Interview with Brenda Milner, Ph.D., Sc.D.
American Academy of Neurology
Oral History Project

December 2, 2011

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Montreal Neurological Institute and Hospital
Montreal, Quebec, Canada
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Heidi Roth, MD and
Barbara W. Sommer, Interviewers

Brenda Milner: BM
Heidi Roth: HR
Barbara W. Sommer: BWS

BWS: I'd like to start the interview for the American Academy of Neurology. It is December 2, 2011. We are interviewing Dr. Brenda Milner, Dorothy J. Killam Professor of Psychology at the Montreal Neurological Institute and Department of Neurology and Neurosurgery at McGill University. The interviewers are Dr. Heidi Roth, Associate Professor, Department of Neurology, University of North Carolina School of Medicine, and Barbara W. Sommer. We are in the conference room on the sixth floor of the Montreal Neurological Institute.

We could start the discussion, you were talking a little bit about your – before we get into the discussion about the work that you do – would you talk to us a little about your background?

HR: It's so hard to know where to start – isn't it?

BM: I was going to ask you where to start – because, because there is so much that could be said. When you have lived to be ninety-three – there is a lot of background. [laughing] – [Tell us] a little bit about your education and how you feel your education, sort of, propelled you in the direction you took –.

HR: – maybe when you started – when you were growing up, maybe starting a little bit with your family.

BM: Yes, I think I should start with my family. Well, I'm an only child, and my father was a musical critic on the *Manchester Guardian,* now the *Guardian,* a newspaper in England. I grew up in Manchester. He was also a grower of pleasure, of delphiniums and plants and so on. And he was also a professional pianist though his main work was a musical critic. And my mother – who came from a broken family in Birmingham – she went to my father – she was living in Manchester – she went to my father for music lessons, and that's how she met him. And this is a point because he was twenty-three years older than my mother, and he died when I was eight. But during those first years, he was a tremendously important figure in my life because he would be going in the evening to an opera or a concert – Manchester was a big musical center then – and so he was at home in the mornings and I would have a *lot* of time with him. Neither of my parents were in science atall, but they assumed I would be musical, and I'm not atall, I'm absolutely not musical. And there was a long time of persuading them, but because I was so much in love with language, my father finally accepted that I was not musical. He found me practicing on the dumb piano – they had a dumb piano to loosen up your fingers before a concert[1] – for a pianist – and I didn't mind working there, I didn't mind reading the notes or doing anything like that – as long as I didn't have to *hear* music. And so –, but I loved language from the very beginning. And I read *so* much Shakespeare with my father and I used to go around, as a little child – I believe – spouting Shakespeare to the slight embarrassment of my mother everywhere. And so I got a little drunk with words very early, and I think I also got my love of foreign languages – and I really feel so strongly about that – because this was – it was, you know, I was born in 1918, and so near the end of the war [World War I] – but – Germany was really the place in music and my father was going to Europe and writing on early modern, all this kind of thing, and so German was a language that I heard a lot of. And the first foreign language I learned a little bit of – when I was very young – was German. And so, it always seemed to me – and I don't speak German now – it always seemed to me very normal – and I

[1] A dumb piano is a soundless piano used for practice and finger exercise as well as limbering up fingers before a concert.

feel very – this is where I agree with Dr. [Wilder] Penfield, the founder of this Institute [Montreal Neurological Institute - MNI] – that it is tremendously important to have more than one language early on. It's a passion with me. So this was what I was like when I was very young.

And so my education – my father didn't approve very much of the normal kinds of schooling around – and so, as long as my father was alive I was at home working with him – I remember this – and I remember he would be upstairs, you know, resting and I would be doing some Shakespeare or something with him – and then there'd be a knock at the door and I would go down and answer the door – my mother would be out shopping or something. There would be the inspector for the schools, you know. I was six-and-a-half; why was I not in school? I would go upstairs – "He wants to know why I'm not in school." And my father said, "Go down and ask him if he speaks German." "My Daddy wants to know if you speak German." "No, of course not." And then I'd go back up. "Well, then, tell him he should go back to school." [laughs] So this was how I remember those years with my – growing up.

