

## **Mildred Templin**

- Born 8/30/1913 in Milwaukee, WI; Deceased 10/22/2008
- B.A. (1936) University of Wisconsin, M.S.A. (1937) University of Wisconsin, Ph.D. (1947) University of Minnesota

### **Major Employment:**

- University of Minnesota – 1958-1976, Professor at the Institute of Child Development
- University of Minnesota – 1965-1976, Professor of Communication Disorders
- University of Minnesota – 1976, Professor Emerita



### **Major Area of Work**

- Language Development

### **SRCD Affiliation:**

- Editor of Monographs (1964-70) and Consulting Editor (1972-74)

## **SRCD ORAL HISTORY INTERVIEW**

### **Mildred Templin**

Interviewed by Shirley Moore  
At the Institute of Child Development, University of Minnesota  
October 5, 1994

**Moore: This is an interview with Dr. Mildred Templin, professor emerita from the Institute of Child Development at the University of Minnesota. It's an SRCD Historic Record interview. The interviewer is Dr. Shirley Moore. I'm also professor emerita from the Institute of Child Development at the University of Minnesota. The date is October 5, 1994.**

**Okay Mildred, we'll get started now. I want to ask you a little bit about your family background - aspects of your childhood, the educational and occupational characteristics of your parents, or anything else you think is significant for this interview.**

Templin: Okay. I guess the first thing that is of significance is that I am here. This is October and at the end of this month I will be 81. I was born in 1913 on October 30 in Milwaukee, Wisconsin. I was an only child. All of my grandparents lived relatively close to us, and all of them were immigrants from Europe - three of them were pre-Civil War immigrants and one came shortly thereafter.

My mother and dad were very understanding parents. Neither of them had any formal high school education. My mother went to a one-room school in the country. She said it was a great chance to learn what the older ones were learning. My father, on the other hand, having finished only the eighth grade formally, took extra courses until he actually matriculated in the Med School at Marquette. He never went through with this because I arrived. The fact that he was a druggist at that time gave him work to do.

I had a sheltered -- but not really a very sheltered -- life. I had a very free and independent existence and could do a lot of different things. One of the things that probably was important in my later interest in very detailed research was that my dad in the drug store had a pay station of gas and electric bills and a post office. My mother helped him a couple nights a week. Not every week, but reasonably often. I worked with her to check the bills against the cash paid. This impressed me with the importance of accuracy and instilled a real interest in detail. I never did any real work in the drug store. I was free to get in and out, but not to interfere with the carrying on of business.

**Moore: This also presented a model for you of a woman who was a businesswoman and a career person, a little bit.**

Templin: My mother? Not really. She worked three nights a week and every other Sunday.

**Moore: That's a fair amount for those days.**

Templin: Yes, and my dad was there. One of the things that I have reflected upon as I've gotten older is that I didn't like living above the drug store as a youngster really. My friends and my relatives had houses. But as I look back, I think my experiences of seeing my father as a professional man were invaluable. For example, when he got a new balance, he could show me how important the accuracy was. He took the little prescription papers and put them on and wrote my name with a very soft pencil and showed me how it weighed. I'm a very verbal person and he never again had to tell me I should never talk to him when he was compounding a prescription. There were many things like that. My mother was really a great helpmeet, and my grandparents being very near provided an opportunity for cross-generational things that today I treasure. At the time, as for most children it was probably not what I thought was the best thing in the world. I also had some lessons. I had piano lessons; I wasn't very good. They didn't try singing: I couldn't sing and they recognized that. But I did have what we used to call elocution lessons, and I did have those from the beginning, which I suspect was an interest in communication shown very, very early.

Then I had some interesting experiences with people that I knew. My aunt was hard of hearing. She became hard of hearing as an adolescent. When I was growing up and knew her in the 1920's she carried one of these old 9x6 cigar-box-type hearing aids around. So I knew that. There was a man in our neighborhood who had a laryngectomy. He had this funny pipe and this strange voice. That was interesting. But particularly there was a youngster in the neighborhood who was hard of hearing. He was at school and he wasn't doing well, so they sent him to the state school at Delavan. My mother and dad took him and his parents and his sister; I went along for this final passage to get into the institution. I was in grade school at the time. I knew his speech. I saw what was happening to him. I went through the whole school when they took the parents there. This really stimulated my interest, and I already had some interest in communication. Then along with that, in our whole family life there was always the feeling that you ought to help other people. That's when I got directed over into speech correction, speech pathology.

I finally graduated from high school. Oh, where did I go to school? I attended the public school, just about a block and a half away from us. It was wonderful, a great high school. I looked through the annual not long ago, and I was so impressed. Gave it to some of the youngsters that I knew who were interviewing the old folks recently. We had girls' basketball teams and boys'; a large number and array of clubs -- the quality of work in them was just unbelievable. There was a very high standard that was kept -- it was just part of the picture. But when I graduated now I knew that I wanted to do something in this new field (speech path at that time was relatively new). So where do you go? Well, you ask and find out. Wisconsin, Iowa, and Northwestern -- my parents found out were the schools with the highest rating in that area at that particular time, and so we made trips to all of them. My dad invited a friend of his along because he was an MD and knew how to talk to people in the academic world, I guess. We talked with all of them and when we came back -- I think this is an illustration of my freedom with some restriction -- my dad said, "Now which one would you like? You can go to any one -- but if you decide to go to a state school, I think you ought to go to your own." So I went to Wisconsin, and it was great! There I was in and out with illness, but essentially I graduated from Wisconsin in 1936 after spending time at a number of different schools. I would just about get going, and I'd have a throat, an appendectomy, or something of this sort. This was in the days before penicillin so some of these things became real problems.

**Moore: More serious.**

Templin: Yes, they were serious problems, but I finally got to Wisconsin the last time. Robert West was the head of the Department of Speech Pathology.

**Moore: So that was your major.**

Templin: Well, no. My major was just in Humanities and Speech. The choice at the end of this period from what I had had as an undergrad was, Do you want to go into speech pathology? Into persuasion, debate, or even the theater?

**Moore: So it was a fairly broad background.**

Templin: It was a broad background, and we took work in what we called at that time Philology. I had a lot of background – a good literature background and other liberal arts. Well, Robert West really was a mentor in many ways.

When I was an undergrad, Robert West took me to Milwaukee to go through the picture records of a Dr. G.V.I. Brown who was a well-known surgeon working with cleft palate. I had all before-and-after pictures, and I made a filmstrip that was used in class for a long time. Also, believe it or not, I pored over German and English. It was the only time I ever really read German -- I wasn't very good at it, but I knew enough to do this. Dr. West did this kind of thing, and for students in the seminars it was very exciting. He gave us great opportunity. You did clinical work. You also did work with the, well, quasi-medical type of thing. It was fun because they had a flame for a dynamometer; when you breathed into it, you could identify physical problems. The equipment was very different. But anyway I did that. I was an assistant to Andrew Weaver (he was the head of the Speech Department), because I had taken extra work in the previous semester. I had been out because of illness and took 18 credits with the notion that maybe somebody would offer me a job, which was a joke to everybody because this was the depth of the Depression and jobs were not running around.

**Moore: There weren't any!**

Templin: The strange thing is that I came back from Christmas vacation and I got a job assisting Dr. Weaver in the psychology of speech. One of the things that we had to do in that was testing the lung capacity of all twenty members of the class. Later I found the old list -- four members of that class were on the faculty of the University of Minnesota at the same time I was when I first went to Minneapolis.

