

## **Keith E. Stanovich**

- Born: 12/13/1950
- Spouse: Paula Stanovich
- B.A. in Psychology (1973) The Ohio State University, M.A. Psychology (1975) University of Michigan, Ph.D. Psychology (1977) University of Michigan



### **Major Employment:**

- Oakland University – 1987-1991, Professor of Psychology and Education
- University of Toronto – 1991-Present, Professor, Department of Human Development and Applied Psychology and Member of Centre for Applied Cognitive Science
- University of Toronto – 1996-2001, Program Head, Department of Human Development and Applied Psychology and Canada Research Chair of Applied Cognitive Science – 2002-2005

### **Major Areas of Work:**

- Psychology of reading and reasoning

### **SRCD Affiliation:**

- Member since 1997

## **SRCD ORAL HISTORY INTERVIEW**

**Keith E. Stanovich**  
University of Toronto

Interviewed by Richard F. West  
James Madison University  
In Portland, Oregon  
July 13, 2008

Stanovich: This is the SRCD oral history interview of Keith E. Stanovich of the University of Toronto, interviewed by Richard F. West of James Madison University on July 13<sup>th</sup>, 2008. This is Keith Stanovich speaking. Dr. West will be asking the questions as the interviewer.

**West: What adult experiences were important to your intellectual development? For example, what are the origins of your interest in child development and what individuals were important to your intellectual development?**

Stanovich: Well, in terms of individuals, again, there's a reason you're doing this interview. The key thing about my intellectual history is--to understand about it is that it has been characterized by very long term collaborations, including the one with Richard West and I that had its origins in 1974 when we were graduate students at the University of Michigan and conjoined our interests to study the psychology of reading, but in particular, reading acquisition and the development of reading skills.

Richard was a student in the developmental psych graduate program, and I was a student in the experimental psych graduate program, but I was TAing a course for Professor Lorraine Nadelman in the developmental area and preparing an experiment on the Stroop Test, which at the time had been reintroduced into psychology as a measure of automaticity of processing. And Richard passed by a graduate TA room in 1974, noticed the test and we were off and running from there.

Both of us were interested in reading processes. Richard had attended an SRCD workshop in reading over the summer and met a number of people in the field, and then I was coming in from experimental psychology with an information processing perspective. And at the time there were a series of very exciting publications in reading and reading development, Frank Smith's book, *Understanding Reading*, LaBerge's and Samuel's important paper on the automaticity of reading, Phil Goff's important paper, *One Second of Reading* where theorists were beginning to apply information processing concepts to the study of reading development. Richard and I began collaborating and eventually our first major paper together was a test of a developmental prediction of the leading so-called top down theories of the reading process—the theory promulgated by Frank Smith in the book that I mentioned, *Understanding Reading*. And we tested a very critical prediction from that book that good readers got to be good readers through reliance on contextual information, particularly they acquired their word recognition skills.

Again, one of our important contributions was to show the importance of differentiating word recognition from reading comprehension, so I'm talking about word recognition here. And an important prediction from this theory, from Smith's theory and most other top down theories, was that contextual effects on word recognition would be larger at more advanced stages of development. And Richard and I found a way to test this hypothesis with some information processing techniques, priming techniques from experimental psychology and the surprising thing of this particular study was that its outcome was exactly the opposite of the prediction of the top down theorists. In fact, it was the beginning readers, the younger children, who were more reliant on contextual information. This led to our first notable publication and it was an SRCD publication (published in *Child Development*), West and Stanovich 1978, was one of our earliest publications, but certainly our earliest publication that got reasonable attention.

Later on when Richard and I had a 15 to 20 year collaboration in the area of reading and later on when our laboratory, the Stanovich and West laboratory, began to change directions and look at some other topics in the area of reasoning Richard and I, again, continued in this new area. When I describe or answer some other questions in this interview I'll mention a couple of other long-term collaborations, although none as long as this one.

**West: Would you characterize the development of your ideas in the field of child development as evolving in a rather straightforward fashion, or in a way that involved sharp turns in theoretical views or research science?**

Stanovich: Yes. The sharpest turn in my research program over this 30, 35 year period has been in content rather than style. I have mentioned a research program in reading, which went pretty intensely from 1974, when Richard West and I first met each other, until roughly the mid '90s, 1995, 20 years or so. In about the year 1990 Richard and I began to develop some of the interests we had long had in reasoning, human reasoning, the development of reasoning, rational thought and the development of rational thought. We had long been--and by long been I mean way back to the 1970s--we had long been fans of Kahneman and Tversky's heuristics and biases research tradition. We were readers and fans of the original papers of that tradition that came out in the early to mid '70s. We often talked about that work, but for many years during our intense work on reading development we didn't actually turn that interest into a research topic in our lab.