And then my – and *oh* – and something you should know before *all that* because I actually think this is interesting – is that, you know I was born in 1918 and I had the Spanish flu – the famous Spanish flu – when I was six months old; and my mother, who was thirty-four, also had it; and we *both* survived and my mother lived to be ninety-five. She had a much harder life than I had in *many* ways, but she lived to an – So, you know, we had that big stroke against us, and we must have had – I always say I owe – why I am sort of healthy at ninety-three – I say well I owe it to my genes, to my mother – but, you know, both of us must have been pretty resistant. My father didn't catch it, and that's the sort of age distribution they had for H1N1, right? My father was sort of older and he didn't catch it, but we both caught it. And, but the consequence of that, which was a little unfortunate I think for me, was that I was supposed to – I don't know whether I had or not because medicine was rather primitive – I was supposed to have a slightly funny, little weak heart thing, that I would grow out of because of the flu. And so I wasn't allowed to play games – sports – until I was eleven, I think. And I've always been very bad – I'm tremendously interested in sports – I'm absolutely passionate – I was passionate about cricket and soccer in England – and last year having the soccer to watch was wonderful. And I *make do*,

I make do, with hockey – I'm not very interested in tennis. But my school that I went to later was very, you know – we had wonderful playing fields and we had hockey and lacrosse and netball in the winter and cricket and tennis in the summer and so on, but I wasn't allowed to participate in any of this until I was eleven. And I wonder if some of the reason that I was so *bad* at it was because I started it so late. And I would have loved to have been, loved to have been, good at these sports, but I was not. I was the sort of person when they were picking teams, you know, I was left at the very end. "You have to go into one team or the other."

So anyway that was my early upbringing. And then when my father died – this was a big change, of course. We had no money. The *Guardian* did not – it was very prestigious paper – but it was not especially generous. My mother had a tiny little pension that disappeared totally in three years. And so, you know, it was a problem. But they managed to get me into – probably at a reduced rate, I don't know (laughing) – into Withington Girls' School, which has become a very, very prestigious girls' school in England now. But not a –, but a day school you know, my mother didn't want me going away. And so I went there, and this is where people now talk about high school being from twelve or something here, but *there* it was high school from the age of nine right through. So I was at Withington from the age of nine to the age of eighteen. My whole education was there, in that school, which is a very good school.

Then I suppose the next thing that you probably need to know about is because – now I need to think about work. I became, well, I was passionate about Latin and very good at Latin, but my school didn't teach Greek, and to go on in classics you know I would – and also my favorite Latin teacher left to get married. This used to happen, by the way – that, in those days, if you got married you couldn't be a schoolteacher. And all the – and it was always the smartest, prettiest, liveliest teachers that got married. And I remember this – it was a French teacher that was very good. She wasn't actually my personal favorite – but she was extremely popular and a very good teacher – she left and got married. And the next French teacher, was – you know. And this Latin teacher, whom I *loved*, also left and got married. I kept in touch with her, she lived to be in her nineties. But she's eh – you know – so this was, not classics, for these two reasons.

And then I was just passionate about mathematics and I wanted to do maths. And I wanted – I was also passionate about Cambridge University. This is partly because I am on the – on the – Pur – on the [Oliver] Cromwell side of the Civil War, you know. The history – oh, there is an historian here. I am very passionate about – you know – this was the important century in British history – was the seventeenth century – and I was a passionate Cromwellian because I – in spite of the "Praise God" – and you know – but "keep your powder dry" is the point, right? I didn't, of course, agree with his attitude to the arts and that sort of thing, but I still think about Oxford as "the home of lost causes" – that's what we called it.[2] And you know, Cambridge had all the great mathematicians. It also had the poets, you know. And so I *knew* I wanted to go to Cambridge and there had also been a couple of girls from my school, who had gone to – senior to me – who had gone to Cambridge, and so I knew that this was where I wanted to go. And so my school was desperate because I was very good at languages and they said, "It would be so easy –" and I had to get a scholarship, right – I couldn't have gone anywhere without a scholarship. They said, "Look, it would be so easy to get a scholarship and go to Oxford and do languages." I said, "I don't want to go to Oxford. I want to go to Cambridge. And I don't want to do languages, except as a hobby." I believed – and I still believe – that you can keep your languages up, but once you give up maths and sciences, aren't you really – and particularly science – it's teamwork and you give it up. Anyway for all these reasons, I was very stubborn.

