R. July 15

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~~Ruxton, Maryland 21204~~

General Delivery,
North Truro,
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July 11, 1971.

Dear Roger,

Do forgive the long delay in returning this transcript to you. When it first arrived I was quickly captured by nostalgia--both for the reminiscences and for the occasion in Washington when we reminisced--and read the whole thing through. But there were a few places where the transcript needed some reconstruction and I put it aside as of lesser urgency than other things I had to do. I was busy getting a paper ready for the SRCD meeting. And then after that I found that my application for renewal of grant support wasn't getting funded, and ever since it seems to me I have been writing grant applications.

So here I am on Cape Cod for a July vacation. This is the first rainy day--and the first time I have settled down to deal with the loose ends I brought along with me.

I haven't actually edited the transcript, except to clarify a few things that didn't come through too clearly or accurately. There were a couple of places that I couldn't make sense of the transcript--and I didn't try to fill in the gaps in your part of the conversation. I trust it is now in good enough shape to go in your archives. The whole thing was a pleasant experience for me. I'd love to see some of the other transcripts--But for that I suppose I would have to devise some kind of appropropriate problem requiring historical research!

The SRCD meeting went well. My two Research Associates and I presented a symposium on our recent findings, and I felt it was well received. Indeed I thought it made a real impact. This made it particularly exasperating to find myself without grant support. One of my applications has, however, borne fruit, and although I didn't get all the funds I needed, I have the major needs met for another three years. By this time, I hope, this project will be wound up. One of my needs that wasn't met was my own salary mark-up--but I have enough carry-over funds to look after that for another year, and perhaps one of the other two applications will bear fruit in the meantime. But the money-grubbing is time-consuming and humiliating. But necessary, alas!

Are you going to be at APA in Washington? I expect to get there for part of it--but I have a visit from my sister that may compete, and no way to get out of that. In any event I hope to see you.

All the best,

Mary

MARY (SALTER) AINSWORTH

MYERS: This is part of the Oral History of Psychology in Canada. I am talking to Dr. Mary Salter Ainsworth, Professor of Psychology, at the Johns Hopkins University. We are attending the Annual Meeting of the American Psychological Association and it is September 2nd, 1969. Well Mary, let's start at the beginning: Where were you born?

A: Glendale, Ohio.

M: I always thought you were born in Toronto.

A: No, we moved to Toronto when I was four years old. My parents were both Americans.

M: Into what sort of a family were you born? What did your father do?

A: When he came to Toronto he was sales manager of the Northern Aluminum Company, which was an affiliate of the Aluminum Company of America, and it subsequently got aplit apart and became part of that whole aluminum combine. His own particular part was Aluminum Goods Ltd., manufacturing cooking utensils.

M: Any brothers and sisters?

A: Two sisters, both younger, both graduates of the University of Toronto.

M: And so you are the eldest of three girls. If you moved to Toronto when you were four then your early education was in Toronto.

A: Yes.

M: Whereabouts?

A: Runnymede Road Public School and Parkdale Collegiate Institute and then the University of Toronto.

M: Have you any idea at all when you first heard of psychology, when it first had any meaning to you?

A: Yes, I remember it very clearly. I think I was fourteen or fifteen and I read William McDougall's Character and the Conduct of Life and that was the only year in my life I kept a diary. But that year I put down in my diary that "I am going to go into psychology". Only I couldn't spell it!

M: Just like that, eh?

A: Just like that. M: You mean this book so interested you?

A: Now mind you, I don't think the result was all that clear, because when I did arrive at university I went into the pass course because I was too young to go into an honour course, and I thought "Well, now, I'll sample around and take various courses and decide what I want to do." Then when I came along to the second year of the pass course I put psychology down as one of my five and I think I got 96 on the first term examination without even studying for it, and I thought "This is for me."

M: That's a fine reason! What alternatives did you consider at various times? Did you consider any other things seriously at that high school stage?

A: I won a scholarship to McMaster University, as a matter of fact, which I didn't take up, and I had to put down something as an intention then, and I put down modern history. But I don't think I was serious.

M: Would that be with an idea of teaching?

A: I have no idea. It wasn't a very serious thing, it was just something to put down on the application form.

M: But you don't recall ever having thought you would be a doctor or a nurse or a teacher, or anything else seriously.

A: One of my high school teachers tried to talk me into going into medicine but I found him a rather offensive individual and I think this turned me off the thought. And there was a point somewhere along in my third or fourth year when I thought I would like to go into medicine but Ithought "I haven't got time".

M: When you came to the University in your first year you were in the pass course, did you take psychology then?

A: No the first-year pass course was sort of a duplication of subjects from high school and there was a standard curriculum.

M: What year would that be?

A: I came in 1930.

M: Was the the first year of the pass course ~~a~~ Grade 13 as late as 1930? That surprises me. I thought that was over then.

A: So I took Latin, and mathematics, and Greek and Latin history, and English and French.

M: And you did this instead of fifth Form at Parkdale?

A: Yes.

M: So in your second year, which was your first proper university year, you were still in the pass course, but you chose five subjects of which one was psychology? Who did you take that from?

A: Well, it was a combination of Bill Line and Karl Bernhardt. Karl, I think, was the class assistant, but he took seminar sections and gave some of the lectures but Bill Line was the real instructor.

M: Now tell me about your earliest impressions of Bill as a teacher.

A: Well I didn't understand him too well but I was very excited by him and I think this is probably the way I felt about him until I was a graduate student and began to tune in a little bit better on him--his wavelength. He was immensely popular as a teacher.

M: He certainly was. I was his assistant in I don't know what year, but I was always amazed at the stress he showed before each lecture. He would burst out in perspiration and he would tramp up and down and get very, very agitated. Did he show any of this as a lecturer?

A: No, he seemed to get completely wrapped up in his subject and he danced around up in the front and put these little diagrams and jottings on the board. He was very graceful.

M: Was he very English, do you recall, at that time?

A: I suppose he was, but that impression has gotten buried under all the years.

M: Did he use cigarette boxes for his lecture notes in those days? I don't recall that myself but later on that was his standard practice.

A: No, I don't think so.

M: I think it is probably impossible to separate out the changing and developing feelings you would have for your teachers since you had them over a considerable period of time. But just broadly now, in retrospect, when did you switch to honour psychology?

A: Next year. I had to take five years.

M: So you were really there four undergraduate years in psychology and...

A: I went into the second year of the honour psychology course.

M: Yes, but that was after the second year in the pass course. Then you were there how many years as a graduate student?

A: Four.

M: And how many years after that were you on the staff?

A: Four.

M: So it is four, four, and ...

A: Wait a minute, three I guess. I started as a lecturer in '39 and lectured until the summer of '42 when I joined the Army.

M: Yes. Well now, don't go too fast. I want you to try and remember things that are now most relevant to you about your undergraduate experience in psychology.

A: Oh, there are so many things. I found psychology fascinating. I think I found every aspect of psychology fascinating. I can't really remember a course that I didn't thoroughly enjoy. I remember we gave Karl Bernhardt a hard time once trying to find out what the value of comparative psychology was. He seemed a little hard pressed to answer! I think I could answer it a whole lot better now, myself.

M: All this rat running and mazes. This would be back in the old Lashley extirpation period?

A: I think I have the honour of being the first, perhaps the only person that was bitten by a white rat the very first minute that they tried to run one.

M: Was that in the back room at 69?

A: Yes. I did something silly like waving my finger at this rat.

M: From whom did you take psychology as an undergraduate?

A: I remember your history course very well. I thought you taught it splendidly, that's for the record.

M: We should really exempt present company.

A: It was practical psychology that I had from Professor Bott. That's what he called it. It was a laboratory course that he gave to medical students, B and M, and honour psychology. Instead of calling it experimental psychology he called it practical psychology.

M: What are your recollections of him as a psychologist, as a teacher, and as a man?

A: Well I think my overwhelming impression as an undergraduate, and this went on into some of the graduate student days, was that he was very dry. I don't think it was until I was a graduate student and particularly when I was his class assistant, which I was for a number of years after my undergraduate days, that I appreciated the depth of his thinking. You have probably gotten this from everybody who took that course in systematic psychology. I felt he anticipated a great many things that took many years to materialize afterwards.

