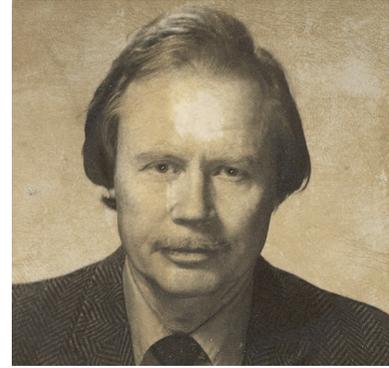


## Richard Q. Bell

- Born 2/10/1919 in Logan Utah; Died 12/29/2001
- A.B. in Psychology (1940) University of Missouri, M.A. in Psychology (1941) University of Missouri, Ph.D. in Clinical Psychology (1951) Stanford University



### Major Employment

- Professor, Departments of Psychology and Psychiatry, University of Virginia: 1975-retirement
- Chief, Child Research Branch, National Institute of Mental Health: 1967-1974
- Clinical Psychologist, Psychology Laboratory, Intramural Research Program, National Institute of Mental Health: 1953-1959

### Major Areas of Work

- Prevention

### SRCD Affiliation

- Governing Council Member (1973-77), Chair of Committee on Interdisciplinary Affairs (1974-77), *Child Development* Editorial Board, *Monographs of the SRCD* Editorial Board

### SRCD Oral History Interview

Richard Q. Bell

Interviewed by Myrna Shure  
In Philadelphia, Pennsylvania  
August 12, 1994

**Shure:** This is Myrna Shure interviewing Richard Q. Bell of the University of Virginia at Charlottesville as a figure in the history of the Society for Research in Child Development. This interview is for the archives of the society and we are doing this on August 12, 1994 in Philadelphia, Pennsylvania.

**Hi Dick!**

**Bell:** Hi Myrna!

**Shure:** Why don't you begin by telling us a little bit about your childhood, adolescence, anything that may be of interest to us, like your educational background, some occupational characteristics of your parents, just sort of how it all began, Dick.

**Bell:** Well I was born to a college teaching mother and a contractor, lumberman, and logger father in a small agricultural county in northern Utah. The interesting thing was that mother was a very intelligent mother, particularly indulgent of the idiosyncrasies of her children. So she not only tolerated them but played them up. This fed into one of my interests which was playing around with words, and it was an idiosyncrasy that I tried to pronounce words that were way beyond me and the family would always just laugh. Now dad was a demanding father, very demanding and also a rebel against the Mormon Church. So got that combination of physical demands and physical activity and so forth helping him work in the business and everything along with his strong opposition to the Mormon Church, and that set the background for us. Mother gave lessons in elocution, so to keep me quiet my other sister was assigned the job of keeping me from interrupting the elocution lessons. She did it by telling stories and she could tell one right after another. She turned out to be a playwright later on.

*Bell, R. by Shure, M.*

1

But I appreciated her world of fantasy because she could pick up from where I would tell her we were in the story and she would just start right from there, make up some more, and just keep on doing that.

**Shure: Oh, so she was making up the stories?**

Bell: She was making up stories as she went.

**Shure: Wow, so if your mother hadn't been an elocution teacher, she might not have been a playwright.**

Bell: That's right and I may not have been as interested in fantasy, because what she was showing me was the world of fantasy, how beautiful it was. We escaped where we were completely, we were gone.

**Shure: I wonder if that interest in fantasy had anything to do with how you developed into who you became.**

Bell: I think it did because I enjoyed it, and I can remember in high school writing themes for other students and having the teacher warn me not to do that because I enjoyed writing and had a lot of fun at it. The teachers liked my academic work. They were a little bothered by my rebelliousness. I seemed to be rebelling against the school as a reflection of the religion in the current society there.

**Shure: That may have been why you rebelled, on the other hand it's normal for anybody to rebel.**

Bell: That's right. I'm a little more that way.

**Shure: A little more rebellious.**

Bell: At a 50<sup>th</sup> reunion one of my colleagues came up to me and said, "Dick, I've just been talking to some other members in our class and we all agreed that you were more rebellious than I was."

**Shure: And you still turned out okay!**

Bell: Right. One thing that was very significant from that period was that my mother was teaching. She let me use her office at the University and I had a friend whose father was in the German department. We would get there and just have freewheeling bull sessions about academic stuff. We both took Latin courses together. We learned about the history of Rome and philosophy and the two of us just had great freewheeling sessions. He was a very flexible thinker. We used that office and in that I thought, what an enjoyable activity this academic world is compared to logging.

**Shure: Oh, compared to logging?**

Bell: Yes, logging and lumber work and contracting.

**Shure: Oh you mean grew up sawing.**

Bell: As a teenager putting in those eight hour days on cement mixers, and feeding a cement mixer, and what have you.

**Shure: It gave you character, right! Do you think any of these early childhood experiences or maybe early adult experiences had any important impact on how you developed intellectually?**

Bell: Yea, well they certainly did. I took a systems course under Arthur Melton, University of Missouri, and that had a lot of philosophy in it and I loved it. I did well in it so he got me in as a teaching assistant. He knew Jack Hilgard at Stanford and he recommended me for a fellowship there, which I got, and so in a sense Arthur Melton got me started in psychology, in a completely different area, he just let me go in any area I wanted. His area was learning, but he let me go in any area I wanted.