However, at this point I was looking for something and something turned up: an assistantship at Purdue. This was when I made the choice of where do I go into these areas. I decided that this was to be in the speech pathology, speech correction area. So I went down to Purdue into the clinic. At that time the speech clinic was part of the Speech Department, which was part of the English Department. There were 31 men and me in it, and I didn't notice that I was the only woman; nobody cared and neither did I. The clinic was just starting. Max Steer, a very creative person, was just completing his doctorate from Iowa, and he had two other young graduate students – one from Iowa, one from Northwestern, and now me from Wisconsin (the three schools that I had been thinking about originally). It was really a wonderful time. Max, as I said, was very creative, very innovative, and in the summer sessions he brought pioneers in the field from all over. So I met Wendell Johnson and Jack Johnson from Iowa, G. Oscar Russell from Ohio, and Bryng Bryngelson from Minnesota during my first year out as a graduate student. It was fun. Well, I worked in the clinic, and I had the title of supervising clinician in articulation. There weren't any people to supervise since I was the only clinician in articulation. But I did research with some college students and during the summer with children brought in, so there were youngsters. One of the problems that we were faced with was, What should you expect individuals to do in correct production of sound? Of course the techniques in therapy were there but that was a real question, What kind of standards, criteria do you have?

**Moore: What are the norms?**

Templin: What are the norms? That's it. What are they, where were they? There were some but really not very good ones, and this was not to be a normative thing but Max, in his great way said, "You are right next door to a nursery school. Why don't you go and listen to some children?" So I did. I ate with them. I played with them. I listened and we set this dam. I think this is an indication of what happens in a situation when a field is developing. You are much freer to make a statement and go out and do something and try it than you are when the field is well established. Well, I did that. Actually that particular study resulted in my second publication in 1938, I think, with Max Steer on the development of speech in children.

**Moore: Preschool-aged children?**

Templin: Preschool-aged children. It's not a very exciting thing, but it did indicate the direction that I started in and stayed with a long time. Then after several years at Purdue I began to feel a little itchy. I had completed a Masters degree meantime doing the study on aggressiveness of stutterers at Purdue. It was applied to my graduate work at Wisconsin. I get very mixed up throughout my academic career on where I am.

**Moore: Well, you went back and forth a few times.**

Templin: Yes, but even in what school things were done. I transferred work from Purdue in my Masters at Wisconsin. I had been at Purdue for about 3 years when I said to myself, "No, I've got to go on and get a doctorate."

**Moore: This is from Purdue?**

Templin: No, I would have gone back to Wisconsin. Purdue had no good program in Communication; they only had this clinic that was developed. So I went back to Wisconsin to get a major in Speech Path and by that time I had become interested enough in children that I decided that I was going to do the minor in Child Psych.

**Moore: I see.**

Templin: Wisconsin was just beginning a program in Child. They suggested that I be a candidate and I said that I didn't want to be. I wanted to go to a place that was more established. So I was at Wisconsin for one semester taking weird things like comparative anatomy and chemistry because we had to have this in speech path, and I went to Teachers College, Columbia because they had one of the old, established programs. Arthur Jersild was there. I was still interested in the deaf; we had been working with them along the way. Rudolph Pintner and a couple others were at Teachers College at the time. So I went to Teacher's College and I got into these courses with these people. Then after about two months of the semester my father had a very severe heart attack. A doctor friend of his called me and said that this was very severe and he thought I should come home. So I packed up and came home, staying home actually for three years.

Meantime, in my persistent way I had to find out what I could do about these courses I had started at Teacher's College. They were quite firm that I could make up anything, but I had to sit at the feet of an instructor the right number of hours -- couldn't take any courses to prove this, you just had to do it. So I went back, and it was really a very interesting time. I continued work with Pintner, and I did intelligence testing, and I went down to the New York League of Hard of Hearing -- continued that as I had every Saturday morning, which was very nice. Got you right downtown where you could go into one of the theaters after your class down there. But the most interesting one was the letter I had from Arthur Jersild. He said that the course that I had taken from him he was teaching again that summer but he had absolutely reversed his order, so the part he would be covering when I would be there would be the part that I had had. But Dorothea McCarthy was teaching that summer (she was at Fordham and she was teaching at Teacher's College that summer), so he suggested that I take this particular course in language development from her because that was what I had missed from him.

**Moore: That was not a bad idea was it?**

Templin: So I did this. I found that I, of course, was more and more interested in the whole language development. This was Dorothea's field. She became a good friend of mine from this. The first study that I did on the difference in the articulation of children with a spontaneous and an imitated model was done as a class project. Later it was developed, and it was one of the studies that I did along the way. Dorothea and I had some good talks about where I should go. It got down to Iowa and Minnesota. Partly because of my father's illness, the war years, the ease of transportation on the trains between Minneapolis and Milwaukee, and also, strange as this may seem, because of the less close relationship between the speech clinic and the child development work at Minnesota in comparison to Iowa (I figured if I'm going to change, let's change to your major; I had now decided that Child was going to be the major), I happened to come to Minnesota. So the illness of my father was really a very important, critical point in my life.

The other thing that I should say is that about two years before this, World War II also had a tremendous impact. I had not been so serious about Child two years before as I was about the whole area of linguistics. So I went back to Madison, talked to my old friend Dr. Robert West about this. After much discussion, we decided that I should go to London to study linguistics. This was fine. We had this all decided, but I had to finish the work I was doing, so I would go in about two years – but in 1940, you don't go to London.

**Moore: No, you don't.**

Templin: So that was changed completely. Also in 1940 I was called home. When I stayed in Milwaukee for those three years, what could I do? My parents were competent, but my dad was ill. I was the only child and I think anything I would have done would have been all right, but we were a supportive and caring family so I wanted to be there. At the same time I couldn't just sit at home. So I went around looking for jobs. I had a number offered – from Marquette, the old Teacher's College, and a number of suburban schools. I took Wauwatosa. A rather progressive, semi-affluent suburb of Milwaukee, Wauwatosa had one other "speech person," as they called the speech teacher at that time, and I was to be the other one. I told the superintendent of schools I didn't think I'd be there very long, and he said, "That's all right. We'd rather have a good person on the way up than somebody on the way down who hangs on forever." So that was set.

Those three years in the public schools gave me an experience that I deeply treasure. I did therapy with children. There were no school psychologists, but my training had been broad enough since I had had intelligence testing at Wisconsin and I had a good psych background also. Actually, I had physiological psychology from Harry Harlow. So we did the intelligence testing in the schools, we met with the parents, and so we had a regular clinical visit. Then believe it or not, they permitted me to do some research in the schools as part of this, so I did a study on the sound discrimination of youngsters and adapted it from some of Travis's work to come up with *The Sound Discrimination Ability of Elementary School Teachers*, published in '43. You see I was there from '40 to '43 and the public schools permitted this. Again, this is the kind of freedom that you would not have, I suspect, today with all of the rules and regulations. After three years of that practical experience, I again had this feeling that I had to go back to school and get a Ph.D. You know, you sit down working with little youngsters and you say, "Shay, shay, shay," and all of a sudden one day just out of the blue, I said, "I don't want to sit here ten years from now saying this to little kids. There is more to what I want to do than that." I announced I was leaving, and everybody was very happy.

That summer I went up to Wisconsin again and got involved in some reading work with the Torgerson; again I was assisting because I had this practical communicative bent. Also, at this time I again contacted Dorothea McCarthy about where to go, and that's when I decided to go to Minnesota. With this came a real shift from the whole clinical to the developmental picture of children. I was really very naive about the whole field of child psychology, child development. I was 29 at this point and I had spent all of this time in the area of speech and hearing. I was an assistant to John Anderson when I first came, and we immediately got into research. Florence Goodenough was there. She was my confidant and the one who really directed me toward my thesis and a few things like this, although John E. was the one that I worked with on this. I was his assistant for one year, and then because of the war, again there was a real shift. Dale Harris left to go into the Marines. So I began to teach right then and there in language development, development of children, and some other things. I was qualified, but not overqualified, at that point.