To skip over to Blatz, I remember being very excited about Blatz's theory of security and I went on to do research in that area. I realize that a great deal of what I am doing now stems directly from him. That was probably, in a way, the most lasting impression.

M: We'll come back to that because it is a very large element. We should try to separate your undergraduate from your graduate experience, because the whole thing now is fused together. But let's do the staff. We'll come back to Blatz because I want to hear more about what your ideas are about what happened to the Blatzian theory. To go on, who else did you have?

A: Dave Ketchum. He was a superb lecturer. Again, he was very popular, both as a person and as a teacher, because of the intrinsically interesting content of his courses and his very good presentation.

M: He was very witty and very amusing. An excellent entertainer.

A: I guess he didn't teach a course in personality at that time. It was social psychology.

M: Did you ever do any research with him?

A: No.

M: Who else?

A: Chant. One thing I remember particularly was the little experimental groups that he had with us for two years. There were five of us in his class, Gord Turner, Ron Hanagan, Helen Newberry and Ruth Marin. The five of us did an independent research project for the class as a whole. This was an undergraduate class. I don't whether this ran two years or whether it was just the fourth year. We worked with the galvanic skin response and played around with this as a lie detector. I cannot remember just what the project was for that year but I was very interested in it. I thought emotion was somehow more interesting than a lot of other aspects of psychology and I did my M.A. thesis on a continuation of that little class project--on "attitudes towards war and the galvanic skin response."

M: It would have been after you became a graduate student that you had his seminar on quantitative and statistical methods. He was, as I recall in those days, an extremely thorough and patient expounder of statistical methods.

A: What I knew of statistical analysis I learned from him but I wasn't a very apt student because I couldn't even add until I came to graduate school.

M: There you _had_ to learn! Who else was there? Gerry Cosgrave?

A: Oh yes, of course. I didn't find him quite as fascinating as the rest because he was so pedantic, so precise. It was industrial psychology I guess. I also remember he gave a course on sensation and perception, and this I did get very much involved with. Particularly theories of colour vision. I can remember one assignment he gave us to look at all the facts that need to be explained in colour vision, like colour blindness, contrast, complementary colours, and all the rest of that, and to examine the three theories of colour vision and account for all the facts as best we could by each of the theories. The object of the exercise, I am sure, was to prove that none of them really covered all the facts. When I started to work on this project I felt terribly excited about it because I thought "If you extend the Helmholtz theory just a little bit this way you can account for this fact, and if you extend it just a little bit that way it will cover that fact. I wrote up this proposal and I thought it had absolutely no holes in it and nobody could find any holes in it. I was terribly excited and I went over to the Library to see what Helmholtz really had to say and I discovered that, in a later publication which wasn't included in the textbook, he had already discovered it. I was terribly pleased because it meant I would not have to devote the rest of my life to research in color vision.

M: Did Gerry know that Helmholtz had already extended the theory in these directions?

A: No. I think it was just the way it is so often with textbooks. The things that get into the textbooks are the early publications and they don't ever get around to putting in the later publications, and people write textbooks on the basis of other people's textbooks.

M: I don't recall that Gerry ever did anything but industrial psychology, but I guess you are right. And who else would have been there in the mid '30s?

A: '30 to '35, I think it was.

M: Well then, let's go back to Blatz. But this, of course, extends right up to the present. Can you give me a thumbnail sketch of Blatz as a person?

A: Very definite. That is one of the first adjectives that occurs to me. He was very definite in his beliefs, he was very explicit in the way he expounded them and he always appeared to be in complete control of every situation. He would come in and give a lecture and it was never this business of, "Well, we'll go on to that next day", he would come to the end of what he had to say, which was just about enough for the hour or two hours and then he would pop out. There was never any business of people lingering to catch Blatz afterwards because that was the end of his time commitment for this thing and off he went to something else.

I can also remember an impression I had, and this went way on to later days: when you had a session with him to talk something over he always ended the interview. He would stand up. It was time to go and there would be no word after that except "goodbye."

I guess I don't have those lecture notes, but I wish really someone had published his lecture notes, because I don't think Blatz ever published the things that went into his lecture notes. To me they would have been a better textbook than the various books that he did manage to write. I keep hunting for things about the theory of security, for example, that I remember, and I am sure were in my lecture notes, but they are not in any book.

M: That's interesting. I think writing a book, to him, was something different. What he was thinking about when he was writing a book was different than when he was talking to students.

A: There was one concept, for example, which I have used and it is very focal to the work I am doing now. It is this business of a child using his parents as a secure base from which to explore the world. This, among other aspects of his theories, captured my imagination then. I was at a meeting in 1961, I think it was, and Harry Harlow was there and presented his work with monkeys, and he was talking about the monkeys using the mother as a secure base for exploring the open field situation. I pointed out to him that this was not a new concept to me at all. I had encountered this from Blatz. He wanted a reference because Harry is one of these people who always wants to give credit where credit is due, and I searched and searched through Blatz's writings to find any clue that I could use but I couldn't find any.

M: I am astounded. I have heard him say that kind of thing so often, we all have, that I would have supposed that it could easily by documented.

A: It's not there. I found something in the last book on human security which reflected the same idea but it didn't express it in those terms.

M: It is really strange. Well now as you say, your work ever since those days has been in the area that Blatz was working in, and I suspect that your impression of what happened or didn't happen to Blatz and Blatzian theory is different from mine. Some of us, I think, feel regretful that Blatz didn't have, we think he didn't have, the impact on American psychology that his security theory should have had--that it did not get the recognition or have the influence that it deserved. Is that true?

A: I think it is absolutely valid. But then I think there were two reasons for it. One was that though undoubtedly Blatz read a great deal, he wasn't one to quote sources. I can remember that when I was on staff finally (I guess this was after the War when I returned to teach a course on theories of personality), it gave me great pleasure to teach Freudian theory and point out the many, many parallels between Blatzian theory and Freudian theory. I finally told him this, how much fun I had pointing out the parallels, and he said, "Where do you think I got it all from?"

M: Oh yes, he had read his Freud. But as you say, he wasn't conscientious about attributing any of his ideas to anybody.

A: Although he was very anti-Freud in many ways and this is one of the things I remember very specifically from the whole Toronto atmosphere. There was this very anti-Freudian bias, which I think was rather a pity, in retrospect. On the other hand I think we got a very, very good grounding on all other aspects of psychology at the time.

M: That was terrible. I suspect there wasn't a graduate student there who didn't read Freud, but it was the style to be agin it. This I have always been inclined to attribute to the medical, and particularly the psychiatric, leaders in the Toronto area. Of them, Farrar was a very, very bitter, you might almost say bigoted, anti-Freudian. But that can't account for it all. Why was the Department of Psychology so anti-Freudian? It is not surprising that Bott wasn't Freudian. Is it surprising that Blatz was not a Freudian?

A: Well he was in so many ways. A great many things that I think of as very basic to Freudian theory, Blatz really did subscribe to. I don't know whether it was because he had reworked it and wanted to have all credit for the reworking which he had done or whether he didn't want to be label~~l~~ed as a Freudian, or some combination of the two.

M: Although he didn't fear to get himself labeled as worse things than that in the popular mind, if there are any worse things.

A: I think it was a search for originality. One of my impressions of Blatz was the enormous facility he had for spawning hypotheses, far far more than anyone could take hold of in a lifetime. You wouldn't have enough students to explore these hypotheses.

M: And certainly a lot more than he could ever be bothered to test or check.

A: But then I think one of the reasons his whole position didn't have more impact on American psychology was that Blatz himself would never be bothered to translate parallels, wouldn't integrate his material into any kind of perspective in the literature, and in his last book on "Human Security" he said something to the effect that "Obviously he was indebted to many many people for his ideas but the people who know the literature will recognize them, and I am not going to be bothered to give a bibliography".

M: Very cavalier.

A: And then, of course, his devoted followers followed him in this until I think the whole security theory became a private language, and it just simply didn't communicate itself to anyone else who wasn't reared in the tradition.

