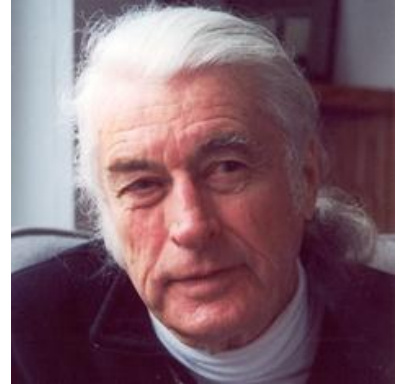


Gerald Patterson

- Born 07/24/1926 in Lisbon, ND
- Ph.D. from the University of Minnesota (1956); M.A and B.S from the University of Oregon (1951 and 1950)

Major Employment:

- Wilder Clinic - 1953-1955, Fellow in Psychology
- University of Oregon - 1957-1978, Psychology and School of Education
- Oregon Social Learning Center (OSLC) - 1977-present, Founder and Social Research Scientist



Major Areas of Work:

- Behavior Modification, Social Learning Theory, Social Interaction, Antisocial Behavior, Delinquency

SRCD Affiliation:

- Member

SRCD Oral History Interview

Gerald Patterson

Interviewed by: Richard A. Littman
At the University of Oregon
June 15, 2000

Littman: This is Richard Littman at the University of Oregon on June 15, 2000, and I'm interviewing Gerald Patterson for the SRCD Oral History Project on his own background. We have with us an outline of the interview to be covered, and we're going to begin right now. Gerald Patterson is at the Oregon Social Learning Center and has been there for many years, and I think the particular data on that will probably come out during the course of the interview. The interview schedule that we have is entitled, 'General Intellectual History,' and in it the first question asks Gerald Patterson to describe his family background along with any childhood and adolescent experiences that may be of interest. Include the educational and occupational characteristics of your parents, where were you born/grew-up, what was your schooling like, any military experience, early work experience. Patterson has a copy of the schedule before him, so he will be working from that and using it as a reminder. And the interviewer, Richard Littman, may from time to time interject comments because he has known Patterson for many, many years and, in fact, had worked with him in the past so that some of the things that Patterson will relate may possibly be amplified or commented on by Richard Littman. Okay, Jerry, you're on.

Patterson: Let's see, I was born in Lisbon, North Dakota in 1926. My family, during the Depression, moved to northern Minnesota living in a wilderness setting with an extended section of my mother's family. The schools that we had in Ely, Minnesota at that time were exceptional; they were probably some of the best public schools in the country mainly because of taxes being paid by the mines, which made it possible to build excellent facilities, both in terms of the quality of the teachers and the physical surroundings and equipment.

None of my family had ever been to college. I think one aunt went to college for one year, got married, and that was the end of our educational history in the family. I had no plans really to go to college myself. I was being trained as a young adolescent to be a professional outdoors person, a canoe guide and that kind of thing, because it was very appropriate to that setting. That's what my family did, and they're still doing it; they're still living in the same collection of small lakes ten miles from the nearest town.

Littman: How many people are you talking about Jerry?

Patterson: There are about 30 people living outside of town in this wilderness.

Littman: Did they form a sort of community out there?

Patterson: Yes, and I think that's a very important contribution to my current work because the nature of their community was to stress cooperation instead of competition. The males were extremely competitive with each other in terms of who's the best at this and that, but when it came right down to it, all of the males and all of the families cooperated with each other. For example, in the fall all the men would get together and we would "make wood" for everybody, including the widows and the grandmothers.

Littman: Is that still true?

Patterson: I don't think so now; I think they're burning propane. We used to put in our own ice too; we had a community icehouse. We'd make ice for everybody, and they'd come down in the summer to get their ice.

But anyways, the basic emphasis was on being responsible and being a cooperative member of the group. Some of the first work experiences I had while I was still in high school at ages 15, 16 and so on, was in working with groups of children, at first, and eventually with groups of adults as well. We'd take them out on a five-day or two-week canoe trip into the wilderness; it was very much a group experience. So I've always been interested in group process without really knowing at that time any of the formal characteristics of group process, but just being intuitively interested in that part of it. I think the reason for bringing that up here is that early experience, I think, is reflected in how we put the Oregon Social Learning Center (OSL) together and, in part, the way it still is.

Littman: It's a cooperative venture.

Patterson: Yes, that's right. In the research setting we avoided building a zero sum game for young Ph.D.'s. You can see in the publication record that most of us have collaborated with almost every other person in the Center at one time or another, and that's encouraged because we're not competing with each other; we're competing with everybody else outside the Center (just to survive).

Littman: I think we may come back to that a little bit later in somewhat more detail, but I think that provides some background to get some sense of where you're heading, Jerry.

Patterson: There's one other thing on this first item since it says any military experience. When I was still an adolescent, they trained me and sent me to the South Pacific, shot me up and rehabilitated me by sending me to school. If it were not for this, we would not be talking. So the military experience played a crucial role in deflecting me from my career as one hell of a good canoe guide.

Littman: Well, as I recall, you had a very traumatic experience also that you told me about many years ago when you were in the Army. Were you forced to shoot, or thought you were going to have to shoot up some innocent people? Do you remember that?

Patterson: I shot a lot of people; and on some occasions it was hard to know whether they were civilians or Japanese troops. There was no way of telling because people--combat is just total chaos, and I mean, there's smoke and people running, screaming.

Littman: Now at the time I spoke with you about this, and it was a good number of years ago, I had the impression that it had a really very powerful impact on you.

Patterson: Oh, yes.

Littman: And why didn't you mention that just now when you spoke about your military experience as something good? You know, you went into the Army and they reeducated you and made you something.