**Shure: So he was one of your early mentors.**

Bell: Very influential. Now the interesting thing was that I left in 1941 for the War, the Second World War, returned in 1947, and met with Hilgard. I just showed up in 1947 and I said, "I wonder if there is any possibility I can get that fellowship back that I had to give up when I went into the service." He said, "We will find something for you." The only thing he could find was clinical training. He got me a clinical traineeship six years after my leaving for the war.

**Shure: Isn't that something.**

Bell: Yes, that's Jack Hilgard.

**Shure: Well, I wonder if a number of people who ended up in child development actually did begin as clinicians.**

Bell: Yes, certainly, there are two or three of my colleagues. It was just a natural switch. It was in the clinic at Stanford I could see how the children changed. But also, as part of the training program where we worked with adult schizophrenics, could see how difficult it was to get any movement or progress. After a year with one of the patients, he was simply moved from an open ward to a closed ward.

**Shure: This was an adult?**

Bell: This was an adult patient, but on the other hand the supervisor said, "Oh, don't let that bother you. You have to get people agitated before you can get them better."

**Shure: Oh yea.**

Bell: But in contrast I was working in the child clinic and I saw how much the children grew and changed. If we did nothing to those children, a lot of them would get over their problems. From that I concluded it is better to prevent these things, and the best thing to do is start with kids when they are malleable.

**Shure: When did you first use the word prevention, Dick? Because only recently, maybe in the past five years has there been any headway with this concept. Yet 30 years ago there were some small groups of individuals who were trying to promote this idea, but nobody was listening. Were you one of those small cadres?**

Bell: I was in an even, or maybe a larger cadre in that it was in my head. I was thinking of it and was determining my development, my professional development, but I wasn't advocating it. I wasn't pushing, I wasn't writing about it. I was only thinking about it.

**Shure: Well do you think the experience with the schizophrenic might have brought that on, like wouldn't it be nice if this never happened?**

Bell: Yes, that's exactly what I thought, that this person I was working with did not have to be that sick. It could have been prevented at various points along the way. Let me mention some other

important research mentors. I've put quite a bit of emphasis on Melton and Jack Hilgard, but Quinn McNemar, the statistician, was also important early on.

**Shure: Oh really? I avoided him, well statistics, as much as I could!**

Bell: He terrorized everybody. I avoided him too but I found his lectures were throwing everybody.

**Shure: You actually were a student of his?**

Bell: I was a student of his.

**Shure: Oh, that would be different.**

Bell: Yes, so I just took that text of his and I just read it and I read it until I understood it. Didn't bother to go to his classes.

**Shure: Really!**

Bell: No, I came in and I got an A+ on the exam.

**Shure: Just think if you had gone to his classes.**

Bell: Quinn was suspicious when I came up with an A+ because he hadn't seen that much of me in the class. I think the reason was I hadn't been traumatized by his lectures.

**Shure: That's great, did you ever tell him that?**

Bell: No I didn't. I met him years later on a plane on the way to a meeting and it was good to see him again. I feel goodness in the man. It was the traumatizing part of it. Now he got me interested in statistics. I really got interested in it from there and that led me into this convergence approach, the idea of brief segments. Brief developmental segments linked together statistically. That was my first paper, first paper at a meeting. When I gave that at the New York meeting of the APA afterward I advocated accelerated longitudinal studies. You could do cross sectional studies and link them together. I mentioned that to John Anderson from University of Minnesota, you know *the* John Anderson, the older John Anderson.

**Shure: Oh, the older one, the elder. The elder John.**

Bell: He said, "Thorndike had this idea 50 years ago," and I said, "Really?" I couldn't find it anywhere. I went up to him after the meeting and I said could you give me a reference on that and he couldn't. So I wrote him letters when I got back to the University periodically asking him if he ever found that reference. Right in front of everybody he'd said that somebody had the idea 50 years ago. It really didn't matter too much whether they did or not. I was pushing the idea. He finally confided in me, that it must have been in correspondence. I found out recently from a statistician that Thorndike was on to a thing called Time Lag Sequential.

**Shure: Oh, that early?**

Bell: That was a different thing. That was what Thorndike was on and it is an ingenious method and it's now being used.

**Shure: Well Dick, we already have touched upon it but let's talk a little more in depth of how you got to where you are today. Maybe let's talk a little bit more about some of your primary interests at the very beginning of your career. We've already talked about how your interest in prevention**

first developed. It must have been more than an experience with one adult. How did you really get into this because at that time and even today it's still a very new and different way of thinking and there is still a small cadre of us even though it's getting more attention that it did before? It's still in its infancy and yet you were thinking about this 30 years ago. How did that evolve, Dick?

Bell: Well I think it evolved out of some more things at Stanford. Jack and Jean Block were there, fellow students of the same cohort, and I first got interested in theory because I was impressed with the intensity of their theory.

**Shure: Oh they developed their theory at Stanford while they were students?**

Bell: Oh yes, they were both working on that as graduate students.

**Shure: Oh I didn't know that.**

Bell: And I was impressed with the power of a conceptualization.

**Shure: Oh I am so enamored with that whole way of thinking. I use it in my research today.**

Bell: It's a very effective measurement tool, but they had a theory behind it.