M: You may have followed it when you were there, but you didn't follow it later after you left there. I have always thought that having a tame household journal like the Bulletin of the Institute of Child Studies combined with the fact that what he did write he wrote in books--still, I am a little puzzled as to why the books didn't have more impact...

A: They were popular. They were nearly always written for parents.

M: Certainly the early ones were.

A: They weren't written for professionals.

M: The fact that, to my knowledge, he didn't publish anything in the regular flow of journal literature seems to me to be another reason for the back yard isolation.

A: And you know, it is terribly hard when you submit a paper and it comes back from the editor with all sorts of suggestions for further changes, but there is something to that kind of discipline. His group never had it in the publishing they did.

M: Recently I have had--of course you know the sad recent history of the Institute--but this summer I was asked to advise the Institute on what to do about their records. I don't know whether Mary Northway thinks she has all this stuff on computer now or why, but the proposal was to burn all these records because they just haven't any room. But this seemed to somebody to be something that shouldn't be rushed at, and so I was asked to look into it. I haven't had time yet but I will. But I myself was puzzled. What would be a good thing to do with those records if they are not to be destroyed?

A: Well, I can remember Bill referring to this as a "gold mine." If somebody would only delve into those records why the answers to almost anything would be there--any kind of project you ever wanted to do, the data would be there to work with.

M: Yes, but you don't believe that, do you?

A: No, I don't believe that because so little can be done with somebody else's old data, especially data collected at an earlier time with earlier methods, earlier hypotheses and problems. The only reason I am hesitating in the slightest is the fact that the Berkeley study has, after these many, many years, been mined, and I am interested that people with new problems are going to Berkeley study data and using it for their own purposes. For example, there is a chap by the name of Gordon Bronson at Mills College, who has recently done a study on infant fears that he used the Berkeley study data for. Mind you, he is going on to do his own study now. He also used my longitudinal material. The material is being used--of course it was somewhat different from the Institute of Child Study material--so it is possible that people might be able to mine the records for specific things that are needed. Maybe three or four years ago I would have said, "No, it isn't of any value." But now this makes me hesitate a little bit. Perhaps there may be found some use for archives.

M: The thing that occurred to me was that, rather than burn the stuff, maybe it should be offered to the children, or to their parents who might be interested to know about their children back in those days.

A: You know there was a time in the late twenties and early thirties when Blatz and his group did have an impact on American psychology and this is still reflected in compendia on psychology. Two instances of this have come to light. One was when I was preparing my first big grant application for a project I wanted to do. I intended to use a technique that I termed "critical situations." The idea was that whenever one of these situations occurred, it was likely to be one in which attachment behaviour would be evoked. This was to be specially observed and coded later. I was groping around for some way to give this prestige--to quote this method as having been used by somebody--and I looked up Mussen's Handbook of Methods in Child Psychology and I found "event sampling." I looked up "event sampling" in the index and there I found myself on a page that referred to Blatz and Millichamp, and Blatz, Chant and Salter!--who apparently used this method away back when. This gave authenticity! The other one occurred when one of my graduate students who was my research assistant also--and this hasn't been published yet because it is part of our work now--did a review of the literature on crying. There isn't a large literature on this. A good deal of it goes back to the thirties, and again she came up with Blatz and Millichamp as being the standard thing on infant crying. Now that observation in infancy is again popular, the thing to do, and naturalistic methods are again....

M: Having to be employed to some extent...

A: A lot of that early work is now relevant. It didn't seem relevant for a good many years.

M: Blatz himself, and his group, never did very intensive observations on infants. It was at two years and up that they began their intensive observations.

A: That's right, but nevertheless they did do a little bit of this, although most of it was records that parents had kept. But even so that was very much...

M: Yes, as they move back to the direct observation of infants from zero on, they become interested in what was done before. Well now, another interesting aspect of this is that Blatz did teach summer courses at Iowa and Michigan. He must have been known. From what we knew of him as a bright, witty, entertaining lecturer he must have been pretty successful, since they kept asking for him, and he must have had some kind of an influence, perhaps more than we realize. And perhaps this is a reflection of it. Is this likely to be the source of these references in the compendia that you refer to?

A: No, I don't think so. I think it was the literature.

M: How did they find it? Out of his books?

A: No, it was in the maligned University of Toronto Series on Child Study. And if you remember, Iowa had a similar Series, and I think Berkeley had a similar Series. This was the thing for Institutes of Child Study to do, and everybody read everybody else's at that point.

M: Yes I see, and it meant the libraries would have them then.

A: At that point there was very much more communication. This was a very descriptive, normative kind of approach that got lost entirely later on. It became a sort of testing approach, and later an experimental approach, and I think that it is only now that some of this earlier work is getting a little more appreciation than it had for a good many years. So there was this early quite exciting period when the Institute of Child Study was in the mainstream of child psychology. Then people moved in other directions, and the Institute kept on its own direction and lost contact.

M: Well, of all the things going on at Toronto, and of all the people on the staff at Toronto, I would suppose that Blatz was the most likely to have had a significant impact on American psychology--that is, I am talking now of the older generation of the Toronto staff in the thirties. Sperrin Chant didn't write much, Bott and Ketchum were both such perfectionists that they... But I was astonished recently when I had to work up a bibliography on Bott for Carl Williams who wanted to find something (we never found it because Bott never published it, something that Carl remembered from a lecture, on the time continuum), I was astonished when I dug out the President's Reports and made a bibliography of Bott's publications and found how many he had. I think it was something over 40 publications! I would have said that he hadn't published more than a dozen at the most during his lifetime, but he had done quite a bit really. Dave Ketchum didn't and Bill Line didn't.

A: Bill Line's influence was personal.

M: Yes, very personal. But he showed great promise in his writing. I ran across a reference recently to a 1931 article of Line's in which he is quoted as making--and I must look it up because I don't believe it, it must be taken out of context--a very critical attack against the Gestaltists. I always felt that he was very sympathetic to Gestalt.

A: So did I.

M: He is quoted as just lambasting them. They never had a significant idea or made a significant contribution of any kind.

A: Oh really? I am astonished.

M: Yes, I am too.

A: I am under the impression that Bill's Ph.D. thesis under Spearman was the basis for the Raven Progressive Matrices that were developed in England.

M: Penrose once told me a great story about Raven's Progressive Matrices. I have forgotten the details of it but apparently Penrose's eldest son was the one who drew most of those designs for Raven. His son was a chess master. Raven would never acknowledge credit to Penrose's son for having drawn those ingenious diagrams. He was a notorious paranoic.

M: What is the funniest thing that ever happened to you at Toronto in psychology?

A: Well, I think the thing that occurs to me is a whole series of funny things that happened to me while being Bott's class assistant. One I remember was at a big lecture to the medical students. It was a cold winter day and the pipes were very noisy. During most of these interruptions Bott would just go ahead talking in his ordinary quiet tone of voice on, on and on--imperturbable--and I began to get really quite giggley over his imperturbability when no one could hear him at all. All of a sudden, as though he sensed my mood, he said, "Miss Salter, see that that noise stops." I went out in the hall and wondered how on earth I would see that this noise stopped. I waited out there quite a little while, indecisivly, and finally went back in and the noise had stopped. He said, "Thank you, Miss Salter."

M: That was one of your lesser miracles as an assistant!

A: There was a great cabinet full of demonstration materials in that lecture room too and every year, at the beginning of the year, the task of his class assistant was to catalogue or inventory all the material that was there. It was demonstration material and a lot of it wasn't used any more, or at least it hadn't been since I had been his assistant. So every year I would catalogue these things over and over and over again. There were a couple of things that always bothered me, I didn't know how to put them in the inventory and I think I just left them out the first few times. Finally, in a mood of pique, I put down "three indescribable objects." Later on in the year he said, "Miss Salter, where are those 'indescribable objects,' I want to use them?" This was the way he had labelled them in his own mind too!

M: He loved to use illustrations where the puzzle would be "What is this?" I guess the really comical things that happened to you were in connection with Bott. He was an eccentric individual.

A: I think I sought to be his lab assistant in the experimental course too, because I had been told, and I think with some reason, that the way to get ahead in psychology was to become an experimental psychologist.