Patterson: You're absolutely right; I did leave that out. It's still like a little neurotic island floating around inside that I'm going to have to deal with. I may still go and get therapy for it.

Littman: Alright.

Patterson: I'm still really angry about that. See how the voice has changed?

Littman: Oh, yes.

Patterson: And I'm writing a series of essays on war and the military and so on. I have a whole shelf of books devoted to that. So when I'm done with science, I'm going to start on that. I have the rough outlines done, but--

Littman: Well, that may come back again a little later on in some other things you have to say. I just want to remind you of some--

Patterson: Yes. Well, it may be no accident that I work on aggression, you know; it's been my life.

Littman: Right. Exactly, that's where my question was going.

Patterson: Yes. Although the nature of military violence and aggression is different--I think it requires a different model, but I don't want to get off on that subject.

Littman: It's probably socialized, certainly.

Patterson: That's right.

Littman: Okay. The second question under general intellectual history is what early adult experiences were important to your intellectual development? And it speaks specifically to collegiate experiences.

Patterson: Well, I think when I went to college I was taking this strong general science background because that's what I had in high school.

Littman: Where was this?

Patterson: I roamed around a lot. I was looking for something; I didn't know what it was, so I started at a small school in northern Wisconsin and took courses from the two or three best teachers in things that were way beyond the freshman level, but they would allow me to do that.

Littman: What was the name of the school?

Patterson: Northland College in Ashland, Wisconsin. I was allowed to sit in on senior level classes in political science and history and had a marvelous intellectual experience. However, when I'd taken these courses there didn't seem to be much else to explore. I got bored, and I went to another school in southern Minnesota, Gustaus Adolphus College; it was really a fine school. Again, I was just taking courses almost randomly, but coming to the decision that I'd like to be an art major. I was doing a lot of sculpting and drawing and taking courses in eastern philosophy, language and so on, and more science. At that point, I was told I had a genetic defect and that I was going blind in two years. It was obvious I couldn't be an artist. They sent me to a psychologist at a local Veterans clinic. He said, "Well, if you're going to go blind you should be like me, a psychologist. You won't have to see; you can listen to people, and then say wise things and be a therapist." So I started training as a psychologist; I totally gave up art.

Littman: Now you're not the only member of your family who suffered this particular disorder?

Patterson: No, half of my children have it too and my mother also, although they didn't know in my mother's case what it was. It was just that she had terrible vision and recurring corneal ulcers that were extremely painful.

Littman: You have a brother also who--?

Patterson: No, none of my siblings have it. I was the lucky one, but the odds are one in two. So anyway, it was a very direct shove into psychology. I enjoyed the courses, but was still very restless. I left Gustaus Adolphus College when I heard the news about my vision. I really could not handle it very well and came up to Oregon, ran out of money in a little rain swept town on the coast, and worked on the railroad for the winter thinking about what I should do next.

Littman: What town was that, do you remember?

Patterson: Reedsport.

Littman: Reedsport, which is a very large port actually.

Patterson: Yes, it is. I wanted to ship out on the Merchant Marines, but they wouldn't have me. So I decided that given the fact I hadn't gone blind, I'd go back to school and become as much of a psychologist as I could. The closest place was Eugene, Oregon, The University of Oregon. I don't know why I remember this, but when I got off the train I stayed at the Smead Hotel by the railroad station. Our institute is right across the tracks from that little funky hotel. That evening they had a seminar about Bellamy's *Looking Backward to the Year 2000* with a tiny group of old men and one very mixed-up boy.

Littman: What year was this?

Patterson: It must have been 1946 or 1947, something like that. Anyway, I took an undergraduate degree in psychology. I had courses with you and Leona Tyler. Tyler was teaching individual differences and counseling courses.

Littman: This is Leona Tyler who ultimately became President of the American Psychological Association--

Patterson: Yes.

Littman: --and who is Dean of the Graduate School at the University of Oregon and came from a section of Minnesota, which was not very far from where you grew up?

Patterson: That's right. She had an accent very similar to mine.

Littman: Did she come from Ely or from Hibbing?

Patterson: Hibbing.

Littman: Her father was a mine master, paymaster in Hibbing, as I recall.

Patterson: That's a harsh climate; it drives people out. If you survive, you have to have been strong. I found the department very congenial though, and you people--well, I remember coming to you Dick and saying, "I wanted to run some experiments," and you said, "Well, what kind of experiments?" I said, "Well, I want to know how it is that music affects human behavior." And you were very patient with me and sent me to the library, and I had to read all these miserable books on music education, I remember. And while we were designing the rat studies that might get at what I wanted, you talked me into running three or four of your experiments testing Hullian learning theory. In fact, we were testing Postulate 16; that was great intellectual fun. I really got caught in the psychological web during those two or three years of undergraduate training and effectively began a solid training in research methods.

Littman: As a matter of fact, that resulted in your first publication while you were an undergraduate.

Patterson: That's right.

Littman: We had a joint paper with Robert Blaya, as you recall (Dick was senior author).

Patterson: Yes, that's right. And in the process, we also built the first laboratory in the Psych Department.

Littman: Animal laboratory at the University of Oregon in Psychology, so that was a very flush period of time.

Patterson: Well, and I remember during one of these studies somebody coming and knocking on the window at the lab and saying, "Your first child was arriving."

Littman: Was arriving? Well, that'll be for somebody else to do me in more details.

Patterson: It was great intellectual fun, and I think, you know, a fundamental turning point in my educational career. I really became interested in psychological problems. I still hadn't given up art. I did a perfectly awful Master's thesis trying to use art therapy with delinquents, and the less said about that paper the better. It was an attempt to pull together the two streams, which was the idea of art in the service of helping young people.

Littman: You haven't really moved very far from delinquency after all!

Patterson: I haven't. I was pretty close. Well, yes.

