd have to develop a completely different set of questions, and if it had been in psychology where they did have what they called a growth study also, they were studying an entirely different set of variables to what we were. So we were concerned just with size and some aspects of regional body composition, proportions, especially around the cranial facial area, maturation, dietary intakes, and we had two psychological assessments of the children just to assure ourselves that they were normal. But we had no intention of getting into analyses of psychological data.

Curry: After Melbourne, where did you—

Roche: Well, I became dissatisfied in Melbourne. I felt that I was not getting the academic promotions that I should get and when I had tried to get other jobs in Australia I always seemed to run second. And then, unexpectedly, I was asked to come to Yellow Springs. I had not applied for the position. And I had spent a lot of time reading literature and regarded this as the best job in the world for the sort of work that I was interested in. And that was true; there was no doubt it was the best job in the world.

Curry: This was the Fels—

Roche: That was the position at Fels. That doesn't mean it was an easy transition. It was a very different lifestyle. There were no breaks for tea, there were—that was true for everybody. It was a

very structured environment in many ways. There were little areas into which you penetrated at your own risk and in general the people running them were friendly, and it was a collegial sort of group of professionals. But nevertheless, the place was broken up into very well defined topographic and subject areas, which is exactly what it was not meant to be. It was meant to be a multi-disciplined study. There were the multiple disciplines, but there was never any interdisciplinary study, never any time when people from one discipline worked with people from another discipline. It's not quite true to say never, but it was very close to never. That was one thing that was odd.

Another thing that was odd was the rigorous control by the director. I was always told that he was much worse 10 or 20 years earlier, but he was bad enough at the time I came to not want him to be younger. For instance, the transition was made difficult by the fact that the visa was delayed. It was a time when the—I think it's the department of justice, but whoever's responsible for this at the Washington level was reorganizing the system and putting everything into a computer. Meanwhile, nobody could get a visa till everybody had data entered into the computer. So instead of being able to travel here around about March or April I was delayed until August and then arranged to begin on the first of September. But arrived in early spring—it was around about the twenty-sixth, twenty-seventh of August—together with quite a few papers and things that I regarded as important that I carried with me and that I wanted to put in my office. I was told I couldn't do that, couldn't have access to the room, couldn't have a key or anything like that, because I was not yet employed. Well, that seemed a little odd to me. But then, when I was employed on the first of September the next thing that seemed odd was that I got a key that would open every door. This was very different to Melbourne where you just had a key to the outside door of the building and then a key to your own room, but nothing in between unless you were a janitor or something like that, so that things were different. On one story—I don't want to spend all this time telling you anecdotes, but I'll just tell you one story—that a day or two after I arrived the director said, "We haven't got a budget for your department. We need a budget. Should have had it on the thirtieth of June or before that." You know? So I said, "Well, what do I have to pay from this budget?" He said, "You know what you have to pay, kind of buy x-ray film and all that sort of thing." I said, "Do I have to pay salaries out of this budget?" He said, "No, of course you don't." You know, it was a stupid question. So I went away and thought, Well, this won't be too hard. I'll add five percent to last year's budget. So I went to his secretary and said, "Can I have a copy of last year's budget?" She said, "We didn't have a budget last year." So I had very few people working in the department who reported to me. There were two groups who were independently funded, and that's—the relationship with them is another story. So there were only about three people working for me, so I went to the person who was the leader of that group if you wish, and said, "Who do we buy our x-ray film from?" And she didn't know, or pretended she didn't know. So after a few questions like that and a few answers like that I went back to the director and said, "I can't put this budget together, because I don't have any facts. I don't know what anything costs or who we buy it from or anything." He said, "Well, this is what you want," and he took a little bit of paper and wrote some numbers on it and some items. It was a very small amount of money. It was around about $2,500. He said, "Have this typed and then I'll approve it." Well I'd said I didn't know what we needed, so I was caught here. So I said, "Righto," so I had it typed, went along to him, he crossed out the $2,500, reduced it to $2,000 and then it came back. And that was my budget for the year. I don't know what was really spent, but I think it was probably about $12,000 on those items. It had no relationship whatsoever to that budget. And the next year, which was his last year, he didn't ask for a budget, we just went along buying what seemed necessary. But if you wanted anything large then you had to talk to him, and that certainly was a difficult procedure.