M: This would be Sperrin's influence, wouldn't it?

A: Sperrin's and Dave Ketchum's.

M: Is that so? He was always very interested in the "hard" scientists. It may be dull stuff but these are the guys who are going up.

A: "Don't model yourself on me," said Dave. "This is a mistaken model, if you want to get ahead." So I had a terrible time with the apparatus. I don't suppose anyone is more inept with apparatus than I am, but somehow or other we managed. If you are told to "fix it" when all the Kymograph apparatus goes off all at once, well, you fix it.

M: That assistantship with Bott got you in with the medical students and that in turn, I suppose, got you tuned in on the medical selection studies you did.

A: That's right, and that got me into writing a book with Art Ham. This was early in the War and I think the proofs came out after I had joined the Army.

M: Well, let's get on to the War now and your involvement in it. How did that start?

A: Wilf Wees phoned me one day and asked me to join the Ordnance Corps. Apparently they were going to use a lot of CWAC personnel in the Ordnance Corps and Wilf was determined that this was going to be the perfect demonstration of personnel work. I don't know why I joined up.

I think probably because at this point you felt that you wanted to cast your lot in with everyone else and so I did. This was in 1942. But it never had occurred to me that Bill Line hadn't been consulted on this, because he was currently starting personnel selection for women. Bill was very annoyed when he heard I was all hooked up with the Ord nance Corps. He managed to pull strings to get me transferred over to personnel selection. So I did get switched over. I think I had about a week or two in Ord nance Headquarters before I was switched over.

M: And your first posting was to Ottawa. Was that your permanent base during the War?

A: Well, first I was sent to Kitchener on field work. One of the reasons I really was looking forward to the Army was the opportunity to put some theory into practice and to gain some experience. I guess the only other thing I had had of that sort was the summer's interning at Orillia.

M: I had forgotten that you were an intern at Orillia. How did that happen?

A: Well again, I wanted to get some practical experience, and I was still toying around with the idea of clinical psychology at that point. But I am afraid that summer at Orillia sort of finished that for a while. It was a very unrewarding spot I found.

M: Who else was there beside yourself?

A: Frank Co burn was the other psychological intern, he was a medical student. Hubie Goodfellow was the Director. But anyhow, I was posted to Kitchener and I was very enthusiastic about this experience as an army examiner. I enjoyed this very much though I was only there about three weeks. I think the next time I managed to get some practical experience was when I volunteered to do some work with the Department of Veterans Affairs after I got back to Toronto. I think I was working a day a week out at Sunnybrook Hospital doing testing just for the experience. At this point Bill asked me if I would be an assistant to him as adviser in psychology for the Department of Veterans Affairs. I don't suppose it was until I got to Baltimore that I ever was able to be a practicing clinical psychologist. I always got involved in administration.

M: Now, what about the psychologists in the Personnel Branch, who were they, what were they like, and what was that experience like to you?

A: My first impression was, when I heard that Bill Line was heading up Personnel Selection in the Army, "Heaven help us, because he is so impractical, he is so idealistic. This can't possibly work." The thing that never ceased to amaze me was how well it worked, and how much of how it worked was Bill's doing. An absolutely magnificent administrator he was.

M: That's news to me. As a professor he was disorganized and I always attributed the fact that Personnel Selection went well to the staff he collected around him. I always thought it was Doug Smith or Blair and people like that.

A: That's part of being a good administrator, to get good staff. And he also had vision.

M: Well, he had lots of that, and lots of enthusiasm and imagination.

A: I suppose probably the most effective thing about the whole thing was the way that Personnel Selection was set up in the first place, in terms of the regulations, and I think probably Brock Chisholm had a great deal to do with this. The key thing to it was that every district army examiner had a direct line to Headquarters and so things really could get done because you didn't have to go through a whole chain of command. Ideas, complaints, urgencies got taken up very quickly and were dealt with very quickly. Bill also knew all the right people, he was very diplomatic and knew exactly how to get his own way on behalf of anybody in his whole network of Personnel Selection staff. The personal devotion that everyone had to him was really remarkable.

M: And he would be personnally liked by these people from whom he had to get things. They respected him. One of the characteristics that used to bother some of us, particularly Wilf Wees, was Bill's tendency to identify with the person he was talking to at the moment. When you were with him he always loved you, and you could feel this warmth and this intense interest in you. But as soon as you walked out of the door, and another person walked in, you disappeared out of the picture. At least this was the impression we got, that it was whoever was with Bill that occupied his full attention to the exclusion of everybody else. Did this show at all in the Army? Would this cause difficulty?

A: I think the main difficulty was felt to be that if you could get to Bill yourself, everything was all right. But the trouble was that he was protected by various staff people, and if there was disgruntlement it was deflected onto those other people who you worked through to get to the Director. I don't think it really became noticeable until after the War when all these people that had a feeling of tremendous affection for him came back and tried to re-establish it and found it difficult. There was real personal disillusionment at that time. Bill could no longer do things for them the way he could before, because before the whole thing had been in his hands.

M: Yes, there were a number who felt rejected by him after the War.

A: I was his consultant for the CWAC and I didn't get in to see him very often because of having to run the gauntlet, but I always had the impression, as soon as I appeared on the scene, that he had been thinking about CWAC matters. He understood completely whenever I made a request for something or said that something ought to be done. He would somehow have the background or, often enough, before I opened my mouth to say what I wanted, he would say, "Now I have been thinking we ought to do such and such" and it would be precisely the thing I had come in to see him about. So he managed to keep an awful lot of things rolling and a lot of problems to the forefront, going from one to another without skipping any of them. And again, you would get caught up in his vision of what the CWAC was: "With the manpower problem the way it is, personnel selection for the men can't work the way it should work, but it can for the women. So here is our testing ground. Let's try to put this ideal really into practice with the women." Well, you know!

M: Yes, the Holy Grail. I don't think there is any doubt that, in terms of Bill's own life history, the two peaks of his whole career were the First World War from which he never really recovered, and the Second World War in which he reached a level of achievement in his own terms that he never experienced before or after.

A: And never could manage to implement afterward.

M: That's right. So he therefore became really a tragic person after that. But he was certainly going great guns during the War.

A: He kept groping. First of all, it was the World Federation of Mental Health. For a while he thought that might be it. And then there was the Thailand affair, and so on. But he never could find the organizational set-up.

M: That would give him a chance to do whatever he wanted to do, that's true. ~~Now~~ Then you were with him in Veterans Affairs afterwards?

A: No, I was with Chant. Well I started off with Chant in Veterans affairs. He was the one who made the appointment. But he left very shortly after I came to go to U.B.C. and then General Burns became Director-General of Rehabilitation Services. I was only there for a year. I got the thing set up for women's counselling and then decided that the fun had gone out of it, when it was all set up.

M: Well then, what led you to Johns Hopkins? Oh, wait a minute, don't you have a period in London before.

A: What led me to London, what led me to Africa, and then what led me to Johns Hopkins! Well I got married and it seemed entirely sensible to leave Toronto because Len wanted to do his Ph.D. I think it was largely through Bill Line that we went to London. This was his strong recommendation. I looked up a woman by the name of Edith Mercer who had been my opposite number in the ATS, and whom I had gotten to know in the course of one of these officer exchanges during the War. I looked her up when I arrived in London. She knew I was hunting for a job. One day she phoned me up and said, "There is a job that looks exactly as though it was hand-tailored for you. It is advertized in the London Times." I answered the ad and that brought me to John Bowlby. It was as accidental as that. I had originally written to the Tavistock Clinic and I wrote to Elliott Jacques whom you will recall, but he was in the adult department and this job was in the children't department and he didn't know about it. So I had gone over there without any job. I had written to the Maudsley also and the idea was to look them up when I got there. I think I did have an appointment with Eysenck before this Bowlby thing came up but I had the Tavistock job before I ever had the interview with Eysenck. Just think, I might have been working with Eysenck, in person! I can't imagine that!