Littman: So, in some sense, one of the next questions we have is what are the origins of your interests in child development?

Patterson: Well, one of the--just to continue that other discussion for a second, one of the jobs that I took when I stayed here getting my Master's degree to earn money and to flush out the money the Veterans Administration was paying was to work as a Probation Officer. Then they made me a Liaison Superintendent of their detention home, so--

Littman: This was here in Eugene.

Patterson: Yes, here in Eugene. So I was getting massive exposure to the phenomenon of delinquency. It's remained a lifelong interest. I think it started there because I was taking courses from Leona Tyler on all kinds of things like psychodrama and psychotherapy and Rogation nondirective counseling. In fact, Dr. Tyler and I created the first play therapy room on campus. She was very supportive of young people wanting to try things out. I could see that none of the things we were trying worked for these out of control children even then. It was fun to do and a rich source of clinical phenomena; the treatment didn't deflect any of these children, and that really bothered me. Dr. Tyler and I used to talk about, you know, what it might take, but I left without having any idea what the alternatives might be.

Littman: Would you say that at that time you had any real feeling of yourself as a clinical psychologist?

Patterson: I think so, yes. I was taking every course on child therapy or intervention that was offered on the campus, and when I went to Minnesota I did the same there. But at that time it was not saying much. At Minnesota, they really didn't have much to offer about treatment. They're very experimental, beautiful, learning theory, measurement and child psychology, but nothing on child clinical. I had to go and find an off-campus facility while at Minnesota (Wilder Clinic, St. Paul).

Littman: Well, would you say that there was very much nationally in child clinical psychology at that time?

Patterson: No. In fact, in most places child psychologists were not allowed to do child therapy. They thought at that time you should be psychoanalyzed yourself. If not that, that you worked with somebody who had been analyzed or trained so they could be sure that you weren't going to hurt people. It's not an unreasonable idea, by the way.

Littman: But child clinical in the sense was more a psychiatric--

Patterson: Yes, thank you, yes! It was totally dominated by child psychiatry and by--I was going to say Anna Freud and her theories, but that's another statement. What was the next question? Oh, the origins of my interest in child development.

Littman: Now who was--you mentioned Leona Tyler, for example, was a significant figure when you went back to Minnesota and took your doctorate. You then must have met some other people that had some impact on you in connection with your interest in child development?

Patterson: Yes. Minnesota was, again, an amalgamation of two very different things. One was the stream of experiences in the Psychology Department, which was heavily oriented towards measurement and experimental. It was similar to Oregon, but Minnesota pushed it much further. They were also very heavily orientated toward statistics, and behind that lay the emphasis of a philosophy of science as practiced by Paul Meehl and his entourage of really brilliant graduate students. I was totally immersed in those experiences because my close friends in graduate school were involved in the same programs. When we got together and we talked, we talked about these new ideas. The other half of the experience was in the child development programs across campus and that represented a very different kind of experience where the main theme was set by John Anderson and, for me, Merrill Roff, who was teaching factor analysis and life history kinds of approaches.

Littman: That's Roff, R-O-F-F?

Patterson: Yes, thank you, yes. His approach was that of a sociologist to child psychology. I found that then, and now, to be extremely useful. I don't remember whether I even took a formal course from Merrill Roff, but my thesis had him as one of the committee members, and I had read all of his papers.

Littman: Was Florence Goodenough in the department at that time?

Patterson: No, she was in St. Paul. I met her once, I think, but she wasn't active in the department then; she was certainly one of its heroes. My advisor in the Department of Child Psychology was Dale Harris. I sort of developed a double major in the Psychology Department where I took my exams and then minor exams in child psychology.

Littman: That was a pattern that existed in a number of schools, in fact, that you had a child development program, which was very closely related to education or counseling--

Patterson: Yes.

Littman: --on the one hand, and then a traditional experimental research-oriented Department of Psychology.

Patterson: Yes, and to get the clinical experience I needed, I had to add on a two-year internship that I created for myself later at the Wilder Clinic. Later, the Psychology Department and Child Development combined forces to create a really strong child clinical program. It was put together after I left, however, but it was exactly what I was looking for.

Littman: When you were in school you said there were a number of graduate students with whom you were fellow students. Have you maintained contact with any of them?

Patterson: Yes.

Littman: Do you have some names to give here?

Patterson: Yes. One of my close friends was Alec Caldwell who is currently a national figure in clinical interpretations of the MMPI. Another person that belonged to this same little coterie of students was Lowell Storm who has written a whole series of papers on Hullian learning theory; you see some themes never die. In fact, one of the first papers I did when I got out of graduate school tested Hull's theory. The third person was Chip Dickens who has recently retired from his teaching career at San Diego.

Littman: That was the connection with Oregon because Chip was the son of the Professor of Geography here at the university.

Patterson: Yes. So I see Chip every other year; we ski together and so on. I have maintained a loose contact with these people over time. And by the way, I think it's the level of the graduate students that in part determines the quality of a really great department. I mean, you have a collection of people we had at Minnesota at that time that's really exceptional, and they were studying widely different things; so it's marvelous to go to parties and just sit around and talk with them.

Littman: That's probably still true.

Patterson: Yes, I'm sure it is, yes. Let's see where are we here?

Littman: What political and social events have influenced your research, writing, and teaching? We sort of hit on that partly in terms of your experience in the Army, but I think you might want to elaborate on that.

Patterson: Well, I think during the 1960's when a lot of us were taking our first jobs after graduate school there was a social activism that really some of us are still carrying forward today; we didn't stop. The 60s eventually seemed to run out, but I feel that the center we have here is in many ways an extension of that build of activism. The Center was designed by me personally (it emerged), but as we put it together it strongly emphasizes non-exploitation of others. And as I was talking briefly a moment

ago, here at the Center we also strongly emphasize cooperation rather than zero-sum games, and to me that's also part of the 60s. Above all was the sense that you could use science to make society a better place; I think that's what this center is all about.