**Moore: So you really were bridging the gap between a student and a faculty member already at that time.**

Templin: I bridged that gap in the second year that I was there, which was in '44-'45.

**Moore: You also came in with a good background.**

Templin: I did have the background. I had six years of experience and two years of graduate work. It was not tailored or focused in child psych, but the interrelationship and the fact that communication is so basic made it a good background to be coming in with. I've maintained interest in both fields ever since. But I really knew that I had shifted when we started doing studies here. There just weren't that many people around. The whole

Goodenough and Anderson Experimental Child Study had just come out. Life was different. Piaget had been translated for not many years at this point. It was a very different world really. When I found that when I was listening to children's articulation (and I was still interested in it), I was looking for correct production rather than erroneous production, I became aware of the fact that I had shifted internally. Both of them were still there, but I had a different orientation: What I was now checking for was quite different.

**Moore: So the difference between norms and disorders, is that the distinction you are talking about?**

Templin: Yes, that's the distinction I am talking about. When you look at the disorders you look for error. When you look for norms you look for accuracy. So that is what happened there. But I made that shift much more rapidly than I would have, had it not been for World War II. Also it had a real impact on my thesis. I had planned an individual study of the development of certain cognitive ideas among the deaf based on Edna Heidbreder's work. I had it pretty far along but I had to go out and be away and I had to teach, so I had to dream up something else. That's why my thesis on reasoning of children with impaired hearing was set up among the hard of hearing and deaf in public situations and in institutional situations. This included environmental restrictions and the hearing, the sensory restrictions. Then the thesis itself dealt with the learning tasks that involved just symbols. Then you had some that involved hierarchical learning in which the youngsters got a sort of dictionary listing – for example, if a child eats “perch” and “bass” and gets ill, or “bass” and some other fish and doesn't, which one -- you know the procedure.

**Moore: That kind of reasoning.**

Templin: Yes. Then the third element of the thesis was an adaptation of Piaget where you had things that children were not as likely to come in contact with. That has been a very interesting study, and it has had meaning over the years.

**Moore: Okay Mildred I think we are working now into, with a little bit of transition, however you want to do it, into the personal research contributions section of this interview and a discussion of your major research activities and what you feel the impact of these activities has been on the field.**

Templin: Well, the concern that I had with the development of the speech sounds persisted. I did this thesis on cognitive performance (we called it reasoning) of the deaf and hard of hearing children. That came about because I had been intrigued with some of the studies of Piaget at the time. When I was struggling for a thesis topic and had to find something that could be done without so many short jaunts that I couldn't take care of responsibilities, Florence Goodenough asked, “Well, don't you have any recurring questions?” I said, “Yes. What would the deaf do?” And she said, “Well, why don't you go and find out.” That got me into the actual study of the deaf. That was published later as a monograph, and it is still quoted. In later years I was amazed because it dealt with some of the concepts that were important then and are still important now. It dealt with: What does a restriction within the individual sensory restriction and environmental restriction do? The fallacy of the thing was that institutionalizing the child does not necessarily mean restriction. It was very difficult to find so-called normal youngsters (because we had those too) who were institutionalized. The nearest we could come was the Mooseheart group down in Illinois.

**Moore: You mean normal in ways other than language that would --**

Templin: Normal intellectually and with no hearing loss. It was a reasonably good study that got me going on other questions. I still was primarily interested in the speech patterns. So I started shortly after I finally finished my degree (which took a little longer than it might have because of these other situations). I started the study of certain language skills, which looked at children between 3 and 8, half-year intervals between 3 and 6 or 5, and then in half boys and half girls with sixty at each age -- the old representative sample. We went through the welfare records, a lot of things that we might not have done. The school system tested all the children to make sure that they did not have hearing losses. The amount of cooperation for the study was really very great. At that time I had found as you stay in an area (and I stayed in the area from the very beginning, as I look back), it expands, but you are still in the same areas from just the speech sounds into the whole communication area. So this study of certain language skills that I did was one of the first studies to include a good solid representative group of children and to deal with a number of the language dimensions - it was articulation (straight production), sound discrimination (perception),

vocabulary (intellectual comprehension), and sentence structure (which had to do with usage). It was, I think, the first study with a rather large sample (some 400? I don't remember how many children anymore) and dealt with several language dimensions. Without a doubt, it is the thing that has had the greatest impact, as far as I am concerned. It was quoted, it is still quoted, and it's a little astounding because it was after the old question: What are the normative expectancies? The last time we looked was I think in 1981; I haven't looked since at all the citation abstracts – that study was cited in every year from the time of publication; just last year in 1993 when I was looking through one of the journals, I came across a study on articulation clusters in which the study is quoted. It is still there, and it was very, very widely used.

**Moore: Do you think that's why its staying power and its impact was of that dimension, that it impacted a lot of different fields or different practitioners or what? Why did it?**

Templin: I think the reason is that it presented something that people in a number of areas were looking for and also that I did not do any interpretation. There comes a time when people grab what you're doing out of the typewriter and say this is finished. I'm always ready for another revision. I never did write an interpretation. I would have written it from the point of view of the speech therapist, the clinician, or the child psychologist.

**Moore: So you might have constrained how it was used.**

Templin: I would have constrained how it was used. I don't think there is any doubt. Looking at where it is used, it has appeared in foreign journals; way back in 1981 it was quoted in well over a hundred different journals. The interest goes all the way from linguistics and child development, teaching of reading, dentistry, maxillofacial surgery, to pediatrics. Some of these are things that I would never even think of, and yet a maxillofacial surgeon is very interested in what effect this is going to have on the speech of the child. So my research was very simple, to get at the question of: What do children do? It's an extension of the thing I started at Purdue, extended not only in numbers but in dimensions, and it does offer a great deal in a practical guide. The other interesting thing about it was that the articulation part used a very broad transcription so you weren't dealing with the slight differences between a lateral "s" and a "sh," the in-between funny little sounds, the approximations that the clinicians are concerned with. It didn't deal with that. It dealt with when children achieve what is recognized as a basic phoneme in English.

**Moore: That can be understood by one.**

Templin: That could be understood as such. And because of this it also -- it's less likely to show the fluctuations that you get when you look at some of the narrower transcriptions. It didn't deal with that. Another thing: it did have the advantage of dealing with the various language dimensions, and it came out at the right time, which has an impact.

It came out right at the time that the psycholinguistic movement was starting. It did not come out so long before that it was overlooked. Neither did it come out two years later when it would have been outdated and not looked at at all. The techniques that were used for analysis are old-fashioned ones -- mean length of sentence and all of this because that is what we were doing at that point. On the other hand, the data are still available, if anybody wishes to look at them (you know I throw nothing out). So it's still possible to reanalyze some of these. I think the significance of the study was its timing, its practicality, and the broad basic needs. I had no expectation of it, but I think that's why the impact was so great.

Now the thing that grew out of that study was the old Templin/Darley Tests, and they're not tests.

**Moore: Would you say something about this?**

Templin: They're not tests. Fred Darley was in the clinic at Iowa at the time. When you get in a field early, you meet many people all over. I was good friends with all the people at Iowa. In fact, when I started a later study, a longitudinal study, I went down to Iowa to talk to them. I had suggestions from various people of what should go in and this helped a great deal in the design of that particular study. But Fred called me one day from Iowa. He was in the clinic, and he said, "Mildred, I am so tired of my graduate students cutting out pictures instead of testing

children. There isn't anything available except what you have done here. Would you mind if we used this?" I said, "No." He said, "Do you want to do something with it?" And I said, "Well, what do you think?" Together we wrote, I think, one of the best manuals for a test. I wrote the research, and Fred wrote the clinical. I had not had any clinical case since 1943; he was in the midst of it. We'd gone through revisions and we'd done a lot. It is still used. The reason it's used is that it was so very simple in the way it presented things. We could incorporate the sounds in the Iowa Pressure Test for cleft palate and other measures that they could draw from it. We did it in a way that the therapist/clinician using this could draw out from the simple measurement the patterns that they were particularly interested in. It did not follow the pattern of somebody, you know, "if this sound is in this position so forth," which restricts; it may sound strange, but lack of such restrictions opens up to greater use. Also, this in particular, in the other study I want to mention here opens up the whole idea of hypothesis generating rather than hypothesis testing, because the data that were presented were germinal or seminal to many people doing other things.