I was very interested in this because the advertizement was for employment as a research psychologist studying the effects on personality development of separation from the mother in early childhood. I had gotten on to this as a real point of interest through the work I had done on the Rorschach. When reading some of the Rorschach literature, I found that

Bill Goldfarb was using the Rorschach in conjunction with a study of maternal deprivation. It was one of the classic studies. So from the beginning the project captured my interest and it absolutely changed the whole course of my career. And I think it is very, very funny that I learned how to do research from Bowlby--a psychoanalyst I began to realize it wasn't so much a matter of methodology, it was knowing how to formulate a problem. It somehow or other brought into focus all these other things that I had always been interested in but had been trying to work with at the college-student level when they really were pertinent to the infant or early-childhood level. And somehow or other (and this was a criticism really of the Institute of Child Study), I hadn't ever been directed, as it were, didn't even think of starting direct observations of infants and young children, but rather to do it with older people, retrospectively, and by tests and measures and so on.

M: I have the impression that the atmosphere during the thirties at Toronto was one in which it didn't much matter what problem you wanted to study. What mattered was whether you had an airtight method. We never objected to anybody who wanted to do anything provided they did it well--did it "right." The selection of a problem or the definition of a problem was far less important than that the statistical and quantitative treatment should be up to scratch. So I guess you are not alone in that kind of experience of discovering later aspects to research that didn't loom very large in our training.

A: Also, if you will recall, in the thirties no one was getting promotions, no one was getting raises in pay and everybody was scrambling to supplement their salaries one way or another, and when I was there staff were not doing research.

M: Oh no, no. They were too damn busy trying to earn a living. We were doing all kinds of things outside. That's true and that had long-term effects on our trainees.

A: So it was never a matter of getting in on somebody else's viable programme.

M: No, because nobody had a programme. Now you were with Bowlby how long on that project, two years?

A: Three in London and then I continued it a bit in Africa. Len got his degree and he didn't see anything for him in England, the academic situation being what it was then.

M: Did he do it with Eysenck?

A: No he did his degree with Roger Russell who was Chairman at University College.

M: Now, tell again the bit about your supervision of your Ph.D. thesis.

A: Well, it started off with Blatz and Chant being the supervisors jointly. As a matter of fact it went back to my M.A. thesis and the final examination on that. Blatz had led me Socratically, as he would, out to the end of a limb, and finally I knew that the limb was going to get chopped off if I didn't give the right answer. I gave the right answer and afterwards he came in and asked me if I would do my Ph.D. work with him because he liked my idea. So I did and found him very congenial. So that's the way that started. I worked from 1936 to the Spring of 1939 on this project. I was in the process of writing my dissertation and had the first chapter written when Professor Bott called me in one day and said, "Miss Salter, I am now your thesis supervisor." He asked me what I had done and I told him that I had gotten the first chapter written. He said, "Give it to me. I would like to go over it. Come in on Saturday morning and we will discuss it." For some reason or other, I had written this first chapter in a way I had never written anything before. And I certainly haven't written anything this way since. But I was so impressed with the need to use terminology carefully and be sure that the words chosen were exactly the right words that I had done this with the aid of a dictionary and a thesaurus. To my astonishment, when I went in that Saturday morning, Bott said, "Miss Salter, you are using your terminology loosely." He had two sheets of foolscap, the kind with fine lines, with a line down the middle, and he had listed words--about 200 words--and we went through them systematically. It took us four hours and I defended every word. I was getting more and more impatient and more and more angry when we came to the word "security" which I had defined in my own way, because it was a key concept, and I had defined it in a sort of Blatzian way. He said, "That is <u>not</u> the meaning of the word 'security'. Don't you know the original meaning of the word security?" And I shouted, "It doesn't matter what the original meaning was!" I was really angry at this point. He said, "It does matter." He implied that a word was a word and it never really lost that original meaning. And you know, he was right. He told me the word "security" had a Latin derivation and meant sine cura (without care). So that was the end of that. Finally I came up for my Senate Oral. Professor Brett who was the Dean of Graduate Studies was chairing the meeting. The questions went the whole way around the table and at the end of two hours it came back to the Chairman. He said he only had one question to ask. Did I know the derivation of the word "security"? I hesitated because I thought, "Supposing the old boy is wrong" and also "I hate to give him the satisfaction" but nevertheless, I said in a small voice very hesitantly, "It comes from the Latin sine cura (without care)". The old boy was gratified. And Brett said, "Oh, I am surprised. I didn't know that the Psychology Department were up on their classical education".

M: That's an interesting historical allusion. What is your story about why Bott never got a Ph.D.?

A: The story I got was that he was told to read the pre-Socratics in Greek and he didn't think it was necessary to learn Greek to read them and therefore his work in Pre-Socratic thought wasn't acceptable because he hadn't gone back to the original.

M: Have you any idea where that story came from?

A: Sperrin Chant told me.

M: Well that's the story and I think it comes from Chant and I have got to check it with him. I have a tape with Bott and I asked him for the story of why he didn't get a Ph.D. and it was nothing like that. So I told him we always believed that this was the story and I told it to him. He had never heard it before.

A: That is an interesting comment, itself, that no one has felt free to tell him this, or to talk to him about it.

M: I am not sure that they didn't, mind you. This was just a few years ago and his memory was not too good. But I was astounded because I just knew that this was why he couldn't get a Ph.D.

A: I think I knew it at the time because I didn't miss that allusion of Brett's.

M: No against the background of   you would get that implication from it. Bott's story is a plausible one that old Daddy Hume just lost it and they couldn't find it. And he was so busy with his war rehabilitation stuff that he just never went back to it. It did turn up years later and I have it in the Archives. But I would like to track down the authenticity of that story about why he didn't get his Ph.D. Anyway, I have no doubt that when Bott called you in to take over the supervision of your thesis, he knew that Brett would be the Chairman and that Brett would do something like that. He didn't know which of those 200 words, but he knew that this was a characteristic thing that Brett would do. He would take some word that had been widely used and go for the classical basic meaning. He could do a good job there. How consciously,I don't know.

A: Bott was always very interested in these--remember "inter est"

M: Oh yes. He just loved tearing words apart. There was a discipline to that that was tremendously valuable. I may have told you that one night when I was visiting Lew McCurdy and we were just yarning about old times, Lew said, "Let's each take a piece of paper and write down the name of the psychologist or teacher who has most influenced our own stance in psychology. The most potent." So we thought for a while and we both wrote down a name, and we found we had both written down "Bott." Yet, at the time, the fury of those people getting out of his systematic seminar, the anger that you generally experienced, and the boredom! Certainly, at the time, I am sure none of us would have attributed to his seminar in systematic the influence that it subsequently had. It makes you wonder whether these popularity contests now are a very valid basis for deciding who is doing good teaching.

Now to get back to Bowlby. You mentioned the fact that he did something to your understanding of research that has been important to you since.

A: When I first arrived it was for a project that actually never did materialize. They had undertaken a follow-up study of sanatorium children who had been admitted to the sanatorium sometime between the ages of 1 and 4. They had come from intact families and were returning to intact families. These children, although they had pulmonary tuberculosis were not in pain and the illness was not presumably the traumatic thing about their hospital experience, but rather the fact that they were separated from their families. He expected to find a higher proportion of children incapable of affection and also expected to find a high proportion of juvenile delinquents because all the systematic case studies that had been done suggested this as a very strong hypothesis. He had expected this to be so conspicuous that he used very crude methods of assessment--teachers' ratings--and centred ~~in~~ on the I.Q. Actually there was nothing in the I.Q. The I.Qs of these children were not lower. And the teachers' ratings were not sufficiently sensitive really, to turf up any very conspicuous differences. There were some statistically significant differences of the same kind found in other studies, such as Goldfarb's and in the direction of certain hypothesis about the effects of deprivation. But one of the things that he had thought was that he would have a battery of projective techniques given to these 60 children. I couldn't see any point in doing the battery of projectives on the 60 without doing it on the control group and the control group numbered 180. This was just too large and massive a thing and I rather discouraged them from going ahead on this project and suggested that we should get going on the direct study of children during separation that had been planned as a next step. So for quite a long time I was kept on to busy myself somehow because they weren't ready to go on with this. And eventually I was put on to analyzing other people's data.

