

Linda Siegel

- Born in Washington D.C.
- B.A. (1963) Queens College, M.S. (1964), Ph.D. (1966) both at Yale University

Major Employment:

- McMaster University Medical Centre – 1981-1984, Professor
- O.I.S.E. – 1987-1991, Executive Head of Graduate Studies
- O.I.S.E. – 1984-1996, Professor, Department of Instruction & Special Education
- UBC – 1996-Present, Dorothy C. Lam Chair in Special Education



Major Areas of Work:

- Cognitive aspects of learning disabilities, Reading, Arithmetic.

SRCD Affiliation:

- Chair, Program Committee, SRCD 1983-1985
- Member of Governing Council, SRCD 1985-1991

SRCD ORAL HISTORY INTERVIEW

Linda Siegel

Interviewed by Keith Stanovich
At Ontario Institute for Studies in Education, Toronto Ontario
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Stanovich: So following the general interview schedule here, let's begin with what they call general intellectual history: Things in your family background, childhood, let's say pre-college experience that might have led you to your present career status or to be the background for later career choices. Is there anything in that early childhood or adolescent, early adulthood period that you think is relevant for us?

Siegel: I was always interested in math and science, but I couldn't see myself as having a career working in a laboratory. I liked spending some time with people also. So when I first encountered psychology in my second year in college, psychology was taught as a science, it seemed to be the perfect combination making use of my interest and skills in math and sciences, with my interest in people.

Stanovich: So it was kind of a meshing of some math interest and aptitude with a kind of social and interest in people as well, the true kind of role, the social sciences in between that and the natural --

Siegel: That's correct.

Stanovich: -- in humanities?

Siegel: Yes.

Stanovich: You mentioned college. Now that was Queens?

Siegel: Queens College. Yes.

Stanovich: As an undergraduate. Are you a New Yorker?

Siegel: I was born in Washington D.C., and moved to New York when I was six years old, where I acquired my New York accent and my New York personality.

Stanovich: Okay. I see. And then Queens was a logical –

Siegel: Yes. My parents could not afford to send me to an out-of-town school. As you know, Queens College was free, there was no tuition, and one lived at home. So it cost \$5.00 a term to use the student facilities, and occasionally you had to pay an extra \$5.00 for a biology lab, or whatever, but basically it was free.

Stanovich: Oh, my goodness. So you're one of the illustrious products of which there are many from that system of free university education. But then the – were there any particular undergraduate experiences that set you on your path? Once you decided on psychology, was it immediately apparent that that was the right niche?

Siegel: Yes, it was. I started working in my third year in the lab run by William Reynolds who was a Spence Ph.D., and we started doing various studies in traditional post-Spence learning theory, and that very much appealed to my mathematical interests.

Stanovich: Yes. Found the perfect axiomatized psychology –

Siegel: Yes. Yes.

Stanovich: -- that was a perfect mesh of that. And then was it that, that sent you on to Yale from Queens? Was it working in that tradition?

Siegel: Yes. Yale at that time was one of the seats of post-Spence learning theory, although certainly, by the time I got to Yale in 1963 it was on its way out, but that hadn't reached Queens yet. And it very quickly became apparent to me, because I got interested in aspects of animal behavior, I took a course in animal behavior in the biology department, that there was a certain superficiality and lack of ecological validity to the kinds of work that people did in the rat psychology of that time, or the experimental psychology at that time. So, I took some courses in developmental psychology with Bill Kessen and Ed Zigler and got interested in development.

Stanovich: So that was during the Yale period –

Siegel: Yes.

Stanovich: -- that that type of – Now there's a migration in your Vita in the period of the late 60's and early 70's, there are a couple of publications and learning theories as you just mentioned, then there are a series of what we might call basic process investigations in developmental psychology. Did this come out of the Yale period and your work with Kessen and Zigler, or was that post-Yale?