Curry: That's amazing.

Roche: But when I came to Yellow Springs, as well as thinking it was the best job, I also thought it was a comfortable sort of job, nice to be working for a foundation which I thought was real rock solid. You know? You had in essence no worry about future funding. But that didn't last. The people who mattered at the institute did not manage their public relations or interrelationships with the—what we call the member directors of the Fels Fund, who were the people who controlled the fund. I don't mean that they insulted them, but they just didn't communicate with them in an effective fashion.

The fund made a visit to Yellow Springs annually to see what was happening, but of the eight members only about three would come and it would always be the same three, and they only came twice while I—two, maybe three times while I was there. It tailed off. They would complain about the report that they got, that they couldn't—they would typically say they couldn't understand a word of it. And these were either bankers or lawyers. Now, it was an exaggeration to say they couldn't understand a word, but it might be true to say they couldn't understand a sentence or a paragraph, because most people wrote the reports in a style that was meant to confuse rather than enlighten, and that didn't help when it came to getting support from the fund. So the fund gradually came around to—they weren't terribly wealthy compared with the Rockefeller or something like that. They had reserves that wobbled between about 15 million and 20 million, and they were well meaning, well-intentioned people, but they had an attitude developing that there was a severe shortage of foundation money in Philadelphia, and that's where the fund was located. That's where they were all located and this money had been—come out of Philadelphia and more of it should be spent in Philadelphia. Some of it was being spent in Philadelphia, but they didn't like it coming to—some of it coming to Yellow Springs. So they looked around for an alternative, and there were various alternatives, but the one that was decided on was to join the medical school at Wright State, which was established I think in '75, '76, so that it was a new medical school, and there was an image that the medical school would gain some research activity quickly, and of course they got the building and they got the contents of the building, and all the senior staff in the building became members of Wright State staff at various levels. But it didn't happen as easily as all that. This transition took about three years and during that time almost all the psychologists left and went elsewhere, and no senior people were replaced between 1968 when I came and 1977 when we joined Wright State. So there was a big dropping off in the professional staff and those that did remain were a lot older and less active than they had been previously, so it wasn't such a strong organization by the time it joined Wright State as it had been before.

Then, of course, joining Wright State wasn't easy, because we didn't really fit into the medical school. We were not physicians, or surgeons, or anesthesiologists or whatever else there might be in the medical school. So the—for the biochemists it was pretty easy. They went into biomedical biological chemistry, or whatever it was called at that time, and subsequently they were transferred to the campus. The psychologists, not many of them remaining, went into psychiatry. Frank Falkner, who at that stage was the director, became an associate dean for development in the medical school, but after about a year and a half left, went to another university. And the growth and development—it was called growth and genetics, the department of growth and genetics by then—had become the section of growth and genetics. There'd been a changing of words, and Roger Siervogel, Cameron Chumlea and myself—Cameron had not been there at the time of the changeover—we became members of the department of pediatrics, and I had a joint appointment in obstetrics and gynecology. So we entered the medical school, we did a modest amount of teaching in pediatrics, which has got a little bit less each year—

(Tape paused)

Well, after joining Wright State and moving—and I'll be personal about it rather than thinking about the group—moving into pediatrics and obstetrics and gynecology, there was a small amount of teaching that I did in pediatrics, none in obstetrics and gynecology, and that lasted for maybe three to four years and then it was taken over by Cameron Chumlea. Within pediatrics I was responsible for trying to organize research by means of organizing research seminars and later being chairperson of a research committee that made recommendations about distributing money that they had available from a women's auxiliary. And I did both of those tasks for, oh, approximately ten years.