Littman: Have you ever been explicitly involved in politics like the Democratic Party, the Republican Party, or the Progressive Party as an active member or participant?

Patterson: I'm chagrined to say I never have; I should. And as in thinking about retiring, I'm thinking that I may become politically active in the environmental movement or in some such thing.

Littman: But what you've done so far has been sort of essentially professionally oriented in terms of your activities?

Patterson: Yes, totally.

Littman: Now that's not necessarily incompatible with being a politician is it?

Patterson: No. It's just a very different set of skills, Dick. I've never learned how to be politically--I was going to say astute or skilled, but I realized in my case I'm lucky to survive. I just don't have the sense about what's going on when strategic decisions are being made and taking into account their long-range implications. The only reason that we've been able to survive as a group here at the Institute is that there are other people here at the Center like John Reid, my colleague, who have this sense and that's really helped us survive. If it were left totally to me, we wouldn't make it. In the Psychology Department at Oregon when I'd sit in the staff meetings doodling and thinking about other things, they changed the department right in front of me. All I had to do was watch and I could have seen it; I didn't. The department changed, and I had to leave.

Littman: Well, that's certainly an interesting part of your existence; I'm not sure that we really got into that. It may come up a little later, but you were in the Psychology Department at the University of Oregon from 1958 was it?

Patterson: I think so, yes.

Littman: To 1960?

Patterson: '67 or '68.

Littman: So for about ten years.

Patterson: Yes.

Littman: How would you describe the influence or the effect of having been in that department upon what's happened to you since then? It's a kind of collegiate experience though, a little somewhat more advanced level, professional level.

Patterson: Well, some of the people in the department who were my colleagues at Oregon, Paul Hoffman and Lou Goldberg, started a research institute off-campus. It was based entirely on soft monies from research grants, and they were talking about measurement problems and statistical ways of measuring profiles, nonlinear models and so on. That really interested me, so I began to attend their bull sessions. In the process of sort of hanging out with them, I eventually decided to join them on a full-time basis. As I mentioned earlier, the Psychology Department (I'll give you my characterization) was undergoing a profound transition. I totally missed noticing the changes until after it had happened. It was in the process of becoming one of the most powerful departments in the country in terms of scientific contributions. The contributions were being made totally in the realm of cognitive science,

and to prepare for that transition a brilliant young new staff was brought in like James McGough and Mike Posner.

Littman: And Lou Breger.

Patterson: Yes. Some of them launched a dedicated attack on behaviorists like myself. We behaviorists lost every debate as far as I'm concerned. By the time the conflict was over, I was the only one left standing in the behavioral group. I certainly could have stayed, but I chose not to. I mean, it was no longer intellectually stimulating. I was too busy having to defend myself and my students, so I chose to join the Oregon Research Institute where there weren't any such debates.

Littman: Did you switch over to the College of Education at that point?

Patterson: I got a five year Career Development Award that could only be administrated in an academic setting. Perhaps I could have gone back to the Psychology Department, but that was too complex for me to deal with. The Career Development Award was administrated through the College of Education, but I don't think I had any teaching responsibilities. Once a week I'd go to the campus and the rest of the time was spent at the Oregon Research Institute.

Littman: So that was really a convenient arrangement then. You had no intention of staying in education at that point?

Patterson: No, no; they were not terribly interested in what I was doing either. I was happily focused by that time on solving the question of, "Where does aggression come from in children and how do you change it and how do you prevent it?" And that's very much applied and that's very narrow, I mean most--

Littman: And it goes back to your early experiences as a Probation Officer, doesn't it?

Patterson: Yes, and maybe to the war, I'm not sure. And it fits nicely with this sense, you know, that I'd like to make the world a better place.

Littman: Okay. Now I think we can move from some of that, although you can't completely eliminate personal involvement to what you regard as your personal research contributions, that's the second section of this Oral History. And to remind people, this is Gerald Patterson who's being interviewed by Richard Littman. Now I think we've already covered the answer to the first question, which was, "What were your primary interests in child development at the beginning of your career?"

Patterson: Initially, I was primarily interested in creativity in children; my Master's thesis was about that and delinquency. Later the focus shifted to learning theory. Then later, we started developing a different way of thinking about aggression. I realize I'm putting this story together; I'm making it much more logical than it really was. To study aggression in children we had to start by developing observation techniques so that we could collect data showing what was really going on.

Littman: How is this different from observing behavior of rats in a cage? I'm just bringing you back to your early experiences as a student and your first research experiences.

Patterson: Well, the problem of observing rats in a cage involves the usual psychometric questions about, "Well, how much data do you need to get a reliable estimate of what the rats doing, and then how do you calculate reliability?" And in fact, when we started studying aggression in children we started with something very similar to rat experiments. They were tightly organized, randomized trials where we'd assign some children to conditions where they get certain kinds of reinforcers or aggressive-like behavior (e.g., punching Bobo, the clown), and we did experiments like that for about five years because that's how I had been trained. The experiments were not bad. I don't know that we

ever published them, but they were very much like what Bandura and others were doing. The problem was that by chance we were given a chunk of money, that for which we didn't have time to design an experiment, so we sent observers out into the real world (that is, the playground at the nursery school) to see if anybody was standing by and giving aggressive children M&M's or other kinds of reinforcers for being aggressive. That's was about the level of our thinking at that time; it was terribly naïve. And the observers came back in the first week and the data said, "You're absolutely wrong that the probable reinforcers for aggression lie somewhere in the behavior of the victim." There's no adult standing around giving praise, M&M's, or any of the reinforcers we had used in our laboratory studies. Real aggression in natural settings was occurring under auspices that were totally different from anything we had thought of. We realized we had to start all over again and begin by going into the field and making very careful observations. At that time in the department at Oregon, one of my colleagues was Phil Schoggen. He was visited each summer by Roger Barker and an entire entourage of Kansas investigators. We started studying how to observe aggression. We began with their technique. As we are talking, I'm struck with how often it is the fortuitous factors that are nudging you in one way or another; I mean, we were very fortunate in having Phil Schoggen as a colleague.