Siegel: It started at Yale, because my thesis was on information processing. I, again, was interested in some sort of systematic approach to cognitive development, and at that time I encountered at Atteave/Gardner information theory and tried to apply that to cognitive development, that was what my thesis was, and then my approach to Piaget and cognitive development came out of that tradition.

Stanovich: Was your interest in the Atteave/Gardner -- through Neisser's book, or was that actually reading Gardner in the original?

Siegel: No, reading Gardner and Atteave. And Bill Kessen had done some work; Kessen and Munsinger, on the random shapes that they used. And my thesis was not on the random shapes, but it was on the trying

to look at various concepts – trying to operationalize concepts with information theory in terms of cognitive development.

Stanovich: That was very, extremely early in that trend to – using information processing formalisms and paradigms to elucidate developmental problems. You, in a sense, were extremely early in that trend which blossomed in the later 70's, but as one looks at your Vita, there are all of these papers in the very early 1970's and late 1960's. Was it clear to you that that would be the Zeitgeist to come, or did you feel at the time a bit of an outsider or a pioneer, or –

Siegel: I certainly felt like an outsider, because it was not a popular approach at that time. Cognitive psychology really didn't exist as a discipline then. But I also became dissatisfied with it, because that particular information processing approach concentrated on the stimulus, and measuring the dimensions of the stimuli in pure numerical terms without any awareness of more general cognitive constructs, so it was an attempt to make a psychophysics of the stimuli, of more complex cognitive stimuli, but without any of the cognitive basis.

Stanovich: So, in a sense, some of your dissatisfaction with those paradigms mirrors some of Bruner's more recent writings about where cognitive psychology went wrong, so to speak, in its emphasis on the measurement on external information. So, in a sense, you're pioneering that framework for developmental psychology, but also your dissatisfaction with it is very long-standing I guess. And I think a major thing that listeners will want to know, and something that again jumps out at a person from your Vita, is that following an extraordinarily long and well-developed series of basic processing investigations in cognitive psychology, we see a very focused interest in developmental disabilities come into your work. Can you describe for us when that – and of the genesis of that particular interest?

Siegel: That was a result of having a job in the medical school, in the psychiatry department, in which work in cognitive development was not highly valued. There were, of course, some positive aspects, some interesting populations that were available. For example, I started to do my longitudinal studies of pre-term and full-term children, and for developmental psychologists a longitudinal study is a wonderful thing to be able to do and to ask some practical questions. So the jump was really a result of where I was working, and the pressures of the particular situation that I was in, it didn't – although there was some aspects of the work in cognitive development that that was not very popular because it was anti-Zeitgeist at the time, which was very much a traditional Piaget atmosphere, and so that really very few other people were doing this kind of work, so that it became a little bit frustrating not having anybody to talk to about what I was doing.

Stanovich: This was the Department of Psychiatry at McMaster?

Siegel: Yes.

Stanovich: That was your position there. So you're again staying ahead of the Zeitgeist at every stage here is in part good planning, and in part the accident of ones life.

Siegel: I think it's mostly the accident of ones life.

Stanovich: Okay. Your interest in developmental disabilities has a number of different strands, which would be clear to anyone who actually looks at your Vita, but from my standpoint one of the most long-standing and programmatic aspects of your work has been the work on reading disability. Do you see that work as special in any way in the range of your interest in developmental disabilities?

Siegel: That work has become an area of concentration. I guess mostly because the acquisition of reading skills is so fascinating, it's such a complex skill, and it's quite fascinating to think of how it occurs. But I think that work is – part of it, one strand of it has been characterized by something which characterized all of the work in cognitive development, the anti-Piagetian, and the information processing strands, and that was the concept which came from post-Spence learning theory, which came from the philosophy of science

before it, in which I was trained. And that's the examination of operational definitions, and how operational definitions influenced the conclusions, the scientific conclusions. So the process, the scientific thinking in the development of scientific thinking really cuts across all of this. And, of course, when I started to study reading disabilities, I was overwhelmed, as everyone is, by the complexities of the definitional issues, and some of my colleagues moved blithely along not being concerned with these, but I thought we could never make progress until we resolved the definitional issues.