My mother was very good, you know – I fought with my mother a lot – but she was very good you know, because she would have loved me to go to Oxford in languages and she could have shared this pleasure with me and so on, whereas she couldn't – but she supported my wishes and went to the headmistress and supported my wishes. And so I managed to get a scholarship to Cambridge – thanks to my physics, really, more than my math. But I discovered at the end of a year there that this was – you know – unfortunately we had a math exam at the end of the year – that really there should be a change of field. But there is a little back – a little backtrack. Am I allowed to backtrack?

[2] Oliver Cromwell [1599-1658] was a Puritan and a military, religious, and political leader during the English Civil War [1642-1649]. He led his forces to a Parliamentarian victory during the war and, followed by his son, ruled England until restoration of the monarchy under Charles II. The admonition to Puritan soldiers, "Put your trust in God, my boys, but mind to keep your powder dry," is attributed to him. The description of Oxford is a Matthew Arnold quotation: "home of lost causes, and forsaken beliefs, and unpopular names, and impossible loyalties."

HR: Yes, you are. Absolutely.

BM: I want to backtrack here because, again, it's relevant because it follows what we were saying earlier, and it is relevant to what came later in my life. It's that – when I went – after my father died, when I got into this school, I went when I went I was nine, actually I was barely nine when I went into this school – and I was extraordinarily bored the first year because of the education I had had with my father. I was really, really bored and restless. And so my mother went to the school in the summer holiday and said, "Would it be possible for Brenda to skip a year, the next year?" And they said to her, "Well, yes, you know, it will, but I must warn you, Mrs. Langford (that was my maiden name) – I must warn you that Brenda would be behind in French." And my mother said, "No, she won't."

["Attention, Attention….Code Blue" Code Blue statement on hospital intercom; discussion about Code Blue]

So that summer – and I just loved grammar, you know, like my later love of Latin, you know – that summer, when I was nine, nine into ten, my mother really taught me enough French grammar for the next five years, I think. She was, obviously I get my language abilities from my mother. My father wrote, my father spoke German so well because he'd studied music in Germany, but not for any other reason, I don't think. But I got my languages from my mother. And so this became very important later, because I learned all this French and I got this really good start in this language. And so that is important.

HR: Also, at home, again?

BM: Also at home. Yes, also at home, and so – that's right. I always feel a little taken aback when people use this expression; they say, "So you were home schooled?" I say, "No, no." I just say that – you know – because my school was so important to me – [laughing] – but it was – .

HR: It sounds like, in addition to your schooling, your .parents were very influential.

BM: That's right. Yes. Yes. And I was an only child and, you know, and only children *do* do well. There are psychology studies that show that. And the oldest person in a family does well. They may feel they've got burdens and so on, but they do very well. And so it's – .

HR: So then at Cambridge, after that first year?

BM: Well then, that was very funny, looking back – it wasn't funny at the time. I thought – I'm still, you know the things one gets romantic about – I'm not a romantic person, but I was very romantic about the idea of logic. You know, I thought that I was *really* a logical person. I wanted to do something with logic. And so I thought, well, maybe, maybe I should do philosophy – the moral sciences track – maybe I should do philosophy if I couldn't do mathematics. The older girls in my group who were students in my college were sent to talk to me, and they said, "Brenda, don't you have to earn your living?" I said, "Well, of course, I have to earn my living." "Nobody ever earned their living in philosophy." [laughs] And so then they said – and this was – you know there have been lots and lots of luck, as you will hear, in my life. and I don't believe – and I tell students this – I don't believe anybody gets anywhere without a bit of luck, or quite a lot. The thing is – you have to recognize it, I think, and take it, because it comes just like that and you might miss is. But I think you need it, you know.