**Shure: I didn't realize they started that as students. That's interesting.**

Bell: So that got me interested in the importance of theory. We had a clinical supervisor, Paul Reynolds, out there at the Veterans Administration Hospital in Palo Alto, and he added one other touch to it. During some kind of a little session with me he said, "Dick, you want to look for discrepancies?" where everybody is thinking along one line and you can't agree with it, there is something wrong with it. He said, "Don't discount your own idea; it may be a good one." This is why I'm going backward to get to an interest in prevention. Sure enough that opportunity came along years later when everybody in socialization was assuming that the parents had the major effect on the child. The child had practically no effect, you could forget about it, and I knew that with my own kids this didn't work. There were some real flaws in this. So I thought, hey this is one of those discrepancies. Something is wrong. Everybody in the field is going that other way so let's take a very careful look at it. Then about that same time I met Earl Schaefer and we worked together, in the offices next to each other, and Earl was carried away with the idea that we could prevent schizophrenia if we could locate schizophrenogenic mothers. Now that seems kind of implausible from what we know now, but at that time Earl had what looked like a lead. At that point I had some attitudes towards theory and attitudes toward prevention but I didn't have a research lead, a direction, and Earl did. We were going to produce an excellent attitude instrument and locate the schizophrenogenic mothers and work with them and help them and then they wouldn't all have schizophrenic children.

**Shure: Is that the beginning of the famous, is it Schaefer and Bell, the PARI? Is that the PARI?**

Bell: That right. That's how it got started.

**Shure: Oh, my.**

Bell: So Earl had the direction there, I just helped him, just worked with him. An early finding that came out of that tipped me off, but it didn't impress Earl.

**Shure: Before you go on, for anyone who doesn't know what PARI means, tell us about it.**

Bell: Ah, yes. It was the Parental Attitude Research Instrument, developed in the late '50s. There were over a hundred studies done with PARI within a few years. Most data results were negative.

People were not getting relationships. It was not copyrighted; we made it available for anybody to use it as much as they wanted. We just asked them to send us the results. A key result was reported to us by Lou Klebanof. He could not find any difference in attitudes between mothers of schizophrenics and mothers of severely retarded children, though they could differentiate both from mothers of normal children. That finally clicked in to me. My interpretation was that the mother is reacting to a general limitation of coping ability of the child. It is not that the children were producing the severe retardation or schizophrenic, which they certainly were not, but the mothers were reacting to a child who wasn't a good copier. So I wrote a paper on that, on coping. That further gave me impulse and direction to move on in child effects, and by 1966 or '67 Bettye Caldwell had a conference up in Syracuse where she had most of the big names in socialization, people who had been very active. Robert Sears was there, Al Baldwin was there.

**Shure: Oh, what year?**

Bell: 1965, '66 somewhere along in there.

**Shure: Because Baldwin was my mentor, just thought I'd mention that! Al Baldwin and Claire Baldwin at Cornell. Well they were gone from Cornell by then, but they were my mentors at Cornell. They were very heavily responsible for my sitting here and interviewing you, Dick. They were very, very important in my development.**

Bell: Now Bettye Caldwell called this conference and I wondered about giving this paper, but I finally decided, oh the heck with it, that's all I can, as I didn't have anything else to give. So I reported on my reinterpretation of the studies on socialization, which were interpreted as indicating the effects of parents, how I reinterpreted them as the effects of children on the parents instead. Everybody was saying it could be the way that it's most reasonable to think, an effect of the parent. I said, "Alright, let's see if we can provide a reasonable explanation of it as due to the child in the case of a particular finding." I expected them all to land on me like a ton of bricks because it had been their career. That had been Al Baldwin's career out at Yellow Springs in Antioch College and certainly Sears and all of his colleagues. I heard comments like this: "It's time somebody said this." Al Baldwin said, "Well, Dick, you are complicating things immensely." That is our complication.

**Shure: Well it's good to start some controversy. That's what gets thinking going.**

Bell: That's right, now there is one more little element that contributed to this main stream. I had carried out a lot of research on congenital characteristics, neonatal characteristics hoping we could get at the child characteristic coming from that direction. We got very few relationships because the child's care is an unfolding thing. It isn't there in a book open in the neonatal period. I also realized that it wasn't necessary at all to advocate a child effect because, if parents are effective, the child is going to change and they will be affected by the products of their own tutelage. So then the methods of social psychology begin to look interesting. How do we manipulate the role and the functioning of this child and adult? Relatively recently Daphne Bugenthal at the University of California in Santa Barbara has used the methods of social psychology to study the effects of children on parents. So it is no longer tied in any necessary way with demonstrations of congenital characteristics.

**Shure: Well we have developments still leading up to what you are doing today. Maybe we should start with what your most recent contributions have been and then we can talk about how your thinking got you there.**

Bell: Well I've actually wanted to mention at this point how our mutual interest in prevention came up because that was a very influential factor in my thinking, like the effects of these other experiences with Earl Schaefer. I was going to mention that I gave a speech to the Community and Developmental Program in 1982 at APA and Myrna Shure was in the audience. What struck me is, I was talking about methods in a general sense, about theory and prevention, and Myrna Shure was already operating

intervention programs on a universal basis. Suddenly I realized that Myrna Shure could make up for what I lacked in knowledge of how you actually go about these things.