Stanovich: Now the interesting thing about some of your more, well-known papers on these definitional issues is that they impinge on both conceptual issues having to do with reading disabilities, but also the practical issue of educational and clinical classification. Were these two concerns always intertwined, or was there one or the other that was really the focus of your interest? And I'm referring here to some of your well-known papers on the role of I.Q. in reading disability classification.

Siegel: The practical in the general theoretical or conceptual issues were very much intertwined, because I started doing this work actually working in a clinic testing children who had possible learning disabilities. And I saw from the very beginning how critical it was to have functional definition that would be useful in the individual case and also acceptable to the school system.

Stanovich: Now you – in the mid 80's as you were working on all these problems, you had a change of affiliation and you accepted a professorship at the Ontario Institute for Studies in Education. Has that particular move from a department of psychiatry to a graduate institute of education and educational research had an effect on the directions that your work has taken?

Siegel: I think being in an environment where there are people who are very concerned with the educational issues has had an impact on the work. I think also, because I've been in a position where most of the students I teach, or at least the majority, are teachers, and then I've had to develop my thinking in the way to communicate with classroom teachers, or people who were in educational consultant roles in the school system. So that has, in some way, perhaps when I think about it, focused on the practical issues of assessment.

Stanovich: So here again is that bridge between the scientific and the practical. Now there's another bridge that one notices in your work that I wanted to ask you about. There are studies that you have done that are essentially case studies of what is known as deep dyslexia, on the one hand. On the other hand, you have done quite extensive large scale, large end, and multivariate studies of reading disabilities. People usually work in one or the other of these domains, but not both. How did that come about?

Siegel: Well, the deep dyslexia, which was a particularly interesting discovery, came about because I was doing a lot of the testing of the children, and saw children making these very unusual kinds of errors, which I had not really read about in the literature on normal reading, and really were quite interesting. So that was a phenomenon that occurred very infrequently, but was certainly worth studying, but I think I've always had an interest in individual cases, and I would say more generally, a finding to me is much more valid if not only it's statistically significant, but if most of the cases show it. So I would say, even in my work on the early detection of learning disabilities, I'm interested in the prediction for an individual child, and I'm interested in how many, or in how many cases actually could you say that a particular finding about. So I think to be valid, a finding has to be true for most of the cases in your study.

Stanovich: So it's that philosophy that's really driving the kind of eclecticism that one sees in your work?

Siegel: Yes.

Stanovich: We've kind of discussed this evolution in your work from the neo-Hullian, through information processing theory and developmental psychology, through developmental disabilities.

Could I ask you to project into the future, to extrapolate where do you think you may be going in the future?

Siegel: I've become increasingly interested in the biological basis of behavior, and hope in the future to be doing work on evoked responses, and the relationship of evoked responses to the performance on certain reading tasks.

Stanovich: So the trend toward cognitive neuropsychology is one that you would endorse?

Siegel: Yes.

Stanovich: Let me just – while we're on the specifics, let me ask you another specific thing. In your work on information processing correlates of reading disability, you have again made use of models from cognitive science, in particular the dual route theory of lexical access, but a major development within cognitive science has been the development of connectionist models of human performance. What implications do you see these as having for work on reading disability specifically, but then if you would like to comment more generally on the impact of these models for modeling developmental phenomenon?

Siegel: I think that I have a lot more to learn about these models. And again, it seems to me that the existing models can't really handle the complexities of reading, specifically the two independent processes, get the visual or direct access and the phonological route, and particularly some data that I collected recently, where reading disabled children appear to be superior in one route, and very deficient in another route. I think that the existing connectionist models can't really explain this, so I'm going to remain skeptical about the connectionist models.