BWS: That was a time when the luck intervened?

BM: The luck intervened here really because I said philosophy and they said no, but what about – they thought about moral sciences – there was a group in Cambridge and I suppose they still do this. They cluster, you specialize, it is extremely specialized, you – both Oxford and Cambridge are specialized to a degree you don't over here. But you still have groups sort of considered a cluster – and sort of – and so philosophy and logic and ethics and psychology, in those days, were in a cluster called moral sciences. And so I have a degree, actually, in moral sciences. [laughing] Psychology was Section C, Moral Sciences, or something. These people were just trying to help me. "Oh, she wants to do philosophy but it's not practical. Oh, psychology. What about psychology?" And so – I didn't know anything about psychology –

and so I said, "Well – you know –." Well, they said, "You could be a factory inspector. You are employable, you know, if you do psychology. If you do philosophy, what's going to happen to you?" So this was in 1936 or 1937. So the head of the psychology department – it's so easy to confuse him with Dr. Penfield – had a Dr. Penfield-like figure – the head of the psychology department was Frederic Bartlett, later Sir Frederic Bartlett, who wrote a book on memory, *Remembering*,[3] which I must say is very different from Penfield's views about memory and closer to my own. But the thing is that he was again one of these – well, authoritarian – not – I don't relate closely to – it's not fair to call them – um – grandfatherly figures – especially in my relation to my father who was almost a grandfather – but these builders of empires or builders of something seem to be almost two generations away from me – although they may not be that many years away from me – and so Bartlett was a tremendously important figure in British psychology. And he was a very important figure during the war because he kept all his psychologists out of uniform – [by] which I mean he kept us as civil servants, which was really wonderful. You could look an Air Marshall in the face and tell him something and he had to listen to you. But if you'd had a rank, he didn't have to listen. He was very, very good. He was the chair of psychology at Cambridge. And his wife was psychology director of studies in my college, Newnham College. And so – she gave, when she knew that I was going to try psychology, she gave me – it's bigger than a telephone book (laughing) – she gave me a handbook of experimental psychology. It was a handbook. It was like this (indicates size) – and said, "Well, read that during the summer." I was so obedient about this, like I did my French grammar during the summer. And this summer I read, not the whole of this, but I read a lot of it. [laughing] I came back and then I encountered, for the first time, the pleasure of being in a lab – you know, after, I mean I did chemistry in high school, but since I was just, it was just very, very delightful, that period. And – but you should ask me some questions, though, ask me some questions because now you know how I got to Cambridge and into psychology.

HR: You know, one of the things that I think might be interesting is – there were so many people in psychology there and some of them, you know, may have influenced you more than others over time. You mentioned Oliver Zangwill.[4]

[3] Frederic Bartlett, *Remembering*, Cambridge: Cambridge University Press, 1932.
[4] Oliver Zangwill was a neuropsychologist and professor of experimental psychology at Cambridge.

BM: Well, Oliver Zangwill is the person who influenced me from the very beginning because I went to – that was another thing – I was quite rebellious, I realize – I went to this – he was lecturing, he was young, he probably would have been more than like a post-doctoral fellow only they didn't do Ph.D.s, you know, in England. That was a sort of Germanic thing that came over to the States and now it went back. But, it was different – nobody, the head of the department didn't have a Ph.D., you know – they had a different system in England. But he would be about just few years older than me, about twenty-six, I think. And he was lecturing on perception, mainly visual perception. He was actually quite a shy man. He was a tall, thin man, and when he was lecturing, he would be standing sort of parallel with the blackboard looking over there, rather than looking at the class. But I got so excited. I knew about his material and I was really, really interested in it and I was excited. I liked him and his lectures.