**Moore: Why do you say that it's not a test? You do make that point.**

Templin: It's never been normed.

**Moore: I see, okay. Is it sort of like competency based as a way of testing?**

Templin: Well, the scores are based on what we found children doing. But we did not take those particular sounds and norm a test on another sample. So methodologically it's not a test.

**Moore: In fact, the way it's used it is.**

Templin: That's right. It has had an amazing impact.

**Moore: It has.**

Templin: And the reason again is that we did not try to impose how it should be used. I think basic to all of this is an assumption that people competent in a number of related fields that are interested in this are competent, but they are looking for something to guide them; then they can do what they wish and go beyond if they need to. I'd guess that that is why it has become a kind of hypothesis-generating thing over time.

**Moore: So as interpreting – extending the use?**

Templin: In any case, it is a very extensively used, and actually it is the thing that has been. I've had a lot of honors because of it.

**Moore: Based on that and your work, the staying power of your work.**

Templin: The staying power of it. And so I do have a number of honors here, such as the honorary degree from Purdue.

**Moore: Do you want to comment on that at this point?**

Templin: Well, I might.

**Moore: Yes, why don't you, because I think you really do have a pretty remarkable array of honors, and I think it needs to be acknowledged here.**

Templin: Yes. Well, I think that the one that has impressed me most is the honorary doctorate from Purdue.

**Moore: Yes, indeed.**

Templin: And of course Purdue is still thought of, I think, largely as a male institution, and so the honorary degree was rather surprising to some people. I had one friend whose son got a degree in engineering and happened to be there and saw my name on the program and called to ask if this was me!

**Moore: That was 1976?**

Templin: 1976, the year I retired. Of course, there are always circumstantial reasons. Purdue over the years that it had been giving honorary degrees had given some three hundred, of which only nine or ten had been to women over the years. A woman regent, or someone of this sort, had suggested that they find women. So they did. In giving these they had criteria that were hard for women to meet. You had, in order to get an honorary degree from Purdue, to have been there. So that was part of it, and the year that I got mine they gave two: one to Barbara Newell, who was then president of Wellesley, and me. The reason was that they wanted to recognize women from earlier classes, but there was a much smaller pool to draw from. When I was there in the late 1930's, Purdue had about 5,000 students, about 500 of them women. Forty years later when I got the honorary degree, there were 5,000 in the graduating class. Thus, if you want to go back, it is difficult to find women who meet all of these criteria; in later classes, more have been meeting them. I'm not saying this to belittle this honor, but I am saying that that was one of the factors. Also, by 1976, the speech clinic at Purdue had become very big -- very, very large really and very important. And it's now in humanities.

**Moore: So you represented them to some extent.**

Templin: Actually, I was recommended by the people there. Max Steer was still there. I knew many of the people there too.

**Moore: I think the list of your supporters for this degree is interesting, and it's pretty heavily peppered with men and important people in the field.**

Templin: Well, do you want to hear a couple? Well, the list I sent in for that was Roger Brown, Fred Darley, whom I've mentioned. Frances Graham, Bill Hartup, Jack Matthews (chairman of the speech department at Pittsburgh), Jack Merwin (the Dean of the College of Education at Minnesota), Bob Sears (he took over the Monographs after me), Dick Silverman (director of the Central Institute for the Deaf in St. Louis), Duane Spriesterspuck (dean of the Graduate School at Iowa), and Clark Starr from Communications Disorders here.

**Moore: That's a pretty stellar group I'd say.**

Templin: It says, again, that when I say that I was in two fields, I was.

**Moore: Yes, you were. Clearly your supporters are broadly based people.**

Templin: And then, as I say, I always stayed primarily in the speech area -- language, but in the speech area. In 1981 the American Speech Language and Hearing Association gave me the honors of the Association. But the interesting thing there too, as I have emphasized a number of times, is that if you get into something early it has an impact. I joined its forerunner, which was the American Speech Correction Association. Then it evolved into the Speech and Hearing, and then Speech Language Hearing, although they still refer to it as ASHA. But when I joined ASHA, it had 250 members, and when I was vice-president of it in charge of a convention in about '65, there were 5,000 at the convention. Now there are about 70,000 members in the organization at this point. So that makes a difference. Then in that same area the Minnesota Speech, Language and Hearing Association just this last year in '93 gave me a Distinguished Service Award. I was never active in the Minnesota Association except as chairman of their ethics committee at one point, but I did contribute in other ways.

**Moore: Now that was just last year?**

Templin: Yes, last April. I had just moved into where I am now. It is interesting to realize that some things you do rather casually at one point have an impact later. For example, I had a really great experience a couple of years ago. I was invited to the fiftieth anniversary of the founding of the Indiana Speech and Hearing Association; there were

three of us there: Max Steer, Bob Milisen from Indiana, and me. I was a little amazed when they referred to me as a founding mother. I didn't know we had founded anything.

**Moore: You don't know that until way after the fact.**

Templin: In '37 there were about 14 of us that were studying people at Indiana and Purdue. People at the school for the deaf and in a couple of other places were the only ones doing this in the whole state of Indiana. We got together and had a session and that became ISHA. I didn't know it was the founding meeting.

Well, you asked about honors. One of them that has interested me very much is the American Men of Science. That goes way back to the early days -- '55.

**Moore: When you were a man!**

Templin: Well, there were very few women listed. Just a few years later things were changed, and it became the American Men and Women of Science. Also, when I became a member of Sigma Xi is a delightful tale. That was in '51.

**Moore: My goodness, that's over 40 years ago, isn't it?**

Templin: Yes. One of the people I knew very well in Physiology was elected at the same time. He looked at me and said, "I always thought Child Development was an art!" And that one really developed.

**Moore: And here you are a scientist.**

Templin: So I was a scientist. Well, let's get back to where I was talking about the pattern of the testing procedure. That is widely used. I think that really has been where the biggest impact has been -- in those two publications. I have done other things, but they were complementary to some extent. Perhaps I am a frustrated sociologist in a way. I've been interested in all of the environmental impact on some of these situations. And I'm such a developmentalist that I like to see what happens over time, so there is a repetitive thing that I like very much in the studies I have done. Also, the one thing beyond this is that I started a longitudinal study with children in all of these various language bits. We had a lot of youngsters who were tested (well over 500 initially) and followed every six months. Unfortunately, that is available only in short reports, although students have used part of it. The reason is that although there is a lot of good material there for hypothesis generating, the big problem with it was funding. It was too big for the amount of funding we were able to get.

Initially, when I started some of these studies, there wasn't any outside funding for your thesis and other studies except for a little help from the Institute in the form of a person here or there. Then funding grew a little. I had some funding through the graduate school and then, as time went on the US Office of Education funded most of it. But the problem with funding that varies over time is that when I was getting over the longitudinal bit, the funded projects had nothing to do with the earlier studies that I had done on a shoestring. Thus, USOE were not interested in the longitudinal picture, but a researcher interested in a longitudinal study does it. So I did it in bits and pieces. As a result we've got a lot of unanalyzed, very valuable data. I am very careful in what I had done, for example, when I had replication of some of Kellogg Hunt's work. We sent all the scoring we did to him so we know. This is just one example. There are some other things that had been done but probably because of my long interest, it is more in the area of articulation than in some of these other things. I guess that's what we really say about the impact of funding over time.