M: Did you do much direct observation of these children?

A: I didn't do any and there wasn't any direct observation being done then. One of the things that I was asked to do was to read over records of field work observations that Jimmy Roberts on had done. I was tremendously impressed with this material. Jimmy was a social worker at the time but he has since been qualified as an analyst. His observations were the most sensitive, direct observations I had ever encountered. I don't think I have ever encountered anyone who was more perceptive. He had done what was considered to be a pilot study on children experiencing separation under various conditions. Some of them were very long-term separations that he took up at the end. Some were short-term separations. He had even gone all the way through a long-term separation with some of them. Then a certain proportion of them had been followed up after they had gone home and their response to reunion had been recorded. Jimmy, himself, felt that this material was very much too unsystematic, uncontrolled, unscientific, to be of any value except as a background for himself. I went through the thing and I thought it should be published and did, in fact, write a draft of a book that was going to publish this material. But it has never been published because John Bowlby said, "I think I had better write a chapter on theory," and the book has not come out as of 1969. So, in effect, the thing that had the great impact, almost more than anything else, was Jimmy's field observation methods and this whole approach of "Let's get back to this early period and actually look at it directly." But I was not considered by Jimmy, who was very influential at that point, to be capable of doing observations myself, because as a psychologist, I hadn't been trained. So Rudolph Schaffer and I, who were the psychologists on the team, were not permitted to transgress into direct observation.

Then we came back to Toronto. But just before we left England my friend Edith Mercer phoned Len up and said, "I know you have been wanting to go to Africa. There is a job advertised in the London Times that I think might be interesting for you." So Len applied for the job. The selection procedure wasn't really set up and we had to leave because it was hard to get shipping. We hadn't been home more than a week when a cable came offering him this job. So that's how we happened to go to Africa.

M: I remember that. I remember how excited you were.

A: Although for the first little bit in Africa I continued working on the draft of this book it never got published. As soon as I arrived, people who were at all knowledgeable--pediatricians and so on--said, "Oh, you have been working with John Bowlby. You must do a study here in Uganda because here it is customary to separate children at weaning and we are convinced that this is the explanation of the African character which we consider to be rather affectionless." I wasn't at all impressed with this hypothesis because I didn't think it was likely that the Africans were affectionless people. Nevertheless it did seem to me that here was a lovely opportunity for an experiment of opportunity. That not all would follow the old custom. We could observe babies for a few months before weaning in order to get some sort of baseline of what the relationship with the mother was like and then they would be weaned and some of them would be separated and some of them wouldn't be. And I would follow up the contrast. I wasn't very far into the study before I began to realize that I had been misled. Separation did not take place at the time of weaning. Besides, when mothers were asked what their intention was in this regard they said they weren't going to separate their baby. It turned out that they couldn't see why they should terminate what they considered a very valuable thing for the baby when they could participate in a research project which got them medical attention and all sorts of benefits.

M: Even if they might have otherwise, they weren't going to lose that.

A: So at this point I shifted into a study of the mother-infant attachment relationship. How it got established. That was the beginning of what I have been going on with ever since. Just before I left London, Bowlby read a popular book by Konrad Lorenz called King Solomon's Ring and all of a sudden ethology and its pertinence to these phenomena that we had been occupied with struck him. He continued pursuing this. I unearthed, not so long ago, a most interesting set of correspondence that I had with Bowlby in which I criticized it right and left, this whole proposition, adhering to what Bowlby now calls a "secondary drive theory of the origin of attachment". I didn't see how you could look at it in any other way and also there was the Blatz dependency ingrained. Then when I started to do these observations on babies, the first direct observations I had ever made of young children, I found that Bowlby was right. I am the product of an overnight conversion experience that this was the way to look at it, not the other. I don't think Bowlby realized for many years, that this conversion had taken place in me.

M: The last he had heard you had resisted any such idea.

A: It wasn't until the Spring of 1960 when he was visiting in Baltimore and he spent a day with me and I started to tell him about this African work which I hadn't yet published and the things that I thought were important about it. Then he began to see the relevance and since then we have had sort of a loose partnership. We see each other about once every two years and compare notes. Our thinking has been parallel.

M: Explain to me a little bit more about the relevance of ethology and Lorenz, is it the imprinting part of that stuff that struck Bowlby?

A: The implication is contrary to the whole psychological tradition and the whole of learning theory tradition which held that an infant becomes attached to or dependent upon his mother because she fulfills his drives and she is the person through which all his gratification is received. Also we talk about socialization as though the human animal has to learn to become social. The implication of ethology is that the predisposition to be social is there and that the infant becomes attached, not necessarily because he is fed and clothed and kept warm, but he becomes attached to the person with whom he has most interaction. -- the most contingent kind of feedback.

M: And at a particular stage in his biological growth, is that important?

A: Yes, I think that was implicit in the whole thing, although no one had it pinpointed. Certainly the first year of life was implicated as a critical or sensitive period. This is where all of this business on separation and deprivation was relevant because from all of that literature you knew a critical period couldn't be cut off at the end of the first year because adoptions that take place at 12 months seem to be reasonably successful. But certainly by the end of 3½ years it seems to be too late to form attachments for most children anyhow, because of findings by Goldfarb on institution-reared children. Somewhere in between 3½ years and 12 months there must be the end of a critical period and I don't know when it is. My best guess would be 18 months but nobody has done the relevant research yet.

M: So what happened to you in Africa as a psychologist is that you got on what has virtually been ever since your main line of interest. It was a not unimportant event in your career. And this cultural difference and the opportunity to observe, within a population of infants, both cases with and cases without, that was initially what you hoped to do, didn't work out?

A: The separation thing looked like an opportunistic thing to do at that point. It would be a nice neat little thing that you could do in a limited time, when you were only going to be there for two years. But I think that almost everyone who has been working very deeply in this area of maternal deprivation or separation asks the question, "If this period of life is so important, as we can tell from these studies, what goes on in the first year? What does the child, in fact, learn from his / mother? How does it learn it?" I don't think that it is any accident that most of the people who forged into this work on early infancy have been people who started off with deprivation. I think of Rudolph Schaffer who, as soon as he left the Tavistock Clinic began ~~n~~ direct observation of infants. And Lee Yarrow who obviously came to his work with infants because of his previous interest in separation and deprivation.

M: Do you think the real impact of this would to make direct observations on the It is not very obviously related to the perception work and that has been quite popular.

A: No it is not obviously related. This is another thing about Bowlby's and my going along in parallel, at some point I felt I must inform myself about Piaget. Since I am interested in early infancy I should know about him. When I really dug into Piaget's sensory motor period I found that it is completely congruent with our kind of work. What he called a schema is essentially the same as I would call a behavioural system. Even his original list of reflex schemata is very much the same list as you would have of original attachment patterns. These are the behaviour systems that are implicated in the development of attachments.

The World Health Organization have organized a series of seminars that are built on the model of the Joseah Macy Conferences on Infancy in which Bowlby and - - - - and Piaget and I don't know who else have met from time to time over a period of years to exchange ideas. [At this point -Bowbery) also Piaget and had pretty well I would say based his theory on it because of it one component .] So I found all of this relevant to perception.

M: Yes, I can see that perception. But what I was really talking about was the ~~lessen~~ kind of versions of minute and detailed observations of the child's and infant's eye movements and fixations of this kind.

A: I think probably both lines of work have emphasized in rich detail that this is not something that you are going to find the answers to by something crude and quick and careless. That you have to study this in great detail. But they are concepts that are worth going into.

M: Then what happend next?

A: Well, it wasn't the London Times this time. We were in Africa and heading back. It was Leonard Doob who advised Len that the best way to find a job from Africa was to consult the APA Employment Bulletin. So it was through the APA Employment Bulletin that Len got placed in Baltimore and I came along as a wife. After we got our visas cleared up and moved in and had the visits with the family over, I decided I had better find out about getting a job. Because of my Toronto experience I assumed that the person to go and see about any psychology job in the entire area would be the Chairman of the Department of Psychology. So

I turned up at Hopkins to make an inquiry about whether they knew of anything, only to find that this was not the case at Hopkins. They didn't know what was going on anywhere else but at Hopkins, and they certainly didn't have the corner on psychology in the Baltimore-Washington area. But about two weeks later I was called in by the Dean and offered a position. I think this was a rather unusual way to find a place in a rather prestigeful university! It just so happened that at that point they were interested in somebody who had a clinical background, not that they wanted to establish a clinical programme, they had no intention of doing that, but they thought that it would be a good idea to use one of the hospitals as a field setting where students could get experience to round out their general experimental education. I arrived on the scene just about the time that this was being discussed and explored and the job was patched together for me.