Littman: Now what time would that have been? You were still in the department at that time weren't you?

Patterson: Yes. That must have been in the mid-60s.

Littman: Have you had any continued contact with Schoggen since then?

Patterson: No, and we haven't been contacted by any other of Roger Barker's people. I think the reason may be that we decided on a different way of thinking about observation; it violated the way that Schoggen and Barker wanted to think of human behavior. They were viewing social interaction in the way a Levinian would; behavior is a function of a field of forces. You need to get a very global picture of what the goals are for each behavioral unit, for example. And in my training at Minnesota, I wouldn't call myself a behaviorist, but--

Littman: Not any more.

Patterson: Well, I'm not sure. I seemed to fit the behavioral definition for a bit. However, as soon as I started building a code where 27 categories were recorded in real time, both the behaviorists and the Levinians lost interest.

Littman: That provides a good part of the answer in discussion of the second question, which is, "What continuities in your work are most significant, and what shifts just occurred?" You just discussed the major shift from the experimentalists' approach, which involves controlled experiences to one in which you're trying to deal with natural phenomena in a controlled fashion.

Patterson: Yes. And then, as we'll eventually get to, 20 years later we come back to the problem of, "Now that you've established correlational models, how do you identify the causal mechanisms?" You can only do that by doing experiments. "How do you do those experiments," but that comes much later. It feels as if we're describing this as sort of a natural progression, and I guess it is; that's sort of the way science works.

Littman: Jerry, I think it'd be a good idea if we stopped here and I turned the tape over because it seems like a natural place to take a break.

This is the second part of the interview with Jerry Patterson on the SRCO Oral History Project. We are now on side B of our tape. Okay Jerry, we had a little interruption, but now we'd like to get you to reflect on what you view as the strengths and weaknesses of your research and theoretical contributions. What you think the impact of your work has been and what its current status is? This is a very difficult question to have to review and talk about, but you're on camera.

Patterson: Yes. I find this really very difficult because I don't trust self-reported data anyway. Now I'm asking myself to report on myself, so--

Littman: Well, there's no reason people necessarily have to believe what you say.

Patterson: That's true. Well, I think what we set out to do was to build what we call a 'performance theory' about aggression in children, which requires that you not only specify the variables that you think cause aggression, but you also have to specify the means by which you measured those variables. You express it in terms of variance accounted for in measures of aggression. And I'd say there have been several insurmountable problems in addressing those questions, and one of them was immediately apparent in the 60s and 70s and 80s. You can see in our papers that you can't use reinforcement metaphors or theories to explain individual differences. When you read the textbooks in experimental psychology, it is clear that there's no linear relationship between density of reinforcers and response strength. In fact, it's a curvilinear relationship. Therefore, are you going to explain why one child is more aggressive than another using such an idea? You can't. I found that to be very frustrating. And it wasn't until five years ago that we successfully published our first paper showing that there's a way of getting around that problem without boring everybody to tears. It involves translating reinforcement theory to an intra-individual level. If you're going to explain why a child is aggressive, you want to account for the contingencies and the reinforcers that bring this about, but you have to study at the same time the reinforcers for all the other behaviors. So at the intra-individual level you have relative rates of reinforcement for deviancy as compared to relative rates for social confidence. You put together relative rating reinforcement and frequency of training trials and you now account for much of the variances in aggression.

And how do we know that? Well, in a study that Sneider and I published in 1995, measures of relative rates of reinforcement and training trials accounted for 60 percent of the variance in observed aggression measured a week later. In the second study of older kids in a wider age range, the observed relative rate of reinforcement for deviant behavior accounted for 44 percent of the variance in measures of delinquency obtained two years later. And then there were more studies that occurred since then that support this idea so that I think the model specifies what it is at an immediate micro-social level that pushes the aggression behavior. Then in more recent studies we found that deviant peers provide fantastic rates of relative rates of positive reinforcement for deviancy, and it produced very powerful effects on long-term outcomes. So we're able to talk about the micro-social variables that function as causal mechanisms; we can measure it. We can replicate the effects and show they do account for enormous amounts of variance.

Our modeling studies in the last ten years are addressing the next set of questions. "Why do some parents reinforce deviant behaviors and fail to reinforce social confident behaviors?" So the kinds of variables that we are now concerned with are drawn from sociology and variables such as poverty, neighborhood, divorce, number of family transitions, the age of the mother, etc. Then shifting to psychology, the depression in the mother, the stress level, I.Q., education and so on, all these are contextual variables. So we must have 20 or 30 papers on this showing the effect of context on child outcome is mediated by what the impact is on these contingencies that I've been talking about.

Now another set of variables that we're trying to get leverage on, and for which we have three proposals in the Center right now, is biological or genetic variables. It's turned out that it's very difficult to get an estimate of how these things influence aggression. This is because there's been a century of twin design and adoption design studies that are just very badly done. It's just not very strong science, and that's unfortunate because those are crucial questions.

Littman: Now would you say what you've been describing could be thought of as a general personality theory? You've been talking--all our discussion so far has been in terms of behavior and actions, but it seems to be that what you've been outlining could well be regarded as a model of the generalized personality theory. It's not a psychometric theory, obviously.