Stanovich: But it is interesting, and, in fact, I'm aware of the finding that you have referred to, it's indeed an important one. It is interesting that a developmental finding such as this, I think, does have a lot of implications for these connectionist models. Let me ask you one of the hard questions that I always find difficult to answer, but, maybe you will or won't. Of all of the things that you've done over the years, can you point to one or a couple of papers or ideas, or theoretical developments that you see as your most important contributions?

Siegel: I think one important contribution, which was never recognized at the time, and probably never will be, is the work that I did on number concepts and trying to operationalize them into rule learning tasks instead of the traditional Piagetian verbally-based approach. But these tasks were certainly not popular at the time that I did them, and I occasionally see these ideas resurfacing. And what has been a little bit frustrating to me is the fact that these – my papers are not even cited, and I won't go into any specific examples, but I find that a little bit frustrating. I also think that the work that I did on the I.Q. was probably on the irrelevance of the I.Q. for the definition of learning disabilities, may become one of the most important contributions I've made, and at least it's made people think about the issue.

Stanovich: Let me pick up on both of those points, starting with the latter. Your I.Q. work has been controversial in many circles. Was that something that you would have predicted, or was that a surprise to you?

Siegel: I was surprised by the amount of opposition to it from some segments of the academic community. Certainly it has not been popular with people, professional psychologists, or some parent groups, and –

Stanovich: And that part you expected?

Siegel: Yes. But I didn't expect very reputable researchers in the area to be so antagonistic toward it. I would say that that's not been a universal reaction. Often the reaction is, well, of course, we knew this all along. What are you telling us that's new? On the other hand, you're absolutely wrong and you can't possibly be right.

Stanovich: And both reactions can't be right, right? You raised another issue with respect to your number conservation work, and that, for lack of a better term, we'll call it the 'a historical attitude of people who do research, the tendency for intellectual and empirical are precursors to be ignored when new, young assistant professors begin to do their work.' This is a phenomenon that many people have commented on, and you've seen it in your own case with your number conservation. What do you think the cause of this phenomena is, forgetting our history, discovery the wheel a new - -?

Siegel: Well, some of it is not basically sitting in the library. Some of it may be educational, in that we really don't stress the history of intellectual thought in any of our courses. I think it's rather rare for both undergraduate and graduate school where the historical aspects have been ignored in the curriculum area. And I'm sure that I also have been guilty of this on occasion, you know, just really simply not spending enough time in the library.

Stanovich: So, maybe ignoring the humanities side of psychology?

Siegel: Right. Now some of it in some cases, there may be something malicious to it that somebody wants to prove their own career, make believe their discovery is more novel. I think that that may be a relatively minor part of it. It may be just something, as I say that we all experience a certain kind of intellectual laziness.

Stanovich: You've actually been in a good position to see these trends, because you have extensive editorial experience within the field of developmental psychology, having served as associate editor of *Child Development*, and currently the editor of *The International Journal of Behavioral Development*. In those positions have you seen any publishing trends over the years that you would like to comment on or --?

Siegel: Now the two journals are very different. What I saw happen in *Child Development*, is that the pressures of space resulted in a lot of the interesting discussion, and perhaps, historical introduction being discouraged from it. As editor of *International Journal of Behavioral Development*, I tried very hard to allow authors sufficient space to discuss the historical, conceptual, methodological, and controversial issues. As a result, the articles are getting longer, and we may have too long a publication lag. But I think we need to be very careful about making sure that when we do publish something that the authors have sufficient space to explain what they need to say.

Stanovich: Now what do you feel though is the answer for a journal like *Child Development*, in between a rock and a hard place, in terms of the things that you've just thrown out. And namely there we have a journal with a ninety percent rejection rate, and with the space constraints that would not allow for the type of more contextualized report that you're talking about. Do you have any suggestions for the Society about what it does with CD?