In about 1990 we began to do exactly that. We were influenced by several theorists at the time writing on reasoning, most notably Jonathan Baron and Jonathan Evans, Baron in the United States and Evans reflecting the reasoning tradition of Great Britain. And I had a sabbatical in the winter of 1991 at Cambridge at St. John's College with Usha Goswami, a leading figure in models of reading development. And although, again, the reading interest was the rationale for the sabbatical, actually during the sabbatical I did a lot of work reorienting our research program to actually turn some of our long-term dinner table cocktail hour conversations about reasoning and rationality into a real research program. Richard visited me during the 1991 sabbatical and we started to develop some experimental measures of informal reasoning, measures that would tap peoples' ability to throw off their prior beliefs and their prior biases about a particular issue and reason according to the evidence. That's a core skill of many research traditions in critical thinking. Certainly when people write about the development of critical thinking--of how we want to facilitate the thinking of children as they proceed through school this ability to reason independently of biases, independent of your own situation, to take another person's perspective looms absolutely in the first rank of any critical thinking skill. Yet, the measures of this particular mental ability, including standardized published tests of it, are quite crude and not well worked out, and also not well integrated with the rest of cognitive science.

Here's a bit of a parallel here in the two research domains that I've worked in, in that Richard and I served the same function in the critical thinking/reasoning area that we served in the early history of reading, and that is to sharpen up very global and imprecise theoretical concepts by importing from cognitive psychology more precise theory and more precise methods so the automaticity theory, priming theory, measures of automaticity that we brought in. The priming and semantic association and semantic priming from information processing that we brought into the field in the '70s had that sharpening function. We feel that we provided somewhat of the same function in the area of the development of reasoning in the 1990s. This skill, reasoning from another's perspective or throwing off one's one prior opinion or prior bias that looms so large in critical thinking had not been integrated with more precise methods and theory from cognitive science. And so I see, although those content areas in which we've done our research, the development of reading and the development of reasoning, are quite different, I see some great continuities in how we've approached the area.

**West: What were your primary interests in child development at the beginning of your career?**

Stanovich: Well, I think we've pretty much covered that one in the earlier questions. It was very much conditioned in an applied direction in the sense--not that I did direct applied work on schools or that type of thing, but it was directed toward a problem, namely reading acquisition, that Richard and I thought would cash out wherein important school based developments that could facilitate children's learning. So although our topics were basic research topics, they were the type of topics where you didn't have to look too far to see that they had immense potential for practical application. And with somewhat less force, the same is true of our work in reasoning in that the now well developed area of behavioral finance is going strongly now in 2008. But this was less true when we entered the field in 1990, that the field of behavioral finances, making use of all of the research tradition that Richard and I have been contributing to, that is the heuristics and biases research tradition started by Kahneman and Tversky.

**West: Would you like to elaborate on the continuities in your work and which ones are significant and what shifts have occurred?**

Stanovich: Well, I think --yeah. I think pretty much we've covered that in the previous question.

**West: Would you reflect on the strengths and weaknesses of your research and theoretical contributions, and the impact of your work and its current status?**

Stanovich: Well, impact--very interesting topic--the study of reading acquisition and models of how children acquire reading skills is one of the great success stories of developmental psychology. We have many, many important areas of interest in developmental psychology that have been opened up. We can fill textbooks with fascinating experiments, including lots of experiments that make for great videos. But I don't think that there's a single area of developmental psychology that has cashed out in such a direct impact on public policy.

And so let me fill that in just a little bit from my earlier answer. Reading education throughout the 1960s, '70s and into the 1980s was dominated by the so-called top down models of the reading process, the ones I mentioned earlier promulgated in Frank Smith's important book, *Understanding Reading*, and then kind of parallel writings by Kenneth Goodman. And again, the model de-emphasized the importance of children learning the spelling to sound code, de-emphasized the learning of alphabetic coding and the grapheme to phoning mappings, i.e., what the public would call phonics, and instead emphasized pretty much every factor but phonological coding. The importance of the factor that Richard and I studied in the much cited West and Stanovich 1978 paper--context--was that good readers were supposed to be good readers because they had skill at using contextual information, guessing about upcoming words and so forth. And these models had a direct influence and impact on reading curriculum. Our work is in the tradition of really seminal studies by many people, many of them developmental psychologists in this very organization, people like Charles Perfetti and Linnea Ehri that demonstrated that these global theories of the reading process and their implications for development were drastically inaccurate.