I decided I wanted him as my supervisor, because you see, supervision in Cambridge is what we call tutorial at Oxford. It's this wonderful – it is a wonderful teaching system in which you have – you know, in Cambridge it was just one person. I would spend an hour a week and I would write an essay. You had to write an essay once a week, on some question you were assigned, and he had to read it. And then in Oxford, I think, you have to read it aloud, but you didn't have to do that in Cambridge. You'd get it in the night before and he read it and discussed it and criticized it and so on. And this is what they call a tutorial system. It's wonderful. I think it's something as the classes get bigger and so on becomes pretty difficult. I have a friend teaching at Oxford and she's really complaining about this. But usually – this is not obligatory – but usually because we have a college system, of course – usually it's the faculty member – let's say the psychologist, or the mathematician, or whatever – who is attached to your college, a member of your college – who will be doing the supervisions of tutorials of the students that live in that college. And so I was – it was assumed that – Miss Vernon, who was a fine psychologist, nothing wrong with her, would be my supervisor. But I went, you see, I went right away – to Newnham – I must have gone in my college – and said, "I want to have Oliver Zangwill," you know. And I managed to get him, you know!

When I think about myself, these are aspects of myself that I take for granted – that, of course, you fight for what you want. But as I *tell you*, I begin to realize more and more, how much I have fought. And that my mother, when I was at school, was fighting [for me]. I probably inherited this characteristic from her too. Although, she was quite, sort of, timid about many things, but when it's important you fight, you know?

So this was Oliver Zangwill that was really, I mean he was – and the advice, because then I had – because part my learning Maths was over in one year – fortunately I got through, but with no glory obviously, but I didn't fail or anything – then we come to two years up to the final exam. And there is an exam in between, the University exam, called Mays. Because you sit it in June, they call it Mays – that's typical British. So Mays is an exam that doesn't count towards your degree, so when you get your degree in the end, physically, it doesn't say anything about this exam. But you always try to do very well in things, and I'm an ambitious person. And Oliver Zangwill told me that I should *not* try [to get a first] – and you are given the whole – it is not the course system atall – you are given the whole field of psychology that you are responsible for. And you go and prowl around the University library and so on. I mean, you know, um – [to HR] you are from Harvard. I once said something to [Edwin G.] Boring – wrote something, I never met him – he was asking me to do something, and I let out, unguardedly that, something about, why were students reading these textbooks, why were they not going to the library and so on? And he wrote back and said, "For God's sake, don't send a student near the library, they will tear the pages, they'll steal the books. For God's sake, keep students out of the library – [laughs] – even if it means writing textbooks!", you know. And I was quite shocked because we were told to go to the library, you know.

HR: Well that's so interesting, though, because it gives you some opportunity to explore – by yourself.

BM: That's right. That's the whole point. That's the whole point of the Oxford and Cambridge education.

HR: So many things are structured. You had this opportunity. But Oliver Zangwell didn't think that was – what did he say?

BM: No he thought it was great. Oh, no, of course, no, he, no – that's the British system. So, no, but what he said to me before the May exam was, "*Don't try to cover the whole field for this exam*. Just accept the fact that there are certain things – because you don't have time – just accept the fact that what you are going to study, you are going to study thoroughly, and you won't do well on the other part, and so you will *not* get a first." You know, the important point – thing was to get a first. "You won't get a first in Mays, which don't count, and you can work for a first in your finals, which do count." But you know, the normal thing is to work for a first all the time. And but he says – if I did that, then I would go sterile and it wouldn't work in the end. And I [laughing] obeyed him. And so I got a 2.1. I hated getting a 2.1 but I got a 2.1 – a top second.