**Moore: No work can start until people are secure of funding. Work is extended over a period of time depending more on funding than the needs of the study. And researchers are a bit afraid to start a longitudinal study because it isn't very useful unless it is completed, and we don't know whether we will be able to complete it. I think that is still a serious problem in our field.**

Templin: I agree that it is very important, particularly if you are really interested in development. One of the things I learned from the cross-sectional study and all the others is that you get cross-sectional norms, but you don't know

what happens to the individual unless you do longitudinal study. It is very, very valuable if you are really interested in development, and as Harold Jones pointed out long ago, longitudinal studies are always out of step with the now.

**Moore: In what sense now?**

Templin: Well, in the sense that the methodology that you use for something you want to follow for 15 years will be out of date in a sense upon completion, but it's the best that can be available. When everybody is doing something, it has already crested. So you have questions of choice and funding. But I believe that if you are really concerned with the development of individual children – to me the central concept of development -- the longitudinal study is the only way you get it. So I think that the question of funding is a central one, and it is not one that institutions themselves can handle.

**Moore: Yes, I think that is right.**

Templin: It's an expensive process. Much more expensive today than it was in the 60's.

**Moore: Okay, let's go on to your institutional contributions. You have been at the Institute of Child Development for a long time. You also spent time at Purdue and other places. Can you comment a bit on what you think was happening at those places at the time that you were there and changes and their particular role in the field?**

Templin: Yes, I was at Purdue for three years from '36 to '39. As I said earlier, it was at a time when the speech communications problems were really developing, and we had unusual opportunities to develop. Since then, it has become a separate unit. It has expanded, and it is a well-recognized program -- instead of a little clinic. I think a lot of it is due to the innovative concepts of Max Steer. He is no longer the director, but he is still around. I remember when I visited some time ago; they had this gorgeous new building -- audiometers all over the place. I noticed because Frank Gilbreth, who wrote Cheaper by the Dozen, had been at Purdue when I was there, coming one semester a year and I said, "Max, Are audiometers cheaper by the dozen now?" But Max is the type of dreamer who could conceive a problem, find ways of doing it, sell it to the Rotarians, and come back with money in the afternoon. So that is one thing that I think is very interesting.

Now my experience in the public school I have already alluded to. It was a priceless experience. In the schools today we see a long discussion of what you call the speech teacher, how to handle diagnosis and treatment of speech and hearing difficulties and where, and the role of the school psychologist. In my day, it was all combined.

Then of course, in all the years that I was at the University of Minnesota, the Institute was my primary base. However, I never ever lost touch with Communication Disorders, even though I had come here because the tie wasn't so close as it would have been at Iowa. Actually, I taught a seminar in articulation development in Communication Disorders for a number of years, largely for the graduate students in their program. In 1965 or so, I had a joint professorship between the two, so I was really based in both. At the Institute teaching was great, but we had a light load and we were free to do the kind of research we wanted. We didn't have an overload of teaching even in the years when things were cut. We taught for other departments. Some of the most interesting teaching that I did for a number of years was in General College when I taught human development. I learned a great deal from teaching a class of two hundred freshman and sophomores in a two-year college situation. One thing I learned in lecturing was that you never use a technical term before you explain the behavior that you are concerned with, because immediately upon using the technical term you cut off too many people. "Inferiority complex" was one that was popular in those days -- you didn't ever mention it until you had a lot of descriptions of behavior.

Then I taught in the Institute again, the same things that you would expect: language and thought, development of. I taught handicapped children. As I mentioned, I was always interested in the environmental impact. I taught children during World War II, and it was just fascinating. I would have loved to have been able to do some more of that, but the research that I did always (with the exception of a few studies) had a practical implication. I think it was so embedded into me, engrained, that I couldn't get out of it. I'm not apologizing for this; it was just the area in which I focused. We had a linguistics group. I remember when I presented my cross-sectional study, and people were really concerned with the very fine discriminations -- where you draw the line, the over lapping of sound, and so on.

Had I covered this too, and they were very understanding of the fact that the sound system -- the transcription -- that I was using was not for how a person trained to listen would hear, but for now the average individual would hear these things. This came through very clearly. A lot of the teaching that I did was of the persons who were not necessarily my students but who were working on some of the research projects, especially when we got into this longitudinal study where we had 16 or 17 people out testing and doing things. Many of them were from Communication Disorders because that was the heart of the study, and also some from of the Psychology because students were concerned with measurement.

**Moore: So in a sense your teaching paralleled your interest in applications because a number of the departments that you taught and programs that you taught for were training practitioners.**

Templin: That's right. It just fell that way.

**Moore: You had that contribution to make to them.**

Templin: Oh yes, with all the years that I've had in the clinical area -- and in two varied settings as well. I also maintained, while teaching the psychology of handicapped children, my basic concern with the deaf. I invited people in (again, the practical application). Bob Lauritson was doing some very interesting work at the technical vocational institute in St. Paul, the deaf -- and these were really deaf -- children were incorporated into the regular programs with an interpreter. They had been doing all this all along. Lauritson came; one person particularly (and maybe more) took this so seriously that she did all of it. She went over because they were looking for not only volunteers but interns to do this type of thing. She became interested, went into the field, and ultimately married a deaf person because this became that important. Thus, although I really don't know how many graduate students I had who finished, there were not so many as most people would have over the years. What I had, however, was a fair number of masters people and a lot of foreign students. They had other advisors, but I was the one who worked with them very definitely. Another group I had was a lot of incoming graduate students. After I got to know them and know their interests, I passed them on to advisers in the field of their primary interests. But I gave them some general background. A number of these people who have come to me remember some interaction I had forgotten. Which always happens, of course.

**Moore: Mildred, Can you say something about -- and maybe not yet. You decide whether that comes next -- about the changes in the Institute over the period of time that you were there? Some of those changes may be idiosyncratic to the Institute, but they probably also reflect changes in the field over those years.**

Templin: Oh, there were such tremendous changes. When I came in '43 the Institute was at a very low point. It had been founded with Rockefeller funds in '26 and then special grants from the legislature, and it was responsible only directly to a vice-president -- in no department or anything else, but isolated, which gave us a lot of freedom. It also kept salaries down. That's very interesting: you could look at salaries of John Anderson and Goody, and today you would just be horrified, but it gave them the freedom.

**Moore: Even relatively, they were low for that time.**

Templin: I think so, yes.

**Moore: But on the other hand, they had autonomy that others didn't have.**

Templin: They had autonomy. But as time went on, the University got rid of all independent units. So there was a great deal of discussion. I went through all of this. This discussion was not a one-term thing, but arose a number of times (Where are you going to locate these?) and we thought of many things. We thought of the School of Home Economics, which here is located in the agricultural school, adopting the old Iowa Child Welfare research station. Adapting that notion of the research stations would fit very nicely, and they did have child development programs and so forth. That was seriously considered. Psychology was considered because the Institute of Child Development is interesting and it has all of the developmental stuff, which our psychology department does not. Initially, the Institute had no undergraduate work at all. It had only graduate students and it taught courses in other departments for undergrads that might be in nursing, might be in home economics, might be in sociology.

**Moore: In education.**

Templin: And in education, since it fit well there too. Finally, we settled on Education after there was very serious concern about going into liberal arts – really two problems, maybe part of or maybe parallel to the psychology department. In fact, if I recall correctly, it even appeared in one of the minutes that this was done. In the end, we went into Education. Why? Because Education had access to children.