It turns out that there's no more important process in reading acquisition than alphabetic coding. Grapheme to phoneme conversion takes place with all readers, both novice and expert. The expert reader has automatize the process to such an extent that indeed it may seem like he's skimming and scanning, but that is simply the result of expert performance in automaticity at grapheme to phoneme coding, i.e., the experimental work by developmental

psychologists in the period 1975 to roughly 1990 came down solidly on the phonics side of the so-called great debate in reading methods. In educational theory the term great debate was coined back in the 1960s with the famous book by Jean Chall that juxtaposed what was then called the whole word method with phonics. The whole word method via the work of people like Smith and Ken Goodman evolved into the so-called whole language method and movement--more a movement than a method--in the 1980s and carried on into the 1990s. It dominated educational curriculum at ed schools and teaching--were it not for the increasing influence of the research tradition that I mentioned. Let me break this up a bit into historical periods. Roughly 1975 to 1985 Richard and I attended SRCD conferences where there were one or two reading symposia, never a lot, but nonetheless, these conferences and others let a core of us start to develop a basic research science of reading acquisition.

But during this period I'm referring to, '75 to '85, this period of work very little of this seeped into actual reading practice and, of course, sometimes to our frustration. Another leading figure, by the way, that worked with us during this period and was later to become one of my colleagues was Linda Siegel who also will have an interview in this oral history project. She can tell the story from her perspective, but I'll come back to Linda in a minute or two. Indeed since I've marked the period from '75 to '85, the '85 SRCD meeting was in Toronto, and I believe Linda was the program chair. And that was one of those meetings where we gathered in the Library Bar of the Royal York Hotel. I forget who all the cast of characters were, probably Linda, Linnea Ehri, George McConkey, Dave Zola, Richard and I, Thomas Carr and others, probably Chuck Perfetti and Rod Barron, and talked about the state of reading in SRCD in particular but in the field as a whole and, again, how work on context effects, how work on orthographic and phonological processing was coming down strongly on the phonics side of the great debate, and yet at the time little of this had seeped into public or educational consciousness.

That was to change greatly in the period from roughly 1985 to 2000. First of all, there was an explosion of research and research interest in the development of reading mostly concretely exemplified by the emergence of the organization SSSR, the Society for the Scientific Study of Reading, an organization that Ron Carver had a major role in getting off the ground for our field. Interestingly enough for this history, the emergence of another scientifically based organization made it less necessary for us to rely on SRCD as a meeting place. But prior to the emergence of SSSR in 1994 SRCD was an absolutely central meeting place for this core group of researchers.

In any case, back to this history, there was an emergence of a large core of people interested in reading acquisition where in 1979 we could have almost had the whole field in one symposium or at least in one barroom after the symposium. SSSR now has meetings with four or five hundred people and that reflects the explosion in the field. With a much larger core of people we had people who really were at this central interface between research and schools and teachers and curricula, people like Barbara Foreman in Houston and many like Barbara and Marilyn Adams and Anne Cunningham, who really worked with putting the insights from the basic research into action. And as the late '90s and the early 2000s were on many people from this early SRCD tradition took leading front line roles, and now I'm talking about not just research, but in dissemination of this information directly to teachers. And a major figure in this work would be Joe Torgesen at Florida State University and the great resources that he has captured in his website and all of his dissemination activities and many of the NIH projects also that were spawned in the 1990s have major dissemination roles, but none greater than what Joe, with some collaboration from Richard Wagner, another SRCD member, none more important than their contribution.