M: Who was Chairman of the Department?

A: Tex Garner

M: And you went to see Tex but he didn't have the job for you until his Dean called you in?

A: It had to be done through the Dean, but Tex started the ball rolling. Wilson Shaffer was the Dean and it was he who was particularly interested in this little clinical scheme of things anyhow. So for the first five or six years I was at Hopkins I had a full teaching appointment and also was in charge of psychological services at the Sheppard Pratt Hospital. I could not see my way clear to doing the kind of research I wanted to do with that split effort in two settings. I got myself free of the Sheppard-Pratt obligation in 1961 and was able to start in on this infancy work again. As usual I had everything backwards because everybody assumed I was a clinical psychologist because I had written with Klopfer that book on the Rorschach technique and I got my first real clinical experience after I got to Hopkins! When my friends from here heard I was at Hopkins they said, "How did you happen to go there? Don't you find it uncongenial?" (because it was known to stress experimental, not clinical psychology). In retrospect, I think it was a very very fortunate place for me to have ended up in because it was one of the few American universities that was not learning-theory dominated.

ide 2, tape 2 → Although the Hopkins' Department was experimental and my investigation of mother infant interaction is problem which calls for a naturalistic approach, there was never anything but the most supportive kind of encouragement. Which I needed terribly because at that point it was hard to get grants. No one would accept this approach as legitimate so I had a hard time getting started. But I got lots of support inside the Department.

M: So that has been good. From all the reports we have you are very firmly ensconced in that Department and very highly regarded by your colleagues. This must have been a two-way street--this cooperation and this understanding.

Looking back over the approximately 35 years from when you were first a student of psychology, there are a couple of things I am curious about. You are a woman. Has your experience as a psychologist led you to believe that women are discrimated against, or that you have been discrimated against? Have you been discriminated against? If so, how?

A: I think I have been discriminated against in terms of salary. Not otherwise, though. I haven't found any other handicap in being a woman.

M: Promotion?

A: I don't think that has been part of the picture.

M: I suppose the salary discrimination is a hangover from an earlier stage when I suppose you got a lesser salary than somebody of an equivalent age, and competence, would have got.

A: Partof that could be explained in terms of having moved around and not having gotten myself into the American academic stream until many years after I had got my degree. This I understood. But it is still lagging and it is not the fault of the Chairmen of the Department who have consistently put me up for an amount to even it out.

M: Is this a general university thing that happens?

A: I am the only woman full professor in the Faculty of Arts and Sciences. There are only about 6 assistant or associate professors. The answer always was, when the question of salary increases came up, "Well, she is a woman. She doesn't need it." The offer from Toronto was the first really big occasion for improvement."

M: I'm glad it accomplished something for you. It didn't accomplish much for us.

A: This is a fact of life. You have to be sought after by other places, and be willing to go and be earnestly considering it, before the leverage is brought to bear. There isn't any point in bluffing. You have to really mean it.

M: It seems to me that every time bluffers try it they get caught. When they pretend that they want to go and they don't really mean it.

A: Although my Chairman has told me I am discriminated against I have never really felt this, because there are all these other explanations.

M: In your retrospection about your career in psychology, you have had lots of teachers--and don't be influenced by my anecdote about Bott--of all of the people who have taught you in psychology, which one or ones had the most potent influence or long-lasting influence, either at the time or since?

A: I would find it very difficult to say one. I was very much influenced at the time, as an undergraduate, by Chant and Blatz. Bott's influence didn't start coming through until I was a graduate student. Line's influence really became more potent in the Army than it was at university. Those four. With Chantie it was a question of "it's fun." It was such fun playing around with ideas. Also he helped me a lot with writing although I had an awful lot to learn, still, afterwards. But it was important to learn to write and language wasn't just a tool to be taken for granted. You had to sweat at it and you had to use all sorts of tools:to get a thesauris and Fowler and things I never would have thought of. Blatz, in terms of theory--I can see the residuals--and when I think of the course of my career, I can see a common thread the whole way through the course that is Blatz, no matter how much I have re-translated his ideas and so on. That is where it started. Bott, and I don't think it was only Bott, but the impact of the whole

Department got me started on what I can only call a "non-behaviouristic" approach. And you came into this with your courses in history. This whole background approach which is infinitely richer than the kind of background that most American students have been getting the whole way through. To me it has always seemed so evident that development takes place through organism-environment interaction. It is a really interactional approach. This goes back to 1932 I am sure, and it was the truth and it has been the truth for me ever since. I have never really strayed from that. With Line I think it was, again, one of these terribly indefinable, visionary, things of value.

M: So it is an anti-mechanistic outlook. You are talking about Line's influence not just as a person, but it is difficult in his case to separate the person from the psychologist. I was going to say that you probably wouldn't say that you learned much psychology from him, would you?

A: No, I said, it was this personal undefinable thing of value. That as a psychologist you are a person striving toward ethical goals. But as a psychologist he was part of this whole anti-mechanistic rather broad background that I found extremely interesting.

M: What books, of them all?

A: One of the reasons that this comes to mind is because of a recent experience, but this isn't psychology at all, it is neurology. Incidentally, I am very grateful for that period when we were all supposed to study neurology because in the course of that I read Adrian's book on neurophysiology. This was, by all odds, the most exciting part of neurology. It made complete sense. This is a quantum theory of neurophysiology I thought and I guess I was right. I encountered him in 1968 at a meeting in London when the Centre for Advanced Study in the Developmental Science was being established and he was on the Board of Directors lending his support and wisdom. I encountered him again this summer and he invited me to be his guest at Cambridge and offered to show me around. This was very exciting.

M: Did you tell him about your encounter with him?

A: Yes I had told him back in 1968. I don't think I would have had the courage to accept his invitation if I had realized that he was Chancellor of Cambridge! We had luncheon in his rooms and a personally guided tour through the College, so it was very nice.

M: So, that was Adrian.

A: Köhler, The Mentality of Apes. That was another interesting thing because this ethological approach to infancy had led me into all this non-human primate stuff and all of a sudden I realized that I had said, back when I was an undergraduate, that I thought that Köhler's Mentality of Apes was, by all odds, the most delightful book in psychology. So I am still with apes.

M: Well you still have the direct observation method and essentially basically the same.

A: Part of my time this summer was spent in Paris, of all places to go for the purpose of monkey watching.

M: Oh, I thought you were joking. You really are a primate watcher?

A: One of our ex-graduate students is French and she is now one of the few French primatologists and has a job with a fellow by the name of Bourlière.

at the Faculty of Medicine in the Department of Physiology. I think that is really the focus of French primatology. She borrowed my research associate for the summer to apply Piaget-type observations to baby and adult monkeys. Silvia (Bell) did a fascinating, classic thesis on the relation of the development of the concept of the object to mother-infant interaction. She developed a scale to not only measure Piaget's permanancy of objects based absolutely on his original observations but also a parallel scale with a person. Instead of using a toy and hiding a toy under a screen she had a person hiding behing the screen as a parallel scale. Now which develops first, the concept of the person as a permanent object or the concept of the inanimate object? It had already been established that most but not all infants are advanced in the concept of person--about 70% of them--but she wanted to know about the other 30%. As her hypothesis turned out it does relate to mother-infant interaction. The more harmonious the mother-infant interaction throughout the first year the faster the child gets this idea that the person has permanence. And the ones that get the idea that the person is permanent don't lag behind the others in the development of the idea of the object as permanent. They are advanced in the person but they do not lag in the object.

M: But the others, the 30% who don't, do they lag on the object?