Patterson: No, you're right, Dick; thank you. And, in fact, when I was talking about the genetics proposal, which we're in now, there's also another proposal that has been funded and we're collecting the data where we're expanding the model further, and it's very much in keeping with your comment. The new expanded model says, "Well, what's the impact of negative emotion on contingencies?" It's not in psychology's best interest to have a course in emotion, then another course down the hall in contingencies, and then a third course taught by somebody else on social cognitive processing. So in this most recent grant by Snyder and Stoolmiller and myself, we're getting the best measures that psychology can provide on all three of these things, and now it becomes a straightforward multivariate model. Well, it's more than a multivariate model, but it can start with a multivariate analysis where you have very good measures of emotion plugged in and very good measures of contingencies. Ken Dodge's work on social cognitive processing to predict a latent variable in aggressive child behavior and the preliminary studies show what you'd expect, that, of course, contingencies account for more variance than anything else; that's not a surprise. But it's necessary to pin that down so we don't go on talking about social cognition as if it's the main causal variable for aggression, it isn't. Well, we'll see six months from now, but that's the way the data looked. But the more important question is, "How does negative attribution or social cognitive processing fit in here?" I think the way that it's going to work is it will help us understand why some parents reinforce deviant behaviors and others do not, that's cognitive. And the function of negative emotion in this model, well, maybe it'll be the way laboratory reinforcement theories laid it out years ago, that the more intense the emotion the greater the magnitude of the negative reinforcement effect, just like with rats and pigeons. I'll bet in part that's going to be true, but see that leaves out the probable path from negative emotion to negative attribution; see now we've got loops going everywhere. And when we get done with this in another half century, it will be a theory of behavior that's empirically grounded this time. It's like putting together a Tinker-toy thing, you know, only a lot more exciting.

Littman: Let me put another proposition to you to think about. Much of what you've been talking about is very reminiscent of the older and now newly current sort of self-theories of people's behavior. That is, you have to understand the nature of the person, what's important to the person, and what values are primary to the individual to determine in an attempt to understand why a particular individual in a particular context does the kind of thing that person does.

Patterson: Well, I think that the self-theories and the behavioral theories and the field theories all had something--they were looking at something that was real. The thing that bothers me is that they never talked to each other and they never acted as if these variables must act in concert. When we have a real theory of behavior, we will understand the inner connection among, you know, the self, the field, the biology, and the contingencies; they're all true.

Littman: So it's like the southern Methodists and the southern Baptists?

Patterson: Well, it feels to me that our approach is a very pragmatic, "How do things work?" And as you really address that question and stay open to the possibilities that there are variables outside of your immediate kin that operate in here too and that you can add them in--what I'm talking about, you can put them in--it's no longer illegal for me to think about emotions, or fields, or--

Littman: Or thinking.

Patterson: --or thinking. Yes, that's right. I'm reading books on thinking, but I think it's a gross mistake for people who are thinking about thinking to conclude the contingencies are not important; I know they're wrong. And we'll never have the psychology until we start building this larger picture. We hadn't originally set off to do this; it's just we tentatively solved, at one level, this question, "Where does it come from?" We know we can intervene, and now we know we can prevent. So in the last five years we're saying, "Alright, let's go back and think about building a better theory."

Littman: So if you were interested in the question such as, “Where does generous behavior come from,” you would essentially go through the same pattern of activity and research that you’ve been doing in connection with aggression?

Patterson: I think so, yes.

Littman: And at some point in the future you’d find that there would be key features, which would interlock them. So you would say aggression is just like moral behavior, except that they have different outcomes?

Patterson: Yes. There’s just similar structure, you know, but a different structure. I think what we’ve inadvertently done is to develop a research strategy that could be applied to studying creativity, moral development, or altruism.

Littman: And you’ve had different weights depending on the particular area, or different factors?

Patterson: Yes, right.

Littman: So you really still have kind of a structuralists approach going back to your experiences at Minnesota and Merrill Roff?

Patterson: Structural approach? I’m afraid that I--

Littman: Well, that you’ve got a big pattern.

Patterson: Oh, yes.

Littman: You’ve got a whole bunch of interlocking variables--

Patterson: Yes.

Littman: --and different variables, different weights, and different situations with respect to different outcomes?

Patterson: Yes.

Littman: So you’re at heart a psychometrician?

Patterson: Very much so, yes. And the psychometrics is a tool for addressing increasingly complex questions.

Littman: Well, I think we’ve covered all that territory. One of the questions now is to reflect on the strengths and weaknesses of your research and theoretical contributions, the impact of your work. That is to say, what is it that you think you did that you wasted your time at, alright?

Patterson: There were so many cul-de-sacs; I don’t even want to bother listing them. I mean, the wrong questions and I’d spend years at--

Littman: So you feel that somewhere or another these have led you down a path where you’re feeling very good about the productiveness or--?

Patterson: Yes. I think the young people that are working at the Center will redefine this path, I’m sure. But the idea of working on practical applied problems like, you know, “How do you help people deal with aggression,” or maybe someday they’ll work on schizophrenia with problems like, “How do you understand schizophrenia or neuroses, you know, anxiety and depression?” It’s a nice strategy and

I could see the Center selecting one new applied problem after another; you should have places like this that do that.

Littman: Would you say that the relation between this and general psychology have some likelihood between the engineering and general physics?

Patterson: Yes, I would think so. We're applying the best knowledge from psychology that we're aware of as a group.

Littman: And, in fact, also in making some general contributions?

Patterson: Yes, in an applied sense. I'm not sure that we're making any basic contributions, but now that gets me into an area that gets really vague to me as to what's basic and applied. But our focus is answering the question of, "Where does the aggression come from in this child," or, "Why does this child fail in school?"