With regard to some of my own involvement in these practical dissemination activities, and here I think I'm seeing from my interviewer that I'm anticipating some later questions and that will save him some effort, but in regard to that, I was lured out into the field much more in the 1990s. In particular, I received repeated invitations from the International Reading Association, a meeting very unlike SRCD in that IRA is not dominated by researchers, but instead is dominated by teachers and practitioners. And that presented an interesting moral dilemma for me and, I will admit, a good degree of personal stress. Richard and I had been drawn to the basic research problem of reading because we thought that it would eventually trickle down and effect actual practice. But I think when he and I were graduate students and in the early days when we were thinking of that we were thinking somewhat of a trickle down model, but here in the early 1990s because of a couple of papers that I had written--and Richard will probably ask me about those at a later part of the interview--I was getting invitations and I don't know whether to continue this metaphor, but they were asking me to take the can and pour the water directly instead of just letting it trickle. People wanted me to disseminate directly to teachers and to practitioners, to not just let the research speak for itself--throw our research out there into a journal and let it trickle, but for me personally to go and to disseminate, and that created actually quite a bit of stress. Of course, it's the paradox of success. This I'm sure happens to a lot of people.

But your success is so way beyond what you had ever thought it would be that you are asked to do things that you never thought you'd be asked to do. And these IRA talks are an example, because of course you don't just go to IRA and give a research based talk with a bunch of regressions and then get on your plane and leave. Both components of the talk are utterly different. First of all, you can't go and talk about your regressions. You have to go and talk directly about what this means for a teacher. And that's not really the way that Richard and I had been oriented with our--we are basic scientists. We were basic scientists from the get-go in 1974 and here sitting here together in 2008 we're still basic scientists. But here come these invitations asking me to do something very different, but incredibly socially useful, yet something I'd never planned on nor trained for in any way. And so stage one is you have to give a different type of talk, and then stage two is you don't just answer a couple of questions about regressions and then run off to the airport after these things. You are inundated by teachers with very specific questions, some of them about individual children, and turning them away with a standard research phrase, "More research is needed," is simply not something that you can do in a situation like that when you've stepped down from your symposium and 30 teachers are clustering around you. And so although it was very heartwarming to go and give many of these talks, I spoke to IRA several times, many of the state IRA organizations, including those in Michigan and Ontario. I actually found these talks quite stressful but hopefully they served a dissemination purpose.

Much more up my alley in terms of direct applied impact was a document I wrote with my wife in 2001 for the Department of Education, *Reason and Research in Reading Instruction*. And again, further I will contextualize Paula Stanovich in a later question. But that was a document we were asked to write that explained what scientific research was for a teacher audience. By 2001 the importance of experimentally--sorry, I should say more accurately scientifically demonstrated methods of reading instruction had pretty much won the day and yet state boards of education and of course the federal government with the No Child Left Behind Act was concerned with the issue of conveying to practitioners just what scientific validation did mean. And hence we were asked to write this document for the Department of Education. There were many reviewers of our document, but again, another SRCD member, Valerie Reyna, gave important input in its construction. And it was published in 2003 and it was used in some of the No Child Left Behind research documents, those for instance for Reading First and Early Reading First as an exemplar of the definition of what the federal legislation called SBRR, scientifically based reading research. That type of contribution was more up my alley, because it involved the writing skills and document preparation skills. So I spent the bulk of my time here discussing the practical implications of the reading work, and just briefly I'll say that there are, as I have mentioned, implications of the reasoning work. But those have been quite multifarious, but I've already mentioned the area of behavior finance.

**West: Do you have additional papers that you believe best represent your thinking about child development?**

Stanovich: Well, here I'll mention the specifics of the papers that I referred to earlier. In the area of reading there are two theoretical papers that I put together that took the empirical research of the Stanovich and West reading lab and put them in a conceptual framework. The first of these papers was the so-called *Interactive Compensatory* paper, a paper in the journal *Reading Research Quarterly* published in 1980 where I put forth the so-called interactive compensatory model of reading--a model to supersede the top down models that I had mentioned earlier and one that gave proper recognition to the importance of spelling to sound coding skills but also did not deny that there were many, many contextual facts involved in reading and word recognition. But the work of Richard and I showed that those contextual effects, although they were involved in reading in terms of individual differences, went in somewhat different directions than people had originally thought.

Now it is interesting, by the way, that this paper probably had so much impact, because I published it in the *RRQ*. This is a journal of the International Reading Association, the organization that I mentioned earlier, that I published it there and not somewhere like *Psych Bulletin* or *Child Development*, or some other journal. It could have gone in many of those sources if appropriately trimmed. The paper is a bit long like many of my products. But it probably vastly increased its impact, because it spoke to a much wider audience by being published in an IRA journal than being published in an APA or even an SRCD journal.