**Shure:** Dick, I think you are missing a few steps here.

Bell: Yes, well maybe so, but you turn up. So all of a sudden I began listening to your reports on your research and I began to see the idea, realize it was feasible to talk about universal interventions, and that you were doing it.

**Shure:** Yea, but I think, you know, we might want to explain what sparked that discussion. Here you were standing up at the microphone talking to about 300 people. You were an invited speaker, the authority in the field of prevention. You were talking about targeted interventions, about working with individuals, and tailored interventions to their needs. This little tyke comes up afterwards thinking she would get about ten seconds with you at the most and said, “Dr. Bell there is another way to look at intervention. What about working with whole classrooms and preventing possible dysfunction and you might even miss some false negatives,” and I thought this speaker would say, “Oh yes, that’s very interesting, thank you very much,” and go on to the next person. I was just so impressed, Dick, that you didn’t know who I was from Adam at that point and you talked with me for at least a half an hour and we really stretched our thinking, both of us, because you had a different way of thinking about it than I did and I had a different way then you did and that lead to quite a relationship from there.

Bell: Up until that time I thought that a universal intervention or mass-targeting was an interesting thing to think about logically but most unlikely in psychological studies. I found out that was wrong.

**Shure:** Well I am just honored that I could have influenced you at all, a little bit. So that was another early experience, although it wasn’t early. It was probably 30 years after you got going, but it was still in the days when the field of prevention wasn’t being paid much attention to. It was beginning to get paid attention by community psychologists but not so much yet by developmental psychologists. So where did you go from there?

Bell: Well let’s see. First of all I began to think in terms of the universal mass-targeted interventions because our false positive rate is so high in the high-risk groups. So that was one reason I was moving in that direction. I was concerned about the false negatives too. By doing, let’s say a whole classroom of kids and not just those already specifically identified as high-risk you might prevent a year or two years later. At least initially in the research high-risk behaviors developing in kids we thought were okay but really weren’t okay.

The other thing is that we were attempting prediction over long time spans such as in schizophrenia, affective disorders and antisocial behavior, and that I thought different. Here’s my developmental point of view coming in. It is most unlikely we will be able to trace continuities across major stages and so forth. We might be able to do it within a stage and that is the best thing we can hope for.

Well you know one of the things that distinguishes your research from many others is that you don’t just look at risk in one age group and say, “Okay let’s do an intervention and try to reduce or prevent that risk from getting any worse.” You really talk about different risks at different ages. That’s a fascinating concept. Most of us really work with specific age groups in mass and then go on to another age group. You do some with the same precursors for the other disorders.

**Shure:** Yes right, so why don’t you just very briefly tell us a little bit about your research and looking at different markers at different ages.

Bell: What I have done really is go over the literature on schizophrenia and affective disorders to see what kinds of precursors were being identified. It turned out that in the case of schizophrenia, despite

a lot of effort, a lot of institutions were involved, and the best you can say is that the precursors for schizophrenia don't look an awful lot different from the precursors for a large number of adult experiences. So that really what they were was academic and interpersonal incompetence, difficulties in academics and difficulties in interpersonal relationships which are just garden variety problems of that period. So I came to the conclusion that there was no use trying to find those specific predictors for schizophrenia at that age. We could talk about whatever the disorders were of that age, whatever the problems were of that age, but they would have more bearing on such things as learning disability, hyperactivity, disorders of parenting, and less antisocial behavior and so forth and less to do with the long term consequences. So, in other words, we had to go stage by stage and give up the effort to identify long term specific precursors of specific disorders. In fact our interest should be in problems between time one and time two, not specific problems, because children change so much and so many environmental changes can occur that they are very hard to track.

**Shure: Do you have any particular published papers or unpublished manuscripts now that might best represent your thinking about child development?**

Bell: Yes, I had one in *Psychiatry* and it's 1992, "Multiple risks cohorts and segmenting risk as solutions to the problem of false positives and risk to the major psychoses." What it does is lay out the argument for looking at problems between periods. Precursors of problems rather than precursors of specific disorders and shortening our goals as far as time management are concerned.

**Shure: I think this paper really summarizes your thinking and your unique contributions to the field of prevention.**

Bell: That's good to hear.

**Shure: Dick, was there any political and social events that particularly influenced your research in writing and your teaching?**

Bell: There probably were but I am not aware of them. I wasn't working on some political or social agenda, basically.

**Shure: You know you've certainly been involved in research funding over the years. Do you have any particular thoughts about experiences with research funding or your participation in shaping funding for research? Either for your own work or for others.**

Bell: I was an early advocate, going back into the '70s, of collaborative studies in order to build up larger Ns, except for one search at the cutting edge of a field. We need much larger Ns and collaborative studies to achieve these Ns because of the complexity of the phenomenon we are dealing with. The more facets there are to the disorder the more you are going to have to expand your Ns in order to have some cases in each sub-cell.