Littman: Have you been approached by the local authorities and reporters in connection with the Springfield Thurston shooting?

Patterson: Yes, we had a flurry of telephone calls that I don't think we handled very well as a group, or that I personally didn't handle very well. The problem is we didn't have much data on Kip and his family, so we didn't know whether he fit the model or not, and--

Littman: Were you able to convey this to the people who had called you?

Patterson: Much to their disgust. They thought we were being very--

Littman: Evasive.

Patterson: Evasive, thank you. Yes, that's the word. We're not. I see, having received a lot more information, that in some ways he represented a very, very different model. Some of his history could be addressed by our model, but some of it could not. I mean, the hearing voices and hallucinating--

Littman: That's very different.

Patterson: Yes.

Littman: You would also need some information about his parents and--

Patterson: Yes, but we didn't have--

Littman: And that's been masked anyway.

Patterson: Yes, as it probably should be.

Littman: I was interested because generally in other areas and with applied areas, reporters go to people that are mostly involved in research, engineers for example, and they'd say, "Tell me about this," and the engineer will tell them. The engineer will say, "Well, you know, we really don't know," and the newspapers report that as fact and the articles are slanted. So they say, "Well, at the present time scientists really don't know." But they don't do that with psychological research, do they?

Patterson: No, we don't do that with each other; we don't take each other's findings very seriously. There's no standardized set of measures that we all agree on that would say, "Well, 'X' is caused by 'Y'." I think it'll be another 50 years before that changes.

Littman: Now you alluded to both published and unpublished manuscripts, so I think we can pass over that particular question, but you do have a lot of experience with research funding, and that's a question we'd like to ask. What are your views about research funding and your participation in shaping, if you believe you have, funding policy and implementation? Have you served on review committees, for example?

Patterson: Yes, for five years; it was an NIMH section on pre and postdoctoral fellowships, and I learned a great deal from seeing how the process worked. I don't know that I have much that's useful to say about this, Dick.

Littman: Except the fact that you received funds.

Patterson: Well, yes. I've supported myself for 20 or 30 years now. I've personally felt that the peer review process works very well, and it's not just because we've been supported for that length of time. When we've turned in bad proposals they get rejected, and we've elected to go off several times and into corridors where we didn't belong; and we were firmly rejected. I think there have been two periods in our history where we missed on three or four consecutive grants so there was no income, which is appalling. In fact, that's worth commenting on.

I think we're not going to have a lot of young people trying the relative, the Oregon Research Institute, Oregon Social Learning Center, because it's really very high risk for a young Ph.D. trying to raise a family to try to come to a place like this and make it as a principle investigator. As a matter of fact, legally when your banker asks you, "Do you have a job," you should say, "No, I don't." You're going to be paid on your research grant for the next year or two, but then you may not have an income and the banker wouldn't know that. But that also emphasizes one of the weaknesses in this whole strategy: "How do you support young people?"

The other thing that's left out, by the way, is in our own case we're divorced from the university campus. That means that we're not training young graduate students in these ideas or in the research strategy and that's really a shame, but we've never been able to figure out how to do that, although periodically we try.

Littman: Well, you do have people, in fact, who are graduate students at the university here and have kind of joined into your project somewhat, now come on to the staff here.

Patterson: Yes, like Capaldi, for example.

Littman: And some of them have gone away. But I think there's no explicit arrangement, and maybe that's what's protected both the department and the OLSC that's it's been left to a collegial--?

Patterson: Yes, and it's very positive. You know, I always feel welcome when I go to the department and they do as well, I think, when they come here.

Littman: I certainly over the years changed my attitudes towards formal alliances. I think they're a little bit like the kind of problem you had in North and South Korea, having a formal alliance.

Patterson: Yes. It's worked all right. I mean, there are occasional students, but your child clinical program is not using any of these new treatments.

Littman: No.

Patterson: Maybe it did when Tom--

Littman: They may.

Patterson: Yes. When Tom was there, but--

Littman: I think he's having an impact, but that's a different interview.

Patterson: Yes, right, ok. But I think I don't have John Reeds astute understanding of policy changes. I mean, one of the problems in surviving this way is in knowing what the winds of change have brought to Washington while we're out here on the west coast.

Littman: I think you've really covered most of the substance of what these questions were getting at. I'd like to move to something narrower, which I guess SRCD would like to have and that is your experiences with SRCD. When did you join? Did you ever join?

Patterson: Yes, I belong. No, I think SRCD is the preeminent group of child psychology researchers, and the first meeting that I went to as a young Ph.D. you had arranged the luncheon meeting with Harold Rausch, who has remained one of the heroes, and the first ten, 15 years of our work on sequential interaction, so--

Littman: You know I was the one who taught Harold how to do that stuff.

Patterson: When you were at NIMH?

Littman: I went there to teach him that and, you know, we had already done a good deal of research here--

Patterson: No, I didn't know that.

Littman: --in the early 50s we had developed a coding scheme for studying Indian children on the one hand with also newborn infants in a nursing foundling home.

Patterson: I'll be damned; I didn't know that.

Littman: So you didn't remember that?

Patterson: No.

Littman: So when you got into this with Phil Schoggen there was a conflict between the technology that I had developed, along with a number of other people including what's his name again at Oregon State, which involved very explicit observational coding systems and the informal essay type account of the Barker and it's just two completely opposed technologies--

Patterson: That's right.

Littman: --and you moved away from Schoggen to the methods that I had sort of developed about 15 years before then, but never pursued, just in my typical--I was on to something else by the time you--

Patterson: But that was the right way to go.

Littman: Yes, I knew that.

Patterson: Yes. And in retrospect it was, well, again the fortuitous theory.

Littman: So when was that, the first Biennial Meeting? That would have been '62, '63?