Siegel: Well, first of all the rejection rate is really not that high, because a lot of the papers that are called 'rejected' are eventually resubmitted and accepted. I'm not sure exactly what those statistics would be. I think, when I was doing it, it may have been closer to a forty percent acceptance rate, if one looked at whether the manuscript was eventually accepted. Now we're not sure exactly, but I think those were the approximate statistics. But I think one thing is to really discourage as much as possible, piecemeal publication, and to encourage people to put studies together. Now unfortunately, there are tenure committees and grant committees to count the number of publications without looking at the quality of them. But I think that certainly would be a trend that I tried to encourage, not always successfully, in my editorial work of journals.

Stanovich: It also may help to publicize to younger people that the rejection rate isn't quite as high as the scuttlebutt has it. You just mentioned constraints of funding, and that's one thing I should ask you about, your experiences in the research funding apparatus over the years. Now in particular, you've been located in Canada, and have dealt with that funding apparatus there, but you are also

familiar with, and have interacted quite a bit with the funding agencies in the U.S. Are there any differences or similarities there that you would like to comment on?

Siegel: The Canadian system, that is the system of federal grant funding, is really quite different from the U.S. First of all, on Canadian grants, at least the federal grants, there's no overhead that's allowed.

Stanovich: A few Dean's would gulp at that in the States.

Siegel: But the total amounts awarded are much smaller, but the universities are expected to provide the secretarial help, for example, and for the most part they do reasonably well at that. But the real difference is in the kind of application that you need to write, and the way applications are judged and funded. In Canada, the agencies look at a combination of track record and the actual proposal. And the proposals themselves are no more than five or six pages, with sometimes one additional article permitted. This makes reviewing and writing them, in some ways much easier, and it also encourages people to publish their work, and, I think, it leads to very rational decisions about who was funded. Also, because of this policy, the success rate for Canadian grants, for example, on NSERC, which is the government agency that currently supports my research, is typically a sixty to sixty-five percent. Now, of course, applications are funded at a much lower level than in the U.S., but it seems to be adequate to do the research.

Stanovich: Wow! So that's a very high probability of acceptance. So, in essence, the research funding is spread around more evenly, a smaller funding but too many, many more people, and apparently less cost to put the applications together from what I hear you saying?

Siegel: And also it's typical now that the funding is given to the investigator in terms of a lump sum without specific requirements about line items, and that makes a difference in the way you actually can use the money. The philosophy behind this is that you can't always predict what direction your research is going to go in, and what you're going to want to spend money on. I think that's a very – that's led to perhaps more creativity in research.

Stanovich: Do you also think that the emphasis in Canada on the track record of the investigator cuts down on the emphasis on faddish topics, the topic of the week phenomena in grant writing?

Siegel: Yes. And I think it also helps people come to some sort of conclusion about their research, and to get it reviewed in the wider public arena. And it encourages people also to move in new directions, because you don't need to write a set program with all the details of your methodology when you write a grant application. In Canada most people wouldn't have room in the number of pages, so the idea is that you find a program of research, not a bunch of specific studies. And I think that leads to, if you have competent people, it leads to a less restricted science, and studies that are less faddish.

Stanovich: So there's a lot of discretion in the system is what I'm hearing?

Siegel: Yes.

Stanovich: We've discussed your role as associate editor of *Child Development*, but you've also had a large role in SRCD itself, in a variety of different guises, including the chair of the program committee of the conference, which is perhaps what we should start with here since we have the '93 conference upcoming. You chaired the program committee for the 1985 meeting in Toronto. Do you have any words to say about that particular experience?

Siegel: Well, it was fascinating. And when I look back on it, it was a lot of fun. I wouldn't have said that in the middle of it. And Joan Grusec, who was the local arrangements chair, and I here felt very strongly that it was important to have some professional staff doing it, because we learned the hard way about all of the details and all of the things one has to worry about, and we felt that without some professional staff we will be continuously repeating the same mistakes. So I think we can say, Joan and I can say, that we were responsible for convincing SRCD to switch to much greater participation of professional convention planners in the planning of the convention.