**Moore: That is why, huh?**

Templin: Well, I wouldn't say it's the only reason, but it was a primary reason, and it was one of the reasons why a lot of the studies done initially at the Institute were normative studies, whereas those that were done at Iowa were not -- we just had more children from the access to the schools in a large setting, excellent relationships with the schools from the very beginning. Then, of course, the nursery school initially was set up for research and dissemination of information. They actually brought children in to maintain, because they had enough money; they didn't change tuition and so forth. By the time I arrived that money was gone, and so it became pretty much a care center without the breadth of opportunity for studies of the children and its time had passed. Right at the time the question where to place the Institute was being considered, the whole undergraduate program in Liberal Arts was put in the College of Liberal Arts (CLA), which is now a very large program -- when we had just decided to go into Education. In retrospect, from outside, it's just a very mixed up thing, but at the time the emphasis was on the notion of utilizing the skills where needed and being able to cut across some of the patterns that you would expect. One result that I have seen from this placement is that the faculty of the Institute take much greater part in the work of the University as a whole. The Institute is not so isolated.

**Moore: Currently, would you say it?**

Templin: Well, I don't know about currently, but during the period that I was there, and you were there, it was very definite: John E. was the only one who went outside. Of course, at one point we only had about seven people. Now the size of the faculty, the role of the faculty has become very different. They are now a part of an intermediate body so that you aren't isolated -- just off here, where you get a lot of what you want. The one thing that was also true of the Institute (and still is -- you can correct me if I'm wrong, because I haven't been around very much recently) was that the Institute always had a deep respect for scholarship, which was true both in teaching and in individual research; there was no major attempt to force faculty into certain pigeon holes. I don't think the Institute was ever known as the place to go for one particular thing. There has always been a breadth. Of course, that was one of the things that appealed to me about the Institute.

**Moore: I think that is still true.**

Templin: It's true. I remember that when Harold Stevenson first came we had some visitors from Russia, and I met the one woman who was there. She was concerned about the role of the director. I said, "I am perfectly free to do what I want." So she brought this up with Harold, her notion was that the director should determine what was being done with that Institute. Harold said, "No." Pointing to me, he said, "She is interested in SRNL; I don't care anything about SRNL. But if she is interested in it, and she is doing a good job, she does it." This woman asked, "How do you know?" And he said, "I don't, but the peers in her field do." I think that that was true when I first came, from what I knew of the earlier days it was true then, and I think it's persisted.

**Moore: Yes, it has persisted. I think there is no question about it.**

Templin: I do think in the days when I first came they were already talking of the decline in the actual multidisciplinary things. If you look at some of the early publications of the Monographs of the Institute of Child Welfare at that time, you find people in sociology and dentistry actively participating with the group. Over the years I was there, it seemed to me they were brought in more and more as consultants, not as people working together; this, I think, was partly the development of the field, having more strength in and of itself. I think that this whole multi-disciplinary thing is quite important even for the Society for Research in Child Development.

**Moore:** You know, Mildred, one thing occurs to me when you describe this dilemma of where to put the Institute of Child Development versus keeping it as a free-standing unit, and it goes to the College of Education but not to CLA, alongside Psychology. Interestingly, that also was a period of time when gradually almost everybody in the Institute was becoming a psychologist, not a member of the other discipline. There are some of us -- and you and I both share this distinction, for whatever it is worth -- who have a foot in two camps, who cross disciplines. But the fact of the matter is, that practically all at the Institute were trained in, or think of themselves (as I do) as a psychologist who has these other interests as well. Right at the same time that we became part of Education, and I don't know what the dynamics of that were -- and I think availability of children and the close link to children was certainly one of them -- but there were perhaps some other factors too that you know of that would cause us not to join CLA and be part of Psychology. Do you have any thoughts about that? You went through that transition.

Templin: I think that in many ways the Institute, being the Institute, felt it could maintain more autonomy in Education than it could have in Psychology.

**Moore:** Either as a part of Psychology or a competitive department in CLA. That concerned me too.

Templin: We did not want a competitive department. Then, of course, at one point the Institute did a fair amount in clinical work. Florence Goodenough was very interested -- reading some of her early letters is just fascinating. They had this program, and they brought children in; it was a child guidance clinic, but that wasn't its name. It was a "parent consultation service." That is a wonderful concept, and I think it spoke very well of a concern of the Institute. As time went on, clinical work was building up so it became a department within the Institute where you work with but you are not competitive, and so forth. Even now in Special Ed there are issues that interested you and me in our research. Looking at the way Special Ed has developed, I probably would have worked much more closely with Special Ed now than I did years ago.

**Moore:** Well, it seems to me that at the same time that our faculty have identified psychology as their primary field, there also is much more cross-disciplinary research and communication, and even teaching and interest -- partly because these respective fields are mature now, and they have something to offer each other.

Templin: That's right, and they didn't initially. Also, in the early days you get one kind of encompassing the other, and that doesn't work either.

**Moore:** Should we go on?

Templin: There is one other question, on applications.

**Moore:** Do you want to comment on that, so-called applied child development?

Templin: Well, practically everything I have done is applied, with the exception of a couple repetitive studies that I have done in my interest in play activities -- and they even became repetitive. That's a sociological interest that I have had from way back. As I said before, after 1943 I didn't do any clinical work. So how does one disseminate one's work? Well, I didn't publish too much, but I did participate in many, many two-, three-, and four-day workshops and in short courses. Most of them, again, were through organizations in the various areas throughout the country that dealt with speech, language, hearing. For some, I would be the only person; some had a couple of leaders. Most of them ran at least two or three days. I remember being in New Hampshire, St. Louis, and San Diego -- each for several days. That just spread. That's an important part of your teaching activities as well as a way to disseminate your research and your expertise.

I've done the short courses at the Speech and Hearing Association conventions and so forth. As I've said, that connection somehow or other has never been lost, and they have recognized that. In fact, at my retirement Jerry Segal, who is in Communication Disorders, remembered the multiple things I got him to join. (At one point I looked in the SRCD directory, and he is no longer a member -- but you know, you can only join so many organizations.)

Jerry wrote a letter from Jerusalem in which he said that Communications Disorders had lent me to Child Development -- and that's not quite true, but there is an element of truth there.

**Moore: It's a nice thought! Okay, should we talk now about your experiences with SRCD?**

Templin: Yes.

**Moore: Your early contacts with that organization, and your involvement in meetings, in governance, and in operations. Whatever you want to say about it.**

Templin: Okay, I think the first thing that I wanted to say is that the Society for Research in Child Development has never been my primary professional organization. The APA and SRCD have been secondary to the Speech and Hearing. There is no question of that in my mind. When I came to Minnesota in '43 with this background in speech, I had been involved with the speech correction associations I mentioned earlier for many years. I really had SRCD and APA just suddenly burst on me. I was not really aware of them before.

**Moore: Interesting.**

Templin: Then I came up in '43, and John Anderson was the head of the department and the one that we had to talk to initially. He was a founder of SRCD. He was president of SRCD in 1942 and 1943.

**Moore: You didn't have to ask what SRCD meant, did you?**

Templin: No, I did not -- and not only that, in 1942 he had just finished his year as president of the APA. And Goody was president a couple years later.

**Moore: Florence Goodenough.**

Templin: Florence Goodenough. Yes, we called her Goody.

**Moore: I know.**

Templin: Actually, her family called her Fluff! But she was president in '46 and '47 and I will say that was very sad. John E., as we called him, was very sad about this. One of the first tasks that I had was helping him tabulate some election, and I don't know if it was for APA or SRCD. I had the vague feeling it was for SRCD. Goody was not elected president that year, but she was several years later. Goody had illnesses. Her hearing went down, her vision was down, and she was not functioning up to par in the years that she was president. It was sad because she was such an able person. Talk about a many-faceted individual -- well, both she and John E. were.