The second paper was the so-called Matthew Effects Paper, again, published in *RRQ* in 1986. And that's a paper that contains a developmental model of the acquisition of reading, but specifically how the development of reading acts to facilitate or impede other cognitive processes, including those that we often associate with intelligence. And the term Matthew Effects refers to the biblical passage about the rich getting rich and poor getting poorer and it,

again, was a developmental model that not only affected theory--there have been many experimental tests of the developmental model there published in both *Child Development* and *Developmental Psychology* by people like Bennett and Sally Shaywitz and also Janwillem Bast and Pieter Reitsma from the Netherlands. And so as far as reading goes what those two theoretical pieces did was to take--and I'm just guessing here of how many there were, but six to ten empirical papers that West and I had published but put a theoretical framework on them.

And then in many of the important papers from both my collaboration with Richard and with some of my other collaborators, most specifically Anne Cunningham at University of California, Berkeley, and Linda Siegel, who was my colleague at the University of Toronto now at the University of British Columbia, many of those papers are collected together and summarized in a book I published with Guilford Press in the year 2000. In the reasoning area, again, a seminal contribution is the paper that Richard and I published in *Behavioral and Brain Sciences* in the year 2000, and again--and of course, our responses to the many, many commentators on the BBS article. And again, that piece summarizes a decade's worth of research on reasoning and rational thinking, but puts it in a theoretical context.

Much of the same function in the reasoning area is served by my books *Who is Rational*, which I published with Erlbaum in 1999, and the *The Robot's Rebellion*, published with Chicago in 2004. Richard and I continued to do work on the relationship between rationality and intelligence, and much of that work will appear in two upcoming volumes. One is by Oxford where we're developing a triprocess model of reasoning, and then another is a book for a wider audience that will soon be out with Yale on the issue of rationality and intelligence, and specifically the issue that intelligence tests miss a panoply of cognitive processes that relate to rational thought.

**West: Could you reflect on your experiences with the research funding apparatus over the years? In which institutions have you worked, the dates and contributions?**

Stanovich: Yes. Let me give a little bit of the chronology so I can set the context of the funding question. After leaving Michigan with my PhD I taught at Oakland University in the state of Michigan from 1977 to 1991 going through the ranks there, and in 1991 I moved to the Ontario Institute for the Studies of Education at the University of Toronto, where I remain to this day. And so I wanted to get that chronology in before answering the funding question that Richard asked me, because it then makes it clear that I have experience with funding in two different countries. Richard and I had NSF grants in the 1980s, and although we did not hold an NIH grant before I left for Canada, I made some NIH submissions. You can record here "laughter." When you transcribe this put in "laughter." And we have experience submitting to NIH, but also I was on some NIH study sections and so on, and I've mentioned before that NIH funding has been important in the history of reading research.

Upon going to Canada I became familiar with three different research organs, actually I would best say four. What in Canada we call SSHRC, the Social Sciences and Humanities Research Council, NSERC, the Natural Sciences and Engineering Research Council, the Medical Research Council, from which I did not get direct funding, but did much reviewing for, and then finally CRC funding, that is, I held a chaired position at the University of Toronto in the Canada Research Chair's Program and it has a separate and different funding source from the other three. And I must definitely say that after experience in both countries I much prefer the Canadian system. And it's not just because it is from an administrative sense a bit more stripped down. The grants are a bit shorter. But really the primary reason is the slightly different logic of the Canadian system, and this is particularly true of SSHRC. That is probably the heaviest funder of developmental psychology, also true of NSERC, and that is the Canadian system depends much more on track record, past track record, than the U.S. system and weights less heavily a promissory note, a fictional narrative of how one will create the Nobel Prize in the future. And you can record in the transcript "sarcasm," which would be accurately reflecting my opinion.

This is another case where, as social scientists, we do not seem to believe in our own findings and data. We don't believe in our own science. This is actually a general aspect of our science that has long disturbed me and I'm digressing a bit here. I promise I will get back to funding. We of course have done seminal work on the unreliability of interviews, yet go to your faculty and suggest that you put that into action when you have your next faculty slot and people will look at you oddly. In the area of funding we know a fundamental fact of human behavior in all domains from the cognitive, to the affective, to the motor domain is that past performance is the best predictor of future performance. And here we have a granting system that doesn't seem to reflect that--that is, a granting system in the U.S. that doesn't seem to adequately reflect that, because the Canadian system does. The

Canadian system, in putting much more weight on past performance, doesn't create the artificial proposals where one feels that one has to aggrandize the work to pump it up, to emphasize its practical import. In a Canadian grant proposal much more weight can be put on the solid contributions made in the past and the likelihood that those contributions would continue. And so for all of those reasons I much prefer the Canadian system.