Stanovich: So that was really kind of an offshoot from your experience –

Siegel: Yes.

Stanovich: -- with the '85 Toronto meeting? Now you have played some other committee roles in SRCD. Could you describe those for us?

Siegel: Well, one was the finance committee, which I found particularly fascinating, and came away reassured that SRCD is watching its pennies and spending its money very carefully, and finances of the organization are very, very well looked after.

Stanovich: So the membership can rest assured?

Siegel: Yes. Yes. And it was really a pleasure to work on that committee. I'm also now a member of the public policy committee, the name of the committee has changed recently, so I'm not sure what its exact name is, but I found that to be quite interesting. It presents a dilemma for those of us who are not located in the U.S., because most of what that committee expressly deals with, are issues related to social policy in the United States. And I think that it's perhaps necessary to broaden the mandate of that committee to have a more international prospective.

Stanovich: An important recommendation. Picking up on that point, you play a leading role on the Canadian Psychological Association, you're a chair of the scientific affairs committee. How is that organization similar to, or different from SRCD?

Siegel: It's quite different from SRCD because it's a mix of people who might be called scientific psychologists, people who are practitioners, and people who combine both roles. And because it has these diverse functions and interests of the members, it is often much more difficult for it to function as unified.

Stanovich: With respect to SRCD, let's backup a minute here and say a few words about your history in the organization. Do you remember when you first joined, and what was the first conference that you ever attended, and what was your reaction to it?

Siegel: I joined in 1968. I got my degree in '67, and I joined in 1968. I did not go to the '69 or '71 meetings because I was about to have my first and second children at that point, so it was not a particularly good time –

Stanovich: A biannual children instead of –

Siegel: Yes. That's right. And they were both born in May of the year, so it was not a particularly good time to attend the meeting. The first one I went to was the Philadelphia meeting in 1973, and it was quite an interesting experience. It was in very, very crowded quarters. I guess there had been a big jump in the conference attendees, because it was in the east coast and there's a large population of student and faculty that could conveniently get to the location. So it was on one hand, very, very disorganized and crowded from the membership point of view, but it was also an opportunity to meet people. And I had spent those years, most of the time staying home raising children, so it was quite a wonderful experience to get out in the professional world again.

Stanovich: Many organizations are changing or experimenting with different formats for their conferences, AERA especially is trying different things, round tables, etc. Do you think SRCD needs to do more experimentation along those lines, or do you like the conference as it is?

Siegel: I liked the idea when we went to posters rather than paper sessions, except for the symposium, because I found that the paper sessions were often somewhat disjointed, because one could not always arrange to have papers on similar topics at the same time. So I think the posters are a very, very good idea. I would like to see more discussion and less presenting of data, more discussion of people with similar

interests in a more of an open-ended way. I would like to see more discussion of issues of the workings of the organizations that the members could participate in.

Stanovich: Are you satisfied with meeting every other year, or can you conceive of an SRCD that had a meeting yearly?

Siegel: Well, I also wanted to have the meeting yearly, and, of course, the staff would groan at this because it's a tremendous undertaking. If something like that were done it really would have to be a professionally managed convention. And I think all of us would like to see a smaller convention, which, of course, would be very difficult to achieve, because certainly you don't want to exclude anybody. So perhaps there might be some way of making it more topical, and every other year having – or every year having emphasis on certain topics. If we went to an annual format, perhaps we could do that, for example, perhaps social development one year, cognitive development another year, but then there are people who would interface at both of those, I'm interested in both of those, so that wouldn't work. So I'm really not sure how I would set it up, but I think perhaps we ought to start thinking about some creative ways to do that.

Stanovich: Let's also touch on some comments with regard to the field as a whole. Now we've done that a little bit. I've gotten some reactions from you on trends. You've given us some comments on trends such as neuropsychology and connectionism. Are there any other general trends in the field, from a framework point of view, that you'd like to comment on, or just from a sociological view that you'd like to comment on?