**Shure: That's an issue in sub group analyses. You really have to have a big N.**

Bell: Now I wrote a paper with Tom Hertz in 1976 in *Child Development* in which we advocated much more use of the collaborative methods at that time, because we traced out some research areas in which we'd had 40 or 50 years of research and no generalizable findings. Even at that time these occurred in several settings and everybody would be pretty agreeable as likely to be stable findings. Now this proposal finds itself furthered by a maze of funding mechanisms to support collaborative research. The McArthur Foundation is sponsoring an awful lot of networks for collaboration and Weiss has challenged the paper with a provocative thought, but I think the challenge primarily concerns the cutting edge of the research not the major direction of research. A new finding has arisen which meets one of the important objectives of this advocacy of collaborative studies, meta analyses. Investigators are achieving a larger N by putting studies together that have already been done.

**Shure:** Yes that seems to be a way to really tell us about a field. It's exciting I see more and more of those in the literature.

Bell: Yes and reporting different results from the meta analysis than you get from summarizing the results of your reviews of the individual studies. Now there is another thing too, I think, now this goes to the funding method, it seems to me that--

**Shure:** I can tell by the sound of your voice that you are not totally enamored with it!

Bell: Ah, that's right, that's right. Well I like the idea of meta analysis actually.

**Shure:** No, I mean the funding mechanism.

Bell: Oh yes, the funding mechanism. Well now the funding mechanism is pretty good. all things considered. I think there is one little element that is very damaging, very bad. I think that at the time any investigator's work is being reviewed he or she should be on standby and available by phone so that if the review committee has two or three key questions they could ask the research administrators to get on the phone and ask this person to respond and give their response, because an awful lot of grants are dropped for reasons that the investigator himself could easily refute. They were not dropped on good grounds. Maybe they need to be dropped anyway, but they weren't dropped on good grounds. Give investigators a chance at the time of the review to hear, "Look it is going to be reviewed Saturday morning or Friday morning of such and such a date. If you stand by the phone and we have any questions we will call you and relay your response back to the committee."

**Shure:** That's a great idea because sometimes nobody knows a subject matter better than applicants. To say that they didn't express it clearly isn't always accurate. They just are talking about something that the reviewers may not be as familiar with and I think that's a great idea, and not expensive either.

You know we have been talking a lot about your research, Dick, what do you see as the particular strengths and weaknesses? We have already talked about the unique contributions you've made, but is there anything else you want to say about that or where, you know, where you were headed wrong? Any places where you have revised your thinking, other than what we have already talked about?

Bell: Yes, in the case of the convergence approach linking short segments together statistically. I thought that if I had to do it over again I would keep in contact with a lot of the investigators in the field of psychometrics to see if I could find somebody who was capable enough to come up with some new techniques. What it needed was methods of analysis to show that something could be done in the convergence approach. You know, I advocated that in '53 and here it is in this era just getting rolling, and it's getting rolling simply because one investigator who is a very competent and very creative statistician, Jack McArdle, has seen in it a great opportunity both in meta analysis and in organizing data collection. So he's developed some methods that fit it into a general pattern of analyzing latent growth curves from incomplete data. That is the way it would look to the statistician. So now only forty years later--

**Shure:** Are you going to explain that in English?

Bell: What you do is you look at these little segments as though they sampled a latent growth change over time and then you try to put them together to see whether they do represent this curve, and it is latent because you don't get at it until you put them together to see how they run together. That is complicated.

**Shure: Sounds sophisticated.**

Bell: Now 40 years later, after I reported on that thing, Jack McArdle has several methods developed for using the convergence approach and it's in an article by McArdle with his colleagues in 1991 in Collins and Horn. So I wish I had pushed that harder earlier.

Child effects is another area where there are some strengths and weaknesses. The 1968 article I put a lot of time in. I must have gone through six to eight redrafts of that article because it was controversial. I knew it was going to be a real problem, that there was going to be a lot of arguing on it, so I had everything really nailed down. Then I got so many ideas after that article that I got together with Larry Harper, a student of Frank Beech, and we decided to put together a comparative and human volume: mammals other than man and man on the effects of the offspring and the young on adults, broadening the thing. I thought we did a great job and Larry wrote several chapters on how this works out in the mammals, most of the mammals. I wrote a very good chapter on methods and we carried it much further than the methods in that '68 article, but everybody cites the '68 article and nobody reads the 1977 book which we had these developed so much further.

**Shure: Well you can quote it, you can reference it!**

Bell: Any time I get a chance I do! Then we had some good methods in theory. We carried theory further and we summarized all the current research showing that, indeed, a lot of the earlier findings were coming from the direction of children, not initially coming from the parent. And also I have a reciprocal theory of interaction in an article in 1986 with Michael Chapman.

**Shure: You know that's fascinating, Dick; that means your very early intuitions 30 years earlier held up in your writing about the impact of the child and parent.**

Bell: And fortunately it was that mixture of intuitions plus statistical logic. In other words, I just thought, this can't be right, let's see if we can do it the other way just testing it. In fact I think there was a bit of rebelliousness in this, saying everybody thinks about this in the wrong way.

**Shure: Well that's because you grew up rebellious, this all fits together, it is really interesting!**

Bell: That's right it does!

**Shure: If your father hadn't rebelled against the Mormon Church you might not have written those articles.**

Bell: There's a good chance. So anyway that's all summarized in a paper with Michael Chapman in 1986. We do have a reciprocal theory of parent child interaction and it contains, it shows that you can get the same finding across a lot of different studies, studies done in different ways.