But nevertheless, we were still a misfit in pediatrics, because we were not doing clinical work and we were not actively teaching students. I mix up a bit here between whether I talk about *we* or whether I talk about *I*. In obstetrics and gynecology they seemed to like me, and we were good friends, and they were a very congenial group of colleagues. I was on their research committee, but the research that they were involved in was different from what I was used to. It was more a matter of testing drugs for drug companies and dealing with medical conditions that I knew very little about. Then during this

period there were two deans in the medical school, Dean Belgan and Dean Sawyer, and neither of those was supportive, shall we say, of the Fels Research Institute. And it was during that period that the term Fels Research Institute was dropped, and I think it really had to be dropped because it had broken up into various pieces, and the only piece remaining was the old growth and genetics portion which we managed to change the name of to Division of Human Biology, so that when the third dean came, that was Dean Kaplan, he had a very positive view towards me and towards the group who worked with me, and that made a tremendous difference with regard to the way we felt and the way we felt about Wright State, and the way in which Wright State in general felt about us. All of a sudden we found we were appreciated instead of being outcasts. And he was responsible for the fact that I got the title of University Professor, which pleases me very much, so that things have been very comfortable over the last few years. Now, with the present dean, Dean Goldenberg, a positive attitude has continued. He's been very positive. So that perhaps describes relationships with Wright State.

Curry: Great. Going on a little bit, let's see—would you like to describe your experiences in the area of applied child development and research or applied research?

Roche: I think a great deal of it has been applied, some of it more obviously applied than others. I've spoken about predicting adult stature for individuals. That's certainly something that one can apply. The development of growth charts, with which I know you are familiar, is another applied area. The development of incremental growth charts, charts that show you the expected growth during intervals—we did it for six months, six-month intervals, we've done it for one-month intervals. The development of correction factors to be added to or subtracted from observed data for recumbent length and stature in children depended on the average stature of the two parents is another large applied area. Methods for the assessment of maturity of the knee and of the skeletal maturity of the knee and of the hand/wrist, again, highly applied areas. Methods for the estimation of body composition variables from data that are easy to obtain is another large applied area. When I speak of body composition variables I'm really referring to fat free mass, which is the weight of those parts of the body that are not adipose tissue, and percent body fat, which is the percentage of body weight that is accounted for by adipose tissue. When I talk about variables that are easy to obtain I'm talking about anthropometric variables such as weight and stature, and procedures such as impedance. Now, impedance is another applied area that we've done a lot of work in. Impedance relates to the resistance of the body to the passage of a small alternating current and that resistance depends on the volume and shape and composition of the conductor, and the conductor is fat free mass with the exception of bone, so that if you combine resistance with weight and stature and a few circumferences you can predict fat free mass with quite a lot of accuracy, much easier to do than measuring it by another technique such as underwater weighing.

Curry: Perhaps we could talk a little bit more now about some of your experiences with SRCD. For example, when did you first join SRCD?

Roche: I believe it was 1952 at the suggestion of Idell Pyle, who was a member. And I think it was a reasonable decision at the time. I think Idell was looking backwards, and I was perhaps looking backwards also, because I had mentioned that I read a great deal of literature. I used to go home for the weekend with the trunk of the car full of borrowed volumes from the library, and I'm not exaggerating. See, SRCD in its early days, and I mean the ’30s and ’40s, was honestly multidisciplinary. There was probably close to an equal representation in the members of people dealing with behavioral questions as to people dealing with physical variables and questions. And that was, however, probably changing by 1952, but I was not aware that it was changing as much as it was, and the change accelerated after 1952. So my—that was why I joined, because it was suggested to me I might, but I had no contact with SRCD except to get—receive the publications until after I came to this country in 1968. You see, that's part of being in Australia, that you don't attend the annual meetings of societies. I've been in the American Association of Physical Anthropologists since probably about 1948, but did not attend an annual meeting until 1969, and then met many people whom I knew very well from the literature and all of a sudden saw them face to face, which is a very pleasant experience. So I believe the first SRCD meeting I attended was in Philadelphia in 1973 when I organized a symposium on

puberty, and that was—in one sense went well and another sense was a disaster. It—I think we had good speakers; we had an audience that was interested, but it was not a large audience. It was an audience of probably about 25 and that was because it was very difficult to get into the room, because a little bit further along the passage approximate to where we were there was a large room filled to overflowing, and you could not walk past it where some report was occurring on an evaluation of Head Start, and that was—