Siegel: One of the things that I would like to see happen, is that we get a more international perspective, that we understand development in a variety of cultural and international contexts, and that we encourage our colleagues outside of North America to be much more a part of our organization. This is difficult for a variety of reasons, among them, linguistic and the very high costs of travel, of international travel. But, I do think it's a shame that we don't really have much more contact with people all over the world who are doing research in child development. And if there was one trend, I would like to see both the field and the organization be much more aware of what is going on in the rest of the world.

Stanovich: Is that a contact that you find you get more from ISSBD than you do from SRCD?

Siegel: Yes, I think that's true. I still think that it will have a positive effect on the research that we were all doing in child development if we had contacts, more real contacts with people in other parts of the world.

Stanovich: And so you do a lot of that in your role as editor of IJBD, and so the general message I'm hearing here is that it's worth the effort to -- the extra effort it takes to make the contacts with our international colleagues and to deal with some of the language barriers?

Siegel: Yes, I think that's true.

Stanovich: We've focused on research concerns and society concerns here, but you, of course, have been involved in the teaching and training of child development researchers for a couple of decades now. Could you give us some comments on what roles that you've played and your thoughts about trends in training of developmental psychologists? (End Side 1A)

Siegel: I think we need to give our students of developmental psychology much more experience in the field, and give them much more practical experience dealing with real issues, because otherwise I think the research tends to be very esoteric. That has certainly changed or influenced my thinking about research, and in particular the teaching that I did in the medical school made me realize that it was much easier to talk about my research, and the field of child development in general, if I could relate it to some practical issues. So I would say that has characterized, I think the way I think we should be teaching individuals the concepts of child development.

Stanovich: In teaching in the medical school setting, did you find any prejudice against the works of psychologists?

Siegel: Actually, not very much, because the McMaster Medical School was a new medical school and it really developed with a different philosophy, it was a philosophy where behavioral sciences were highly valued. And in most – at least theoretically, and for the most part that was true, so I generally got a very good reception for the material that I published in *Child Development*.

Stanovich: So you didn't have to fight to be recognized in that setting?

Siegel: Not at all.

Stanovich: One issue in SRCD that I passed over was your role on the Governing Council. Do you have anything you can tell us about that?

Siegel: Well, it certainly was a fascinating experience to work on the Governing Council and see the inter-workings of the organization. And one of the things that impressed me the most was how hard some of the people worked, and how little compensation got for it. I'd particularly like to single out three individuals: Dorothy Eichorn, who just absolutely did a superb job in her role as executive director. She really did a wonderful job. And I would say the same thing for John Hagen, who was filling that role currently, how I was impressed how helpful they were and how hard they worked for the good of the organization. I also encountered Barbara Kahn, who functioned in the secretarial membership kind of a role, and she is really quite amazing. Most SRCD members don't know about the role that she had, but she works with the press making sure that the journal and monograph get published properly. She keeps the membership information. And to each Governing Council meeting she used to bring a large computer printout, which would be a list of all the members. And as we discussed -- there's committee memberships or whatever, and the issue came up about whether somebody was a member, Barbara, without consulting her printout, used to be able to know not only whether somebody was a member, how long they've been a member, and often their address and phone number and the correct spelling of their name, without looking in this printout of four thousand names. So she functioned as a computer. We always used to tease her about why she bothered to bring this heavy computer printout along when everything –

Stanovich: Was in her head.

Siegel: -- was in her head. Yes. One of the wishes I had for the Governing Council, that there was an opportunity for more younger people to become involved, and that also people who are at a smaller, less well-known institutions would also have more of a chance to be involved in the committee structure, or in the Governing Council itself. So this is the one wish that I would have, that the structure of the organization, that they look very carefully at their policies in this respect. But, it was a wonderful experience to work with my colleagues at SRCD.