**Moore: Can you recall what years those were? That was as you went to Minnesota right in '43?**

Templin: I came up in '43, and John E was still president of SRCD in '43.

**Moore: Okay, so it was in the 40's and early 50's.**

Templin: Goody was the president of SRCD just a couple years later. Now, I don't know when I joined SRCD. I do know I joined with that kind of dropping into the midst of it. It was a very important time. Now you talk about biennial meetings. I did get a copy of the meetings at the Society, and I discovered that their meetings were biennial between 1934 and 1946 and then they were biennial again between '53 and '95. So this was right at that point in between, when there was only one meeting between '40 and '53, a 13-year period, and that is the time that I became acquainted with them.

**Moore: You say one meeting -- one meeting a year?**

Templin: Oh, no.

**Moore: One meeting?**

Templin: If you look at their meetings, they're in '34, '36, '38, '40, '46, '53, '55 on.

**Moore: Really? They didn't meet at all?**

Templin: They didn't meet at all. It was a period, partly of war, partly -- I don't know too much, but my guess is -- partly a struggle of the organization because the meeting that I went to in 1953 was the first one after seven years. I remember it very well.

**Moore: I would have guessed a couple of years in there, but that long I didn't realize that. I knew there was a break.**

Templin: John Hagen sent this to me. I didn't have it, so I asked him for it. The 1953 meeting was at Antioch College in Yellow Springs, Ohio, and I went to it. I don't have a program. I don't remember what we discussed in detail. I remember that it was a very small meeting and that in the discussion there was a great deal of concern about the direction and the future of the organization. What do we do? This is a critical time. Apparently, they made the decision to stay multi-disciplinary and to keep on going; this was a very important decision I think, and a very good one. All you have to do today is look at what has happened to the Association. The growth and the multi-disciplinary membership; it is still pretty unbiased, and it does have representatives over a number of fields.

The next one that I went to was at the University of Illinois and that was held at the perfectly beautiful Allerton Park setting. We were just enthralled. It was somewhat larger. I don't remember too much about that meeting except there didn't seem to be quite the crisis situation feeling. Also, though I'm not sure at which meeting it occurred, there was one speech that I still remember and that was by Boyd McCandless. He was talking on nutrition of infants -- breastfeeding, nutrition, and all -- and he made this comment, that I think very relevant in all of this applied bit. He said that we've studied the nutrition of children, we've done all of this, but the one thing that really changed the feeding of infants was the canning of baby food. I think that that is true in a lot of what we are doing, and I think it is quite relevant to some of the things that I have done: Things that are outside your control really take over the situation. I remember that very well.

The next one I attended was in Iowa -- again, I don't remember anything about the meetings except that they became bigger and more organized. I don't know if it was that meeting or the next one at Bethesda that was an anniversary meeting and they had as many as possible of the founders back. One of the things that I remember about that meeting is that's where I met Bill Hartup, who later was at the Institute. And I did attend those that were in Minneapolis. The only other one that I attended was at Santa Monica in '69 and that is, as I recall, the only one at which I presented a paper. So you can see that my association with SRCD was pretty tangential, but I did get interested in it at a very crucial time. Then in 1964 I was invited to become the editor of the Monographs.

**Moore: The monograph series.**

Templin: I accepted that for a six-year period, and that too was a very critical time for the association. Going back to this '53 meeting, at Antioch College -- with the discussion of what is our direction, the first meeting after seven years -- if you look at the records, within the next year in '54 they made some decisions. They set up a business manager, an editor. Bill Martin became the editor of *Child Development* and of the *Monographs* and the business manager, and over the next 10 years that combination became pretty overpowering. It became too big a task really. By '64, when he resigned, the association really had to look at a number of things that had to be done and faced all at the same time.

First of all, there were many changes and reorganizations, discussions of purpose, and so forth. Al Baldwin was the president at the time. As far as a business manager was concerned, Bob Hess came in about six months or so after Bill had resigned. Meantime, Marian Radke-Yarrow was secretary of the association (Again, I've known her since she got her Ph.D. here the year I first came. She was down at Iowa, she came back for her final exam, and we had a wonderful session at Goody's seminar). Marian took over the business function until they had a manager. She was

working on paying -- they gave editors \$500 as an honorarium, but that is beside the point. They also changed the press (I think from the Antioch Press, but I don't know which press had been publishing for SRCDD) over to the University of Chicago Press. Of course, when you change presses, you've got all kinds of issues -- What do you do? How do you do it? What's the system? So you have a new business manager, new press, and new editors -- Alberta Siegel took over as the editor of *Child Development*, and I did the *Monographs*. So you had all of these changes at once. Now I'll talk a bit about the situation for me, but I think one thing that made life good at that point was the cooperation of these various groups. The board of the association, the editors, the business manager later -- but the business manager was not so important as the press and the board. Alberta and I corresponded and telephoned.

**Moore: Now that's the council, right?**

Templin: No, the other editor. She was editor of *Child Development*.

**Moore: I mean when you say board -- ?**

Templin: Yes, that's the council. When Alberta and I took over as the two editors more or less simultaneously, although I think she started a bit earlier and *Child Development* was moving a little bit more than the *Monographs*. We wrote, consulting each other on consulting editors, exchanging lists so that we would tap a broad range. Of course she had many more articles coming in, and the journal had a broader range than the monographs did, so she tapped a number and I did too. Only one consulting editor resigned very early; some stayed on all the while. Then we added others, and the one who resigned came back when we got a monograph that fit into his area. But Alberta and I exchanged those lists; then we talked about what style should be used. None of this had been formalized. After some discussion we decided that the APA style was to be used, which involved conversations with the University of Chicago Press. How do you do this? What do you do? Then we talked about such things as page allotments and other restrictions we were working with. When we had recommendations for the council and the publications or editorial board, they came out of the whole agreement between at least Alberta and me (and frequently, whether Alberta and I and the Press could handle them), so that kind of thing had been worked out. I think that this systemization was a very important thing at that point.

When I got the Monographs, I found that six or seven had been accepted, but they were just lying there; nothing was being done. They had to be edited. We set up a system whereby the board approved an editorial assistant. Sylvia Rosen was there for one year. She was followed by Anne Harbour Jirasek (this was a moonlighting job for her; she was then an editorial assistant at the University of Minnesota Press). We worked out the editorial process, and we handled a great deal of correspondence among the authors, consulting editors, and the press. All of these monographs were published in 1964 with Bill Martin as the editor, but we did the editing.

The first publication that I had was the monograph on mathematics and learning. That was a conference sponsored by the Carnegie Foundation, which was willing to pay for early publication. It had been there for some time. The discussion was fascinating, and we did some interesting things. We learned something right then and there about corresponding with people, and how you handle this. The big problem was the mathematician -- I think it was Stone; I'd have to check. He was at the University of Chicago, and he was happy when he learned that the University of Chicago Press was printing it because they had all of the special type characters that were needed in mathematics. He definitely wanted all of this higher mathematical thing in.

**Moore: In the monograph.**

Templin: He was the first speaker. When we said this is just too much, his answer was, "Well, psychologists ought to know more mathematics." And maybe they should -- Well, it took us a month before a light came: our suggestion was to edit this to take out all the mathematical notation, because it was familiar to the mathematicians who would read it and it was incomprehensible to many of the psychologists, and to put it into the appendix so it could be used. He thought this was great. The memorable part was he was going to be out of the country and he was not going to be happy until he saw the galleys to be sure that the right markings were there. He sent the galleys back from Singapore or another Far Eastern town with a delightful letter and a particularly beautiful set of stamps that he had selected. This was getting to the kind of communication we wanted. We did this in many ways -- even receiving letters along the way from people that we rejected, thanking us. We did, I think, a lot.