Curry: A hot topic.

Roche: —very popular at the time, probably a very hot topic now, too. And we didn't do very well in terms of the size of audience, but it was a nice symposium and that was the first meeting that I attended.

Curry: Along—over the years what other activities or involvement have you had in the Society?

Roche: Well, the involvement in the Society has been twofold I guess. One is to organize symposia, and I did organize one for the Detroit meeting—for the New Orleans meeting in '77 on secular trends—it was later published as a monograph—and one on measurement of physical growth in Detroit in '83; they were the only ones I think. And my sort of organizational involvement with the Society has been in terms of trying to increase its interdisciplinary nature, and I think that was a disaster. It was a disaster for two reasons, I think. One—or three reasons—one is that it failed. I did not really increase the interdisciplinary representation in the Society on a permanent basis, but I at least got people to talk about it and think about it. It was a disaster because I could not get the other disciplines, non—I mean, when I speak of behavior I mean in a very broad sense. I include education with behavior and so on, the physical disciplines, to be accepted as being of equal merit. Topics that were of interest to the physical disciplines would only be accepted if the topic was of interest to somebody working in the behavioral area, or the educational area, or psychiatry or whatever. And I think that was unfair, because they could have topics which the physical people who were not the least bit interested in, but the physical people couldn't have topics that they were not interested in. So it was not a fair relationship. Nevertheless, I was on the Interdisciplinary Committee, which the purpose of that was to increase interdisciplinary activities and representations within the Society. I later became chairperson of that; I was on the committee from '77-'83 and was the chairperson for the last two years. Now, that committee did manage, by personal contacts more than anything else, and by inviting people to symposia and saying, Why don't you join, and so on, to increase the interdisciplinary membership, and it probably came up from something like one percent to two percent. And that lasted for maybe five or six years when there was a very unhappy group of people who said, I've been cheated, you know, I've been asked to join a Society where I thought there would be something I'd be interested in and so on, and now there's nothing that I'm interested in, and they dropped out, and it quickly went back to one percent again or—I don't know a—have not seen a recent list of members. It might be lower than one percent now. I dropped out myself; I dropped out in '87 because I was not getting anything out of SRCD that interested me. I realized it was a good society for other people. I'm not denying that. But there was nothing in it of a physical growth nature. I was on the Social Poli—well, I was on the Social Policy Committee, I was on the selection committee for congressional fellowships one year, I was the chairperson for one year of the Program Evaluation Committee that was meant to evaluate the previous program to see how interdisciplinary it was and see whether the interdisciplinary content of that program could have been improved, more interdisciplinary things could be introduced at the next meeting is what it really comes down to. I was on the Governing Council from '75-'81, not as an elected member, but as an appointed member. There is a mechanism for appointing a non-behavioral person and that's—I would never have been elected of course.

Curry: Interesting. Do—the people that you saw leaving the organization because of its lack of emphasis on perhaps the physical aspects of development, do—what organization do you see them—

Roche: Oh—

Curry: —did they, did they—

Roche: —well, that—some were pediatricians—

Curry: —right.

Roche: —some were physical anthropologists and human biologists, and perhaps some would think of their primary organization as being sports medicine. But pediatricians would be the biggest group.

Curry: Just as—

Roche: And nutritionists also in there, too.

Curry: —have you been involved at all or associated with the group that, I think—are they called the Society for Developmental Pediatricians that—

Roche: No, I've not been involved in that.