A: They lag behind behind on the persons particularly, but they also are slower to get the concept of the object in the final stage than the others. But not so retarded in the object as they are in the person. We think, "Isn't it nice to do experiments with monkeys, because there are so many things you can do with monkeys you can't do with people," but by golly, there are things you can do with people that you can't do with monkeys.

M: Like what?

A: For example, you cannot take an infant monkey away from his mother to test him because the infant is so distressed at being apart from her that he can't cope with the test.

M: You are talking about infants at what kind of age when there is this total and complete dependence?

A: It goes on into seven months.

M: That means that all the time the monkey is using the mother as a base of operations and so is in contact with her except when he explores a bit. Is that what you mean? Whereas the human infant is accustomed, at an earlier age, to being separated?

A: Yes. Actually, with a human infant you usually do your testing when the mother is present because babies don't like being alone into a strange place. But you can tell the mother that she is not to interfere, but you can't tell the monkey mother not to interfere. The monkey mothers are terribly fascinated with the task that you try to get the infants to do and they horn in.

M: That reminds me of the fellow who went to Africa and took the Porteous Maze Test. He wanted to get some measure of intelligence and he discovered in this particular tribe he couldn't do it because nobody ever did anything alone. You couldn't test children individually or adults individually. These things were not even tried unless they could all get together and work on it as a group.

Adrian Köhler?

A: Allport , remember?

M: Social?

A: Personality. The David Dunlap Memorial Essay that got written every year. One year Dave Ketchum set the topic on Allport's book on personality and I wrote an essay on this and won it. The essay was almost as long as the book! At the time I thought that his was an extraordinarily big stride forward but subsequently I found fault with Allport's position. At the time I was very excited about it. That was about the first (non psychoanalytic) psychological book on personality that had been published. Then later on I taught theories of personality but that gets out of the graduate student days into the teaching days.

M: Well I think you should go on to these books that have influenced you very much later.

A: I don't know why this should be hard.

M: Well it is hard because there are too many. I have one other question I would like to ask but I don't quite know how to put it. Canadian psychology to you is not just Toronto, although it is mostly Toronto and the Department and your experiences as an undergraduate and as a graduate. After that you were involved in organizations of psychologists and encountered a great many during the War and in all the various kinds of ways. From the perspective of Hopkins, from the perspective of a senior academic centre for psychology in the States, what have you got to say about psychology in Canada?

A: I feel sad. (i.e. to be away from it) Hebb's been an influence on me very indirectly. I can remember that CPA meeting when his book on the organization of behaviour had first come out. A large portion of the meeting was devoted to the discussion of that book. At the time this didn't have any great influence on me, but in retrospect, having gotten very interested in some of his ideas about early learning and the Piaget sensorimotor material which suggests how the development of intelligence is influenced by experience and we ask "What can the neurological foundations be?" In a sort of round about way I have come to realize the extraordinary impact he has had on psychology. And the wide variety of different problems that he has explored and done some classic studies--that Hull thing yesterday. It occurred to me at the time that you could almost say the same kinds of things about Hebb only I don't think you would have to write the obituary so soon. The nature of fear and all that.

M: He throws these out. Recently I heard Ryan on ESP and I was recalling one social occasion at which Don said the most unexpected thing for him to say: (this was after his fame and acclaim) that the one thing he would most like to do with the rest of his life is to design and carry out the crucial experiment on ESP. He has that kind of almost omnivorous interest and excitement. This stabilized image stuff he has been doing recently with Pritchard is all very original, very brilliant stuff.

A: I have a nice little anecdote about Hebb. We were on very friendly terms. He was a rather juniorish sort of person at the time I was a juniorish person. I had just got my Ph.D. and Bott was sort of pushing me out of the nest and I didn't want to go. I was hooked on security and wanted to stay on working with Blatz. Nevertheless Bott kept setting up these interviews and one he set up for me was with George Humphrey. I didn't want this job so I felt totally at ease. He wanted me to be in charge of the experimental laboratory, of all things. When he asked me what I would do about apparatus I said, "I don't know a thing about apparatus. I would have to have a good shop person to do it for me. I couldn't possibly cope with it myself." The he asked, "How are your statistics?" I said, "Oh, I have kept getting As but I didn't deserve them. Really, I am not very good in statistics. I have managed to master the theory of a correlation but that is as far as it goes." Something about all of this disarmed him, and Humphrey

decided I was the person they wanted. This also reveals discrimination. The Queen's Senate turned down because they wouldn't give a woman a university appointment . Humphrey was very upset about this and came down to Toronto personally to tell me. So he had to have somebody, so he took Don Hebb.

M: I wondered what the relationship was to him. That would be 1939?

A: Yes. Talk about the best of a bargain!

M: That is reminiscent of a thing I stumbled over somewhere, that in the same year that Titchener came to Cornell, Kirschman came to Toronto, only Kirschman found a ready made laboratory that had created and was very happy. But Titchener walked into a cold wall of hostility, no lab, no nothing, they didn't know anything about what psychology So toward the end of that first year at Cornell after he had been corresponding with Kirschman and the contrast was so marked that he applied to Toronto for a job and Toronto turned him down. And those two names are in the history of psychology now! We have done some peculiar things, haven't we?

A: One of the things that has struck me is the extraordinary thing that happened to Canadian psychology in the 1950s. You people expanded two or three years before the big American expansion.

M: Do you mean student enrolment or staff?

A: Staff but it also was in student enrolment as well. The way that Canadian departments of psychology have built up in this period seem to be nothing short of miraculous. I was just talking to Tony Doob about this to-day. He was talking about Lynn Newbigging, as a matter of fact, and Mary Wright and you. He thinks that you are a good chairman. I agree with him and I think that you deserve an enormous amount of credit for this kind of blossoming in Canadian psychology.

M: I think that those of us who were chairmen at that time were much influenced in what we subsequently did by our participation in such things as the Boulder thing in 1948 and even our own opinion where Mary was a junior, she wasn't even a delegate, but she was tremendously influenced, and her aspirations of what she wanted to do at Western. It was a miserable mess when she took it over. I think much of her subsequent practice and policy and staff recruiting was molded by the kinds of goals set, defined and formulated that night. I think we did do a good job.

A: Also it was a lot of awfully hard work. You weren't doing this as a sideline. The thing that drew it to my attention to-day was when Tony said the McMaster had invited him to come but he naturally had gone to Toronto, and he just didn't want to move,but he mentioned somebody's name and a telephone call went from Newbigging to that man the same day, with investigation in the meantime.

M: Lynn at McMaster has created a damn good small department and defined its area. Hebb always argued that you can't be good at everything so, for God's sake, be good at something. So Lynn created, out of nothing, in a very short time, a helluva good department. Mary had a big general kind of department and she pulled that out of a terrible state that Gord Turner had got it into. An aspect of the current situation is that, in doing this, we have Americanized our Departments of Psychology by determining to go after the best people we could find and who have turned out to be American. This is somewhat dismaying because, as a result of this, there has arisen in faculties particularly at Carlton and it has spread to other universities, very strong grounds for a protest against the Americanization of Social Science Departments. Where they have really got some grounds for being excited about it is in Political Science, and History, and even perhaps in Sociology. They feel that the American brand of history, for example, is not entirely appropriate to the Canadian scene. The history as taught is Americanized history, and it is influenced by that culture. This is spreading. There are all sorts of proposals that just horrify me, that there should be a quota on the proportion of your staff that can be American.

A: You can't nationalize talents.

M: On the other hand, what I am concerned about is this public backlash--this kind of propaganda that has been stirred up. It has a very unhappy effect on some of our young Americans who feel, "If I am not wanted here, I can go somewhere else."

A: When I was a student it never occurred to my parents to send me anywhere else but the University of Toronto. It never occurred to me to go anywhere else for graduate work. In retrospect I think it would have been a very good thing to have done.

M: I think so too. But it didn't occur to us. Partly this was because of the Depression.

A: Partly. But if you were going to go to a Canadian university Toronto and McGill had the only Ph.D. programmes. It is always assumed in the United States that everybody is going to go away to college. They certainly don't stay on staff where they have their graduate work.

M: They won't be allowed to, in most cases.