**West: Do you have anything you would like to add in your description of your experiences in applied child development research?**

Stanovich: Yes, I think I pretty much covered that by giving the history of reading research and its frontline contributions to reading curricula and the teaching of reading.

**West: Another question that you've also possibly already finished answering has to do with when did you join SRCD, what were your earliest contacts with the Society, and especially your first biannual meeting?**

Stanovich: Yes. The first biannual meeting, Richard, you attended SRCD before I ever did in at least the 1977 conference you attended, and maybe before. My first attendance was in the 1979 conference. I believe I joined in '77 and first attended a conference in '79 and that was the San Francisco conference. Richard and I had both left Michigan in 1977, me, as I mentioned, to Oakland University in Michigan, and Richard to James Madison University in Harrisonburg, Virginia. And then we began our long tradition of meeting together to work and plan future experiments. Keep in mind you younger SRCD members who are reading or hearing this that this is before the age of fax. There was no fax. There were no cell phones. There was no email. And phone calling was relatively expensive. Fortunately Richard's University had a Watts line that helped us greatly, but no electronic communication, and so touching base at all of those meetings was very important. And so Richard and I had begun that tradition a couple of years before.

We left Michigan in '77, but I quickly attended a Psychonomics meeting in November of '77 in Washington and then Richard, of course, being in Harrisonburg, came up and I believe that's where we first met Chuck Perfetti, one of the SRCD's leading members in the reading area. And so the San Francisco meeting we did I think important planning for the early work in reading. And actually there was another anecdote where I could maybe segue here to bring in another colleague. At the SRCD meeting we presented some of our early context work showing that contextual deficiencies weren't the source of the reading problems of children that had difficulty. And at that meeting an old friend of Richard's, George Marsh, was there and I believe he directed the question to Richard, but I overheard it, and it was with a little frustration or maybe a provocative challenging George said to Rich, "Okay. Then if it's not context what is it?" And that actually led to a section--this is 1979 remember. At the time I was working on the interactive compensatory model, which was pretty much about models and context effects. And because of Marsh's provocative question or rhetorical question to Rich I added a short section onto the Interactive Compensatory paper at the end called Where are the Differences? And of course, I mentioned the emerging work in the area of phonological awareness. I say emerging, because in 1979 the famous paper by Bradley and Bryant was just one year old and not known to many reading researchers. Peter Bryant and Lynette Bradley, and Peter I know has been a regular attender of SRCD, published their seminal paper on the importance of phonological awareness in the extremely prestigious journal *Nature*, but prestigious or no, lots of developmental scientists didn't know of it.

And so in 1979 we were very early in the history of the importance of phonological awareness, of course. Bob Calfee had published some very important work, and Gene Chall and the important paper by Isabelle Lieberman had come out. And so I included a little section about the emerging work on phonological awareness, but at the same time pledged to myself that we really had to do something that was in a sense a bit more positive. We kept showing that context didn't operate the way people thought it did, but Marsh's question was a good one. Okay, well now do something positive. Now show us where the differences are.

And the investigation into those factors is the important contextual setting for my second longest research collaboration and that's the one with Anne Cunningham, now at the University of Berkeley. Anne was a teacher working in Clarkston, Michigan, and walked into one of the first statistics courses--the very first statistics course I taught at Oakland University in 1977. And Anne was looking to build on her teaching experience, and become a developmental psychologist, and do research work on applied and developmental psychology, and had come to Oakland and did her masters in developmental psychology with me, and then did her PhD at the University of Michigan, jointly supervised by myself and Scott Paris, another SRCD member.

And in the early 1980s then Anne, because she was in the classroom, and I conducted some important studies on phonological awareness. I have characterized us as being kind of the third wave of research in phonological awareness kind of nailing down this important process in early reading. And a paper I'll point to there published in the *Journal of Experimental Child Psychology* in 1984 by Anne and another masters student, Barbara Cramer, on the importance of phonological awareness. Anne and I continued to collaborate, not only in that area, but in another area of research that linked now all three of us, that is, myself, Anne and Rich, and that is in the area of the cognitive consequences of literacy where we developed some innovative measures of the degree to which people had been exposed to print and then we linked people's amount of print exposure to a variety of reading related cognitive processes. And we've done in that area developmental studies and also studies of fluent adults and there are several papers there where the three of us have worked together.