Curry: —because that sounds—

Roche: I know it exists.

Curry: —that group sounds more like on your wavelength per se—

Roche: No, not mine.

Curry: —not really?

Roche: No, no, no, no. The societies that I feel involved with, if you like to put it that way, are American Heart Association, in particular their council on epidemiology, the American College of Sports Medicine—

Curry: Okay.

Roche: —American Society of Clinical Nutrition, and the Human Biology Council. Those are the ones I would think are—no longer physical anthropology. That's also gone in what, to me, is the wrong direction. It's to be expected. Things become more and more specialized, but they're not completely prehistoric or almost all prehistoric. Some of it's comparative anatomy, comparative behavior, whereas living populations have all moved to American—human biology, Human Biology Council. The journal is the *American Journal of Human Biology*. So those are the groups. If I had to sort of pick one group that I would—one or two groups that I would put high on the list it would be *American Journal of Clinical Nutrition and Cardiovascular Epidemiology*, which meets as part of, but separate from, the American Heart Association. They have their own sub-meetings, if you like, specialist meetings.

Curry: So I think maybe we've covered this. What important changes do you feel have occurred in SRCD and its activities during your association with it? Would you summarize that perhaps as the change of the focus of the group perhaps?

Roche: Yes, I think it's reflected in the nature of the membership, and the membership has become entirely non-physical in its interests or its origins. So the membership is entirely, I believe, psychologists of various sorts, psychiatrists, education people, perhaps some people who are social work perhaps—well, if they're nurses, they're special sorts of nurses. They're not nurses interested in how do you keep temperature normal or something. They're a special group of nurses.

Curry: Okay.

Roche: But there are some nurses; that's true, and there are probably still some ped—there are some pediatricians. No doubt about that. There are still some pediatricians, but it's a very special group of nurses and of pediatricians.

Curry: Right, because Berry Brazelton has been president lately.

Roche: Yes, yeah.

Curry: Basically, in the field of child development, do you have any feelings about the history of it as participate, any major continuities or discontinuities? A lot of this was kind of indirectly touched on. I thought maybe you might want to—

Roche: Yes. Well—

Curry: —pull some of it together.

Roche: —yes, I think so. But there are continuities and discontinuities and—but I can only speak of it in relation to physical things, not behavioral. There's a discontinuity—for a start, there's a discontinuity in the sense that multidisciplined longitudinal studies are not being conducted. There used to be about six major ones in this country; now there are none. There's only one longitudinal study of growth, truly long-term longitudinal study of growth being conducted, and that's the one in Yellow Springs. That's a discontinuity.

There's another discontinuity. It was that the previous studies stopped at ages ranging from 16-18 except for a few of them where there were follow ups, typically at about 35 and at about 45 years of age, but very modified examinations, and they were ones that I managed to get funding for. Otherwise, they stopped at the time, more or less, when the child stops growing and I think that's throwing out the baby with the—instead of—with dishwater, if you like. At Fels we've continued, so we're still enrolling them. The oldest of the participants is now 63 and they're—we are as interested in the adults as we are in the children.

I think another continuity/discontinuity comment that I would make is that the longitudinal studies made the mistake until the last 20 odd years in analyzing data cross-sectionally. They have serial data, but they did not look at them from a serial point of view, so they made cross-sectional analyses, didn't even look at increments typically, lots of correlations, lots of regressions, and that's about it.

Curry: Great. So how—what should they have done with the longitudinal studies, what changes would you have seen done?