**Shure: Well, Dick, before we go to talking about your institutional contributions, are there any final thoughts you want to inform us about your present thinking?**

Bell: Yes, I think.

**Shure: You had to think about your thinking there for a minute.**

Bell: Yes. Well what's currently on my mind is how we can organize these multi-risk cohorts and how we will go about it, I'm sure we can identify them and I think we can compose them. The question is how will this relate to intervention? I can see if you give the community a task and the support to identify all children in, say, the stages of early infancy, the preschool period and so forth, if we define these segments that the community can identify the children that are having problems in these

segments, then we can test our ability to identify problems by how many didn't develop problems from one of kind or another by the end of infancy. But then how do we go about intervention, because it is different interventions for different children in the same cohort. This is what we are talking about right now because I can't really see that link, logically I can't see it. But of course one way of looking at it is that you can use general universal interventions, you will cover a lot of children and a lot of different problems.

**Shure: As long as the intervention doesn't hurt, as you have always said in your writings, and that I have quoted many times, as long as they don't hurt anybody, including the true positives.**

Bell: As a matter of fact I thought the most interesting thing I ran into in your own research was that you not only were doing universal interventions, but you had identified the risk groups so that we could tell what the effect of the universal interventions was on the risk groups. You had some evidence down in the preschool period that some children had been adversely effected, but it was pretty easy to see how and we could see the necessity from your study of not only doing the universal intervention, but also finding out what the effect of it is on those risk groups and whether it is temporary or permanent. I found, you know, just very briefly that sometimes when you talk a lot about feelings and your own feelings and getting into other people's feelings that for children who never experienced that opportunity before it was temporarily even a little upsetting. But by continuing to express those thoughts once they could identify them and put a language label on them that upset dissipated.

Okay so we were talking about the temporary, potential temporary setbacks that kids might experience during any interventions. I was just saying that in my own case children who never had the ability to even identify particular ways people feel, and not only their own feelings but other people's feelings, you know, it may upset them at first but then they get comfortable with it by continuing to talk about until it ends up doing them a lot of good.

**Shure: But what I wanted to say, Dick, it was that this issue of mass-targeted verses tailored interventions that got us together, this issue. I'm beginning to realize now that probably maximum impact of otherwise benign interventions might particularly involve high-risk kids that are really more extreme than others. These kids might benefit from individual help from school support service personnel like counselors, school psychologists, social workers who work with children individually anyway. If they could be trained in the same interventions going on in the classroom to keep the communication style consistent that would probably serve maximum benefit for the child. What do you think of that thought?**

Bell: Yea, I agree.

**Shure: Dick, let's go on to some of your professional experiences. Tell where you have worked in the past, when, and it what capacity.**

Bell: Well, from graduate work I went to the National Institute of Mental Health in their in-house research program and was there from 1951 to 1975. I saw it progress, move, and change from a period of strong support for the behavioral sciences, strong interest in developmental psychology and psychiatry and even psychoanalysis. The in-house program was very psychoanalytically oriented when I arrived there. Saw it move gradually away from the behavioral side because of a lot of successes on the biological side. Success in identifying neuromediators and so forth and so many promising findings, and the research on genetics of schizophrenia looked so promising at that time that the NIMH moved away from the strong support for the behavioral sciences in the direction of the biological during that period, but it is primarily the result of more interesting findings coming in, we just have to admit it, we weren't producing things then. I think that since that time the research on attachment and prevention have moved considerably and we're making big contributions in that area now.

**Shure:** So you've seen a tremendous change even within institutions such as NIMH and what they're supporting. Well they have prevention research centers now and, you know, that's only ten years old now, maybe a little more, but that makes a great statement about what their priorities have become or if not a priority at least equal status with other research, so that's an important statement I think. Dick, don't you agree that child development specialists are now into the field of prevention in ways that they never were before?

**Bell:** Oh yes, no question it's happened finally! I've always been identified with developmental psychology but community psychology first saw the relevance of school and classroom interventions and so my big dream after I became acquainted with community psychologists was that the cocktail parties of the American Psychological Association should combine developmental and community psychologists. They were always separate. I was always running back and forth between the two and I remember the first time it ever happened, the first developmental psychologist that ever came to a community psychology cocktail hour was Sandy Scarr and I was so thrilled. As you know she has done a lot for community psychology in her career. I mean she has carried out interventions and so have you. It's just very exciting that you've, in a sense, addressed the division of community psychology now and so you're a nice combination of the two and hopefully over the years there will be more and more because there is so much to learn from each other, community and developmental psychologists.

**Shure:** Dick, as a teacher of child developmental research or as a trainer of research workers what courses have you taught and do you see any tension between teaching and research in the field of child development?