And then, although I don't think it's directly in the lineage of this question, I'll just mention the third of my longer term collaborations and that's with Linda Siegel, who had a major role along with Anne Jordan and Merle Walstrom in bringing me to the University of Toronto and the Ontario Institute. In 1991 Anne, Linda and I went on to collaborate on several studies, but by far the most notable one was only published in 1994 in the *Journal of Educational Psychology*, not only helping to establish what was then called the phonological core model of the reading difficulties of children with reading disability, but also one of the major issues that Linda and I worked on jointly, and that is discrepancy definitions of reading disability specifically showing that discrepancy definitions, that is, definitions based on differences between reading and intelligence, did not play the role that clinicians and practitioners thought they did, that defining reading disability on the basis of reading IQ discrepancies is totally wrong headed both from a theoretical and a practical educational point of view. And Linda and I had many productive years at OISE and the University of Toronto until Linda left for the University of British Columbia in the late '1990s.

**West: Okay. Do you have anything additionally you would like to add about your personal associations as they had a bearing on your scientific interests and contributions?**

Stanovich: Well, I'll just finish this by mentioning someone who was briefly mentioned earlier, but has set a wider context for this entire thing, and that is my wife, Paula Stanovich. Paula has been a professional in the field of special education since the year 1973 and she had a 19-year teaching career, the largest part of which was in a class that was then in the 1970s called Multicap, a self contained classroom that contained children that had both intellectual disabilities conjoined with a physical disability. And working in a self contained classroom in a self contained school fueled Paula's philosophy that we don't want self contained schools, that fueled her lifelong quest for inclusive education, the including of children with disabilities in the normal and mainstream educational facilities. Paula also taught in a learning disabilities resource room at the high school level in Huron High School in Ann Arbor for a few years. This was interrupted in 1991 when we went to Toronto and Paula, who had gotten her bachelor's degree from Ohio State and her master's degree from Oakland University, enrolled in a PhD program in the special ed department at the Ontario Institute for Studies in Education and received her PhD under Anne Jordan, who she continues to collaborate with, in the year 1994. And then Paula taught in the faculty of education at the University of Toronto and received tenure there in 1999.

Paula's influence on my work goes back all the way to the beginning, including what to an outsider would have seemed a very odd product I produced as a graduate student. Indeed the product seemed odd to all my faculty supervisors as well and it was my third year paper as a graduate student. Recall from my earlier description of the collaboration of Rich and I that Richard came from the developmental side, the developmental graduate program, but I was from the experimental psychology side. Nonetheless, I did a third year paper on the information processing differences of mentally retarded individuals. And I reviewed the literature on every experimental or information processing task I could find at the time, which would have been 1976 I believe, every information processing task I could find that had ever been run on, again, what was then called mentally retarded individuals, what we would now call an intellectual disability. And so I did my third year review paper on that topic, which all of my faculty advisors found extremely weird. And I thought I had gotten my revenge on them when I waited about 18 months and succeeded in getting the paper published in one of the most prestigious journals in mental retardation research, the *International Review of Research in Mental Retardation, IRRMR*. When I announced it to these same faculty members they still dismissed it as unimportant because it wasn't important experimental psychology, which again, given that this is an historical interview, it's actually a very important anecdote.



We can see the incredibly low status of individual differences in the field of experimental psychology when we look at the fate of my third year paper, at least with those people. But of course, the writing of that paper was entirely motivated by Paula and her work with intellectually disabled individuals and then her work in the classroom and prodding both Rich and I and Anne to make sure that our research in reading was disseminated, and then directly collaborating with me when she was at the University of Toronto on several papers that were directed at practitioners, a couple that we published jointly in the *Journal of Learning Disabilities* and in the University of Toronto's magazine for teachers called *Orbit*.

But then our kind of magnum opus was the Department of Education paper with her as first author on trying to disseminate to teachers how to distinguish science from pseudoscience. So Paula has provided the framework for all of my life, both my personal and emotional life, but my professional life as well. As I mentioned in the dedication of one book, she is everything to me and this explains a little bit more explicitly that dedication. Richard?

**West: Thank you very much for the interview.**

Stanovich: Thank you, Rich.

**End of Interview**