Roche: Well, I think the first thing you perhaps have to do is get help from a statistician and realize in all the time that you've gone through a lot of trouble to collect serial data. Therefore, you ought to make some use of the serial nature of the data and that's not something that the average person can do. It typically requires professional statistical help. And apart from the papers that deal with methodology that have been produced from Fels, I'd say that at least 80% of the remaining papers from Fels have analyzed the data in a serial fashion. And that's one discontinuity, if you like, it's a change, it's something that was not done in the other longitudinal studies. The other longitudinal studies stopped publishing approximately the early ’60s and at that time had made very few serial analyses of their data. We have made more serial analyses of other people's data than the other people did themselves, I think, because we do have the data from the major longitudinal studies for the variables we're interested in in Yellow Springs, and we have used them in some collaborative work. We've done the analyses, there's been joint authorship and so on, so some serial analyses have been made of those data that were collected in the past. If you think about the future of longitudinal studies, I think it—one has to say it's dismal, because it's very difficult to get funding for anything of a research nature

now, and if you ask for funding for a longitudinal study you couldn't contemplate asking for funding for 20 years. You'd have to ask for funding for three, four, five years and you'd have to make promises about what you would deliver at the end of the three, four or five years, what you would have published. Well, it takes 12 months to get a manuscript published, so you're talking about the work being—somewhat being completed in two, three or four years and there's very little that you can complete from a longitudinal study in two, three or four years, so that it's very difficult to understand how new studies could begin. I just hope that the present study at Fels can continue. Whether it continues or not depends on whether the right questions are being asked and whether they are being addressed in the best way, and whether good questions are being developed in a continuing sort of fashion. All the time the questions have to be of interest to people who might provide the funds, and if you think about NIH providing the funds, and they're the most obvious source, therefore the questions have to be health related. They can't be just interest related, that you would like to know how big the ear is or something like this. This will not result in funding from NIH. You have to have a health-related question, so that's why our major emphasis now is not so much on growth, but on body composition and on risk factors for cardiovascular disease. But we are also interested in newer technologies. We're interested in, and working with, magnetic resonance imaging, have worked with computer tomography but have moved away from that because of the radiation burdens, are working with magnetic resonance imaging down at Kettering mainly as a validation tool, but partly in collaboration with them, and they are the front runners in this, not me, in using magnetic resonance spectroscopy to determine the composition of adipose tissue, both subcutaneous and deep in terms of how much of it is saturated fat, how much is unsaturated, and what are the average chain links of fatty acids. We're working with a colleague in Denmark to validate high frequency energy absorption. This is a procedure for measuring—hopefully is a procedure for measuring regional body composition. It's a matter of using a nine-volt battery, putting a—something that looks like a thick tape measure around a limb, and assuming that all the electrolytes are in the fat free mass, there are none in adipose tissue, that there are none in bone, and by calibration against beakers of known circumferences and containing known concentrations of electrolytes, it looks at this stage as if we can get good estimates of muscle mass in cross sections of limbs, essentially noninvasively and with something that's easily portable and comparatively cheap. I am convinced that muscle is the neglected tissue in the body, that there's been far too much emphasis on fat and adipose tissue and far too much emphasis on bone, and we've neglected muscle for a very good reason, that it's very difficult to measure. And this will not measure whole body muscle, but it will measure regional muscle, I believe. They—we're into mach three, we're into the third set of modified tapes, and the third set is certainly highly replicable. We're halfway through finding out whether the data are valid by comparison with magnetic resonance imaging.

Curry: Wow.

Roche: This is one area we're interested in. The other area I'm interested in, but it's not turned into active research yet, but thinking about the whole topic Sarcopenia. Sarcopenia refers to a lack of muscle, lack of—literally a lack of flesh. Flesh really is muscle—so a lack of muscle, and does a lack of muscle really matter, and I'm convinced that it does in the elderly in particular, that low values for body mass index, which is a combination of weight and stature, are associated with higher mortality rates and that that is due to a lack of muscle. And also estimates of muscle area from anthropometry derived from arm circumference and the triceps skin fold are associated with mortality rates, at least in middle aged men followed for 25 years. No comparable study of women. And there are other less convincing sets of data in the literature. So I'm convinced that low levels of muscle are important and I speculate that it's different in terms of which muscle is important from what it is for adipose tissue, where there's a lot of reason to believe that abdominal adipose tissue, and in particular deep or visceral adipose tissue, is the more important. I think with muscle it's the muscle in the extremities that's more important, because that's where 65% of it is, and falls, fractures are important in the elderly. And also, muscle is a storehouse for protein and for micronutrients, and people who lack muscle also lack immunocompetence, so there are many reasons to think that muscle is important, not just as to whether you can get the top off the beer bottle, but for other reasons as well.