**Bell:** Yes, I've taught courses in theories and observation of development, parent/child interaction and prevention and I've had the usual experience of people who have been in both research institutes as well as in academia. Having these courses lead directly to research, students respond to things in the lectures and suddenly you get an idea and you get caught up in it and so forth. And this happened in one of the courses I was teaching on parent/child interaction. One of the students, Dan Shaw, said, "You know, they seem to be talking about attachment in one way in the early development literature and in another way in the antisocial literature," and away we went. Suddenly we realized that these were different usages of the term, but attachment may play a role in early social development, and thus a big project in Pittsburgh was launched out of that class in parent/child interaction and I've stayed with and consulted with that project and it emerged directly out of course content. It worked, I should mention, in spite of some of the pressures there are to get moving very rapidly in academia so the student can get the dissertation or thesis done. But also as a result of that course we started meeting about weekly or maybe every two weeks and we had jam sessions until we had worked it through pretty well in our minds and once it looked like a good thing we were ready to roll. So that's the usual interaction that you get out of course content. I should mention that I shifted to academia late in my career. I've only trained a small number of graduate students. Barbara Keller, who consults at NIMH; Michael Chapman; let's see, Dan Shaw at Pittsburgh; Linda Stanhope at Union College; and so forth.

**Shure:** I bet they'd all be thrilled to hear that they made a marked impact on your life too.

**Bell:** They certainly did. Barbara Keller did one of the first studies in which she altered child behavior experimentally and showed a big effect on adults interacting with them. She did one of the first studies in that area. Dan Shaw is carrying on the study that we got launched on the effect of attachment on early antisocial behavior, and Michael Chapman worked with me on kind of a general survey of the field.

**Shure:** Have you had experiences, Dick, in so called applied child development research as it relates to some role you might have played in putting theory into practice?

Bell: Yes, I see that as putting theory to work. I have no experience in applied research but I have worked closely with those who have. Myrna Shure is actually carrying out prevention research.

**Shure: He's not just saying because I'm sitting here!**

Bell: But as we mentioned before it was a considerable factor in developing my viewpoint on multi-risk cohorts in developmental segments.

**Shure: Well of course it is very difficult to put theory into practice and I know in the early days the methodology was not as squeaky clean as developmental psychologists would like it to have been. You can't control for everything out there in the field that you can in neat little correlational studies. One of the things that I think you've helped developmental psychologists see, Dick, is that you can still learn something even if it isn't perfect. I think that's really opened the way for developmental psychologists to feel free in experimenting and learning and improving and I think because of that the methodology for interventions and analyses has really become significantly more sophisticated today. Now developmental psychologists are really trying to transfer theory into practice.**

**What about your experiences with SRCD, when did you join and what were some of your earliest contacts with the society, who did you know?**

Bell: I think I joined in the mid '50s and should mention that one of the first meetings I attended was at Airlie House in Virginia, not too far from Washington, D.C. At this first meeting there were no parallel sessions and not only that, I played chess with a friend during some of the sessions.

**Shure: During the session?**

Bell: Yes.

**Shure: How many people were in the audience?**

Bell: It was a very small meeting, very small. Very easy to know absolutely everybody else in SRCD.

**Shure: Actually SRCD was like that for many years.**

Bell: Sure was.

**Shure: You know I remember the Presidential Banquets and you know you really got to meet people there and I miss that.**

Bell: We all miss the small group approach. Of course that has some relevance to important change. But I should mention one other thing; I was on the Governing Council until sometime in the late '70s, on various committees. On one of which we launched the idea of an SRCD liaison office in Washington because of the need to present SRCD views to Congress and agencies, and to be informed ourselves.

**Shure: When was that?**

Bell: Some other later committee finally put it into effect.

**Shure: And when was that, Dick, that you were on that committee?**

Bell: I think that was the late '70s.

**Shure: Oh so that's been quite a while that we've had this liaison office in Washington then.**

Bell: I think mine was an Interdisciplinary Committee and I launched the idea there. The next committee picked it up and put it into operation and added the idea of the congressional fellows. So all of a sudden we had a very effective program there with a liaison office in operation and the congressional fellows as well.

**Shure: So you really played a very major role in some of the great breakthroughs of the mission of SRCD.**

Bell: Well I was interested in seeing that happen. I know it has been beneficial and it was just nice to see it finally into operation. I was surprised to find out we had a liaison officer, having thought of it earlier. We have a liaison officer up there now you know.

**Shure: It was there but nobody knew how.**

Bell: That's right.

**Shure: Well were there any major problems or issues that confronted you during your time on the governing board?**

Bell: No, just the problem of trying to make better public contact.

**Shure: Did you play any other roles in SRCD? I know you have presented many times.**

Bell: No, I think just the usual presentations.

**Shure: Now of course there have been a lot of changes in SRCD over the years, one of which we already eluded to. It certainly got a lot bigger. It also got more interested in social policy and including social policy as a critical part of the makeup of SRCD. Any other changes in SRCD and which ones do you like? Any that you don't like?**

Bell: Well the social policy interest I would like to see tempered with a concern for how stable our findings are in some areas where we are being advocates. It seems to me that frequently what gets expressed as social policy that should come out of our research doesn't actually come of our research. It comes out of individuals who have an idea that they've got the field behind them, and very often they don't. I'd like to see our social policy based on well agreed upon findings that most people in the field would accept. That's the only kind I think we should be communicating to the public and the governments.

**Shure: So you are saying there should be more data before we start servicing?**

Bell: Yes, that's right. Now in addition to the fact that we have had rapid growth, I know there are always smaller groups forming to offset the disappointment with parallel sessions and everything. The size has an advantage primarily in the readership. *Child Development* and *Monographs* have tremendous readerships so I like SRCD for that publication impact.