Patterson: I think so, yes. Yes, Schoggen was still here because his wife Dickie went to that meeting and it was held at Penn at a beautiful campus.

Littman: Had Dale Harris moved from Minnesota to Penn at that time?

Patterson: I don't think so, no.

Littman: It was just after that meeting because he died--Dale died very recently because he had been at Penn State in charge of the Child Development program there for many, many years.

Patterson: Oh, right. Yes.

Littman: So Minnesota's fingers have spread out.

Patterson: They're everywhere.

Littman: I think what they're really interested in with this question, Jerry, is how much you've done within the fabric of SRCD as a participant in committees and things like that.

Patterson: I think that I have a history of non-involvement, and so--

Littman: That's what I was thinking. So you've kind of stuck to your role as investigator rather than as a politician?

Patterson: Yes.

Littman: Or an administrator?

Patterson: Yes. They've been very generous with me in presenting me with awards and forgiving me for my non-involvement, I guess.

Littman: Well, let me ask you this question. Are you aware of any significant or substantial changes in the SRCD itself over the years that you've been a member, whether or not you've sort of been intimately involved in any changes that you might see?

Patterson: I don't think so. For me the high points are the Biennial Meetings, the quality of the papers there, and the people. I find all my friends there; everybody I want to talk to is there; you could spend a week. So I've not been privy to any of the political changes in SRCD. I think as a non-academic, but SRCD is for people in academics.

Littman: Now it does have a fairly substantial number of M.D.'s and Psychiatrists who belong, but are they generally associated with an academic institution?

Patterson: Yes, and most of them are teachers, like Brazelton and Glen Elder.

Littman: Okay.

Patterson: Yes. I'm a maverick that doesn't quite fit in there.

Littman: Well, to put words into your mouth, I'd say you're kind of hopeful about the field. What do you think might disrupt its development, besides lack of money?

Patterson: No, I don't really expect there would be a lack of money. The big problem that we have now is understanding the political field; it's not the scientific field, but the field containing the

decision-makers and the state legislatures and at a federal level. I always thought as a young, naïve scientist that if we have proven by carefully designed studies that we could intervene effectively and have long-term follow-up, and that if we replicated that then the mental health field would come to our door and demand that we train people. It really hasn't happened; it will not happen. That's our biggest problem now is understanding how to work within the decision-making field and help them change and incorporate some aspect of these ideas.

Littman: It's a little bit like the problem that people who want to fly to Mars are facing.

Patterson: Do we know how to do it?

Littman: Know how to do it.

Patterson: Yes. All you have to do is say yes. I was invited to come to Washington to testify at a hearing. I got there and it turns out I had seven and a half minutes to testify on what we should do about violence. I said, "In the first place, you have the wrong theories; your theories don't work, and therefore your treatments don't. There is a new theory that we've known about for 20 years that you paid millions of dollars for," and then a little light started going off, which meant I was running out of time.

Littman: Which committee was this?

Patterson: Oh, I don't know. There were five people from the House who wanted a photo-op. We only had two hours, so an hour was taken up with ten minutes for your photo-op. It was a fiasco. I was really angry and I wrote our senator a letter and said, "You put maybe 50 million dollars or more, a 100 million considering all the different centers working on this thing, and we now have the set of techniques worth trying and you show no interest so I'm giving it to Norway. If I'd been building a bomb for you and had one that was pretty effective and gave it to Norway, you'd shoot me as--"

Littman: Did you get an answer?

Patterson: Yes, he said, "Well, we should get together," DeFazio, who I had written a letter to, and I will.

Littman: Well, I think he's got more sense and he's likely to be responsible, but he's really kind of powerless, so--

Patterson: Yes. I know. I was ranting and raving at Sorias Restaurant last night and I didn't realize my voice was so loud. Some guy at the next table sent his card over and on the back it said, "I can't help but overhear what you're saying, let's get together and have breakfast at Zenon," and it was Al King from the House of Representatives. So I'm going to do that next week, but I don't think that's how things are going to change. They changed in Norway because the ministry said, "God damn it this stuff doesn't work; I want something that works."

Littman: It's a different political system.

Patterson: Yes. It's rational.

Littman: Yes. It's rational, but it's also completely individualized and power-oriented, and we don't have that here. We have too many cooks trying to cook different kinds of food in the same pot.

Patterson: I'll bet you ten years from now, Dick, that our stuff will be used. I think the new young staff here is much more effective than some of us older types were in the political arenas.

Littman: I think we've just about covered everything.

Patterson: Yes.

Littman: The one thing that is missing is some kind of documentation of what they might like to have, an up-to-date CV and a picture if possible. If you'll provide that to me I'll send it to them.

Patterson: Yes, sure. Should it be a picture of when I was 50, or does it have--?

Littman: Choose your date. It could be at 17 with an ax in your hand, or you could be 20 with a paddle in your hand.

Patterson: Or 60 with a paddle in my hand.

Littman: Okay. Jerry, let me turn things off and then maybe there are some other things that you might want to say. So this terminates the interview with Gerald Patterson for SRCDC. We have two sides of a 90-minute cassette, and we're very close to the end of this. So let me just say, Jerry, it's been a pleasure; we're reminiscing and covering some old stuff, and I'll get from you whatever else is necessary to send to the SRCDC.

Patterson: Thanks, Dick.

Littman: And we're shaking hands on it.

Those who inspired and were influenced by Gerald Patterson:

Mentors

Leona Tyler
Richard A. Littman
Merrill Roff
Florence Goodenough
Dale Harris

Colleagues

Phil Schoggen
Alec Caldwell
Lowell Storm
Chip Dickens
John Reid
Paul Hoffman
Lou Goldberg
James McGough
Mike Posner