Curry: All right. Well, that should probably have a lot of implications for getting funding from NIH what with the health promotional—

Roche: Yes, I think one of the—

Curry: —right now.

Roche: —good things about muscle, and particularly in the elderly, is how reversible it is. You—we all know how difficult it is to lose adipose tissue. It's very easy to gain muscle. You only have to train for maybe 10, 15 minutes every second day and you can increase your muscle strength by—you can double your muscle strength. This is true for the elderly. If you're already a trained athlete you're not going to double your muscle strength like that, but if you're a sedentary, elderly individual you can improve very, very rapidly.

Curry: That's great. Anything else related to the field before we go to the next area at all that you'd like to focus on?

Roche: Well, the field keeps changing. The way I define the field keeps changing, because I've not been—as I said earlier, I've not been focused so that the things I'm interested in change from decade to decade. There's not going to be any more decades. I'm getting near the end. But for the areas I'm interested in, yes, I think they are changing. I think we have reached a stage where we know how to measure total body composition, but the methods are a little difficult to apply, but I think we know what we need and we can measure what we want. But there are some people who can't comply with the procedures.

I think with regard to regional body composition we're—we can measure that satisfactorily now, but magnetic resonance imaging is very expensive, and there's not—cannot be applied to large numbers of people. And again, there are people who cannot do magnetic resonance imaging; too big to fit in is the problem, so that I think the field is improving conceptually and so on, but not from a point of view of practical—from a point of view of practical application there are still limitations. From a point of view of growth, I think we've done everything we need to do except that the growth chart revision is a question that will be addressed in Washington in about two weeks' time. I am co-chairperson of a group to look at that problem; this has been looked at ineffectively over the last three years. Now, as a member of that group—but it did not work very well. I hope the second group, which is working with a different government agency, will work more effectively. There may or may not be a need to revise the growth charts, but there probably is and there probably will be separate growth charts for Mexican Americans and for Blacks in about four years' time, but not until then because there are data becoming available that were not available previously, and there's lots of statistical problems with the present growth charts. So that's one other way in which I see the field changing.

The way I would like to see it change is to become involved again—in terms of our work, involved again in the para-clinical. I would like to have access to relatively large numbers of patients, but they've got to fit special criteria; they've got to live long enough in order to get serial data and they've got to have relatively homogenous conditions. No medical conditions are truly homogenous, but you'd like them to be somewhat homogenous, especially homogenous as to treated, or untreated, or getting the same drug or something so that you can make some sense out of the data instead of just having one of each type. I would like to see us continue with the collection of data from the present participants and their offspring. I would like to see more work with the genetics. We now are deeply involved in genetics in characterization of DNA from Fels participants and looking for genetic variations that can be potentially related to differences in growth patterns with individuals. So these are all, to me, exciting possibilities.

Curry: Great.

Roche: And we're getting hormone data these days that have not yet been analyzed.

Curry: Maybe we'll switch gears now and focus on some personal aspects. Would you like to tell us a little bit about some of your personal interests, or your family—

Roche: Well, I'm interested in the family. I'm not going to deny that or else I'd perhaps be run out of the room. My wife and I have been married since 1945, and we have three children. One lives—two live nearby; one in Enon, one in Beavercreek. If this tape goes anywhere else I'll say they're approximately ten miles from Yellow Springs. And the other lives in Boston. We have very—always had very good relationships with all of them. We're a very close-knit family.