**Shure: Yes.**

Bell: And the ability to reach all the developmental psychologists is just great. I think we offset the disadvantages of the rapid growth by just forming new societies. They've launched the Society on Adolescence now and that's the way it will always be and we will just have to make our proliferation easy so that it reaches the interest groups. So then we have small groups forming now that are about the size of SRCD when it started.

**Shure:** But even so walking around SRCD it's amazing with the fact that it is so much bigger but you still see a lot of people that you know. It is still a very intimate feeling. To me there is nothing like the Society for Research in Child Development. It is just so much fun to go there and see the people you studied with as a student and who are now your colleagues. It's still the most exciting conference of them all, in my opinion, and you played a great role in its development. It's really exciting to sit here and chat with you about it.

**Do you have any hopes or fears for the future of the field? Either prevention or child development in general?**

**Bell:** I think that the idea of development, the importance of development is so logically overwhelming that we can count on a continued increased interest from the younger investigators and graduate students. But I think we have to deliver in three key areas in order to retain leadership and not just size. The first is in getting something going, more research on prevention, multiple pathways to and between disorders. Everybody's thinking of a pathway to a disorder. Let's think of the many pathways to disorders. Another concern is with how to intervene without impairing a child's coping. I think the problem here is anchored in another problem being studied in the field of socialization which is that if you reward children and respond to them as we used to think in very good ways, they can lose the feeling of effectiveness. It is very important for them to feel that they're doing something well and the question is how can we launch interventions that will accommodate them and also realize that we have had quite a few studies reporting negative effects of interventions. I've summarized them in that 1992 article in *Psychiatry*. Another related but earlier article that appeared in 1982 was age specific manifestations in early development. I think that there have been some adverse effects of intervention and we can't just assume that anything we can think of should be all right, it couldn't possibly hurt them. We now know this is not true and it's very important that we identify the risk groups and see how they are being affected by the intervention.

Now another area on which we need to spend quite a bit of time is in basic theory development to guide and monitor this tremendous overcapacity to carry out empirical research. A good example of this is Thelen and others' use of the dynamic systems approach. I think we are ready to move beyond simple hierarchical models and beyond learning theory. We now know how far it can get us. Social learning theory is at its limits. It has encountered those limits precisely in that research on motivational change. If children lose intrinsic motivation they will only do things while you are around. They won't do them when you are away. And so that's a good example of how far that's going to get us. The theory is going to have to be shaken down and squared off with the theory of motivation. We have to tie the learning theory into motivational orientation.

**Shure:** Well that's a good topic for student dissertations.

**Bell:** Yes right there. Now let's see if there is anything else. I think those are the three areas that I'm most concerned about.

**Shure:** Well, you know, I know you have influenced a lot of students over the years and you certainly have influenced me in my thinking and, you know, it has just been very exciting to sit here and talk to you about your professional thoughts. Before we finish I would like to know a little bit about you personally. Tell us a little bit about some of your personal interests and your family and what you like to do when you are not doing psychology.

**Bell:** Well as you know being a tennis player, I'm an enthusiast in tennis.

**Shure:** And you are very good I can vouch for that. You beat me!

**Bell:** It was not easy! That makes me think that I think we need more balance in the tradition of the ancient Greeks in our lives. I see graduate students who don't do anything except study developmental

psychology. Developmental psychology is good and it seems to me they ought to have some ways of having some fun. I can see that the department is functioning well when we have a few people around who are organizing softball games and picnics, canoe trips and things like that. Tennis was my fun. I can work hard, very hard, as long as I can play tennis. I'm sure you have the same experience.

**Shure:** Well you know somebody once said everything in moderation and if you study too hard and don't balance it, you could probably be more productive if you had some balance in something totally unrelated. But you know I'm not sure tennis is so unrelated. It certainly builds self-esteem and makes you feel good about yourself, that is, if you don't lose too badly. But any endeavor outside of work is very important, but I can vouch for your tennis skills, Dick. I had the pleasure of playing with you and I'm looking forward to playing with you again. Since we are in Philadelphia we are going to go out and play.

Bell: When we are in Philadelphia we play on grass courts which is most unique.

**Shure:** And took you all of ten minutes to adjust. And your wife too. Good tennis player.

Bell: I should mention on a personal note the difference between children in my own family gave me the ideas on child effects. A lot of people say there should be a law against psychologists studying their own children, although Piaget did pretty well with that. The thing of it is you know them in detail and I think it is those experiences which you know from personal depth that are most productive. I could just see these kids were affecting us; we were operating very differently to this child, and this child and this child. We were retaining the major direction of effects however, but we had to go about it differently with different children. And my own children have taken very different directions and when I saw the contrast with this theory that was entirely committed to the effects of parents on the child I knew that there was something to help them.

**Shure:** That's a beautiful composition because that's how we started this interview, talking about the impact of the child and your own experiences with it and I think that's a great note to end on. Again I think anyone listening to this interview will have benefited greatly by getting into Richard Q. Bell's head and how it all came about and how your wonderful contributions have evolved. I really thank you for this interview.

Bell: I want to thank you for an excellent interview. It was an enjoyable experience.

**Shure:** I was honored that you asked me to do this and I learned a lot today. I'm sure anyone listening did. Again this is Myrna Shure interviewing Richard Q. Bell for the archives of the Society for Research in Child Development on August 12, 1994 in Philadelphia, Pennsylvania.