My goal for the Monographs was somehow to make them become increasingly seen as a good channel for publication of substantial materials of research on children from various disciplines. The question was how to do this -- by project research, or conferences, or a selection of a number of things. I should say the council was very helpful. One thing needed here was a page allotment, not year by year so that you didn't know how you were going to handle a large or extra Monograph. They gave us a three-year allotment, so the editor had some flexibility. I do think that in those six years we did a fair amount to establish the multi-diversity of the possibility, and the organization got things on a good foundation. There were 43 monographs published during that time, and we still had a 76 percent rejection rate.

**Moore: So you were still being discriminating.**

Templin: Yes. Initially, when I took over as editor, a lot of the materials that came in seemed to be fresh from a Master's thesis. We suggested that some of these were overwritten and cut them down to article length. This is where the cooperation between the board, the Press, and Alberta was very helpful in ensuring that good research was published in a suitable format, whether shorter articles or longer monographs.

**Moore: Did the Monographs represent the range of concepts and the purpose they ought to serve in the field? Do you think that has continued?**

Templin: Well, I think it has. Editors are bound very definitely by what comes in, but I do think that you find Monographs that get off into physical growth, others that are definitely clinical, and some that are almost medical. So yes, there is a range, but I think the problem of how to handle it remains. I'm sure that it's something the editors have looked at pretty consistently. However, I also think that the Society has much more stability. In addition, as you can see from the membership directory, while psychologists are predominant, with educators next, there is a scattering across fields. Also, SRCD has a geographical scattering of members that is wonderful.

**Moore: And I think that the contributions to the work of the Society through publications is much broader than the membership.**

Templin: Oh, yes.

**Moore: That is the multi-disciplinary aspect of it. Because there is a lot of collaborating with people from other disciplines.**

Templin: Yes, and I think that all of that is extremely important, and I do feel that to some extent the work of organizing and clarifying that was done way back there was helpful in that it set the direction. We had no formats, no cover designs. I can't tell you the number of little details that have persisted, and some have changed -- obviously, you don't want things to continue forever.

**Moore: But you had a lot of changes in one period of time, right at the beginning of your term as editor.**

Templin: And not knowing what we had -- we had the backlog of accepted manuscripts, but no background information about editorial policy. What we passed on to Bob Sears was much better than what we received. I think today you see a more mature organization. I think that the multi-disciplinary concept was and still is sound, and we still have to work at it, but the more it works, the more it generates more. That's what I want to say about SRCD. I think the field itself is a very difficult thing to define. It is not the province of any single discipline, and if we can recognize that, then there is the problem of how to let all the disciplines know that here is a focus for this across the broad variety of approaches. It must in a way recognize that different interests in the whole picture of child development can be representative without everyone doing all of it, that people are enriched by an awareness of what others who are concerned with the same general area are doing. I think that the problem of focus in any multi-disciplinary organization or field is a very difficult one, as is the question of how much you really want to focus it. Today, for example, how much do the social workers want to take over one field? What about the criminologists and all of it? So you can't do all of it, but you can do what those concerned with research development can in this

area -- I think that's about all. In a sense, I think that my Certain Language Skills tried to do that, although I didn't know that at the time. It hits a lot of different areas within one field, but yet I think gives some focus.

**Moore: Do you want to comment on personal notes? Anything of that kind -- your personal interests or -- ?**

Templin: Well yes, I'd like to say a couple things. I guess, I myself am multi-disciplinary within me and so that's why I resonate very definitely to an attempt to reach out to those areas. It's funny: you get linkages in interrelationships all over if you have a deep commitment to one thing. Now I said that I have probably been more well known in what I have done in the speech and language area than in the child development area, but I have been known in both and in the ramifications that I am mentioning. Also, I would like to say a couple words about how I would attribute some of this "success" -- and what I mean by success here is a kind of a personal satisfaction in what you have done (and what you haven't done too I suppose) -- perhaps not satisfaction but awareness of. A good part of that is the period that my professional career covered.

**Moore: In what sense?**

Templin: Well, one reason is that you have here a time when the fields that I was concerned with were both relatively immature in developing. This was the whole period of interest in children that came in after World War I, which had gone down but was still there. This period is very important because some of the things I did were related to things like my dad's illness, the war, and situations of that sort, which combined with the level of maturity of the fields to help my finding some relationship.

Another reason is that I had found interest in two fields that were really very complementary. They overlapped so much -- none of them completely, but there are so many divisions and aspects of either field that you could have placed what I have done in either one. Another reason is my long-term commitment as a person to the same area -- not to the same thing, but to the same general area of communication within children, which encompasses behavioral things, sensory loss, language development, speech development, the whole bit. I continued in it. If people stay in a general field long enough and live long enough, if they keep doing it, they are bound to have more of an impact than if they dip into a number of different things. Finally, some of my research was important for its practical aspects because it met the needs of a very diverse group of people.

**Moore: Yes, it certainly did.**

Templin: I think that is a real comfort. And then I think that there is no doubt about it that some of this also gets into the personal characteristics of the individual. Anybody who knows me knows that I am very persistent. I will stick with things perhaps longer than they ought to be stuck with at some times, but I will. I'm adaptable; I think I am relatively independent in that if I am interested in something, I do it. I remember as a child my father said, "I don't know what is the matter with that youngster. If you say she can have something, she says okay. If you say she can't, she'll say okay -- or can she do something else." That didn't bother me because if I wanted something enough, there would be a roundabout way of getting there -- if I really wanted it. I guess I am still a little that way, and I have been quite satisfied in many ways with my own little --

**Moore: Your own mode of operation.**

Templin: My own mode of operation. There are disadvantages in that. You don't have the external kinds of connections, but they are there when you need them, and that's okay if you are self-directed, as I guess I am. So I guess that those are the things I think that we're primarily concerned with. Then, as I have said a number of times, I think I am a semi-frustrated sociologist, so some of the other things that I have done that I wish I could have done more with are the play activities, the children's literature things, how this reflects the environment, the impact of that upon the child -- but you can't do everything. The other thing that I rather regret is that I had too much basically good data that are not analyzed, or analyzed but not published, and I guess we all have that to some extent. You live with what you have done, and that's all you can say there. Now I would like to say that the same kind of commitment and so forth has appeared in a couple of other areas of my life. For example, I have been very interested in the neighborhoods and the improvements there.

**Moore: Yes, you have. You have been a sociologist there.**

Templin: For many years I was involved in our neighborhood association, and then a small group of people established two corporations -- one profit, and then a second one nonprofit so that we could accept government grants and so forth. We bought shares in the profit one. It was founded back in '64, I think, and it is still going. Now it's had a much greater impact than what it has actually done, and a lot of young people in the neighborhood are still involved in that. However, if anyone were to ask me what I had done in the neighborhood that was the most important, it wouldn't be any of those, but rather the simple fact that when changes occurred I didn't move. That I am borrowing as a choice from somebody -- he did a study on change and how you change environments and what was the most important; the most important factor was that the people who were trying to change and stabilize something didn't move. I have moved now; given the needs at my age it made sense. The other place where I had done a lot is in the church activities in my own congregation and then on aging both at the local or community and the regional level, and what the churches were doing. We did some good studies in that too -- applying some of the same things. I guess that's about it.

**Moore: Okay, well thank you very much Mildred. I really appreciate having been your interviewer. We have known each other for such a long time, and I have learned a lot about you today.**

Templin: Well, you know, I wish I could interview you because you do know people when you meet in professional settings. You ask the formal questions, but it isn't until you get beyond that that you get to know them as people.

**Moore: I think that we don't chat with each other about these things and reflect about them. We are more problem-oriented as colleagues, although we have been friends as well and reflected together about things. But it really has been a very enjoyable afternoon. Thank you on behalf of SRCDC as well.**

Templin: Thank SRCDC for including me, Shirley.