With regard to my personal interests, these days I do not work at home at night, because my interests really are in reading. The reading—I have to read some magazines that come, because you feel if you bought them you better read them. But they are more nuisance than anything and we do not get a newspaper, because we think—we both agree that's an awful waste of time. We only get the Yellow Springs paper, because you do need to know who's died and that's about all you find in it that you can believe. So I read books, and I differ—a fairly even balance between history and novels. I don't like to be all into one or all into the other. The main interest that my wife and I share is an interest in art history, and when I talked to you earlier about that need to get things together for the doctor of science in a hurry, that also happened to coincide with the three years that we did art history together and that made it even more difficult. We did not sit through examinations but we attended all the lectures, and we wrote the essays, and we went to the seminars and things like that, and we enjoyed that. We ori—we were interested originally and we became much more interested as it went along. Now my wife has—is much better and better informed about all this than I am, because we often go away to meetings together and she spends her time in the local museum and I spend my time listening to people give papers, so that she gets much more exposure to art than I do these days. But we're both very interested. Her other interest is in Shakespeare. There's a Shakespeare club for women only in Yellow Springs that she has—she is very active in. We used to be interested in gardening, but as you get older you decide you'll do less and less of less and less.

Curry: All right. Do you feel your family had any impact on the research or scientific contributions that you—

Roche: Not in a very direct fashion. I was once asked by a newspaper reporter on the telephone whether I was interested in growth because I was a dwarf. So it's not—the research interests have not grown out of a family crisis or something, and it's not because we've got a deformed child or something like that. That's not why I was interested in the Trisomy 21 group, so that it's hard to say that there's been a direct influence. I'd say that they've been very accommodating; the children have been—when they were younger have been accommodating to my working and so on, and I think they suffered somewhat from that, that I spent perhaps less time with them going to football games and whatnot than I perhaps should have done. I think my wife has been very accommodating of an evening. But we're not by nature joiners. We don't tend to join a lot of societies and clubs and mix and mingle a great deal. We have some friends and do visit people and so on. I don't want to sound as if we're completely recluses, but we are towards that end of the spectrum rather than extroverts and meeters, and greeters, and joiners, and shakers, and doers, you know? We tend to keep to ourselves rather.

Curry: Great. And how many grandchildren do you have?

Roche: One—two, two—

Curry: Two?

Roche: —oh, it would be a disaster to say one. Two, two, who are 12 and 10, boy and girl, in that order. They live in Beavercreek and they spend quite a lot of time in our house, because our daughter did rehabilitation at Wright State and now has come back to do education and is just at the end of it. The finals that occur in about two weeks' time will be the end of the course work, so during the period

that she has been—come back as a full-time student my wife has done a fair amount of child minding. And we appreciate them.

Curry: Do you think your work has influenced what your children have gone into at all?

Roche: Well, they've gone into the growth study for a start.

Curry: Did they really?

Roche: Not the children, the grandchildren.

Curry: The grandchildren are in—

Roche: Oh yes, since they were born. No, it has not influenced them, no.

Curry: Okay. Anything else you'd like to—final closing—

Roche: I think we might round it off by saying that retirement has to occur. It's either retire or die, one of the two had to occur one day. I don't know when I will retire, but I think I will probably stop working full time about two years from now, and then may or may not try to work about 60% of full time for another few years. But I'm not sure of that, but I just want to say that it's going to occur one of these days, and there is an orderly transition going on. Work—responsibility has been shifted to another person during the last two to three years on a gradual fashion and that's going to occur again at the end of this year when I will cease to be head of the division and he will become head of the division. And I think that orderly transition is much more appropriate than a sudden transition.

Curry: Great.

Roche: So the final thing I want to say is to thank you, and I'm going to call you Donna, for conducting this interview in the first place and giving up your time, and secondly for conducting it very pleasantly and helpfully, and then of course we've got the lady behind the scenes here who's been pressing buttons and pointing the camera, and I want to thank her also.

Curry: Thank you.

(End of Interview)