And then, and then I *really* went all out. And it was, again, it was very stressful because it's a small group – and we were twelve people taking psychology finals at Cambridge – and you always think of this little distribution curve, and I *had* to get a first to get anything. I had to get a scholarship; I had to get something to continue. And the thought of being a factory inspector – I'd never learned to drive a car – and I would have to learn to drive a car, and I didn't want to be a factory inspector. So you can't be a schoolteacher – you know there is no job in teaching, unless you are doing psychology [at a higher level], you know, so you have some other kind of job. So – so there I was – and I was, I was – and we were this group of twelve and we used to meet – ourselves for little seminars, you know in the evening ourselves – we did this. And it was perfectly obvious that there were – you know, who the – who the good – more or less, you know, what people, how people were ranking themselves. And there were three or four of us that were – that were, yeah, there were two – two or three – men – there were two or three women in this class but they were not really very competitive, I would say. But there were – there were – a couple of men that were, you know, very good and I was pretty good. And then there was this man, Trevelyan; that was his name, and he was so brilliant – he was absolutely brilliant. And I've seen this kind of thing repeat itself, incidentally. This man was so brilliant, there was just no question – he was very original, he was very exciting. So I wondered what would happen. I

thought, "Trevelyan is going to be the only person getting a first because he is so much better than the rest of us." And he was. This was true. And, then, you know, I would get a top second again, you know, and so I worked so hard; I've never worked so hard – well I guess I worked equally hard in mathematics and physics to get to Cambridge, but anyway, I worked very, very hard, you know, for this exam. I really worked very, very hard.

And so I got a starred first. I was, it came out – I was written up in the papers and all this sort of thing: A Manchester girl wins something or other at Cambridge. [laughs] And the other people that were good – the three – they got firsts. And Trevelyan, Trevelyan didn't write the exam. Trevelyan was found later in London doing some experiment with rats in the bathtub, where he was living or something.

HR: It goes to show there are other ingredients.

BM: There are other ingredients. You know, it's amazing; these people who are really so, so way out. And we were not wrong in thinking this guy was bright – and brighter than we were – it was true, but –.

HR: You mentioned your tenacity.

BM: Yes, I guess I have that.

HR: And maybe that was something that helped you through all those – that studying.

BM: Tenacity of purpose. Yes, I do have a lot of – I do. And I think you are wanting to watch – again, what one advises students, you know that, if they, well, when – because it is easy to get a romantic notion. I think it's one of your questions later. It's easy to get a romantic notion about what, you know, science is like – neuroscience or whatever field – and you know there's such a lot of time – that you are spending – it depends on the technology you are using – but it doesn't matter whether you are a bench scientist or a behavioral scientist. There is so much time where you are actually taking repeated measures; you don't get to, you know, you don't make a

discovery every year. You know you make little discoveries along the way. But you have to have tenacity of purpose. And you have to be – you mustn't get bored easily, and I don't get bored very easily. And I think that's an important – it's a negative kind of quality, just to say, well, you shouldn't get bored, but I think it's terribly important not to– you know – some people get bored very quickly. And they, I don't think, should be doing any scientific field because there is *a lot* that's, that's not romantic at all. It's very repetitive and very frustrating. And so, you have to be able to, you know, to take that as part of life, you know.

HR: Maybe we should move on a little – go ahead.

BWS: Can you tell us a little bit about coming to Montreal?

HR: One thing I wanted to ask though, just real –, I know it's so hard to be brief, but I was interested in, you know – Henry Head being there –.

BM: Oh, yes. Back to Cambridge.

HR: The legacy of Jackson. You know, Hughlings Jackson was influenced by philosophers.

BM: Yes, yes, yes, I think it's more [Sir] Henry Head I should be talking, saying rather than – When I came into psychology – there was –..

HR: What were the dominant paradigms? Or –?

BM: Well, well – I, I – the sort of – you see, this is actually interesting in the history of psychology – because I was – this was '37 to '39. I graduated from Cambridge in 1939. So it was those two years – '37-'39. And then I got a scholarship, I *did* get my scholarship and stayed on till '41, but we were on war things then. '37-'39 – the influence, I think the influence of Head on Bartlett had been very strong because Head was talking about posture and movement and Bartlett, Bartlett always talked about the organism. I got so used to that. Not the subject or something or other, but the organism, the organism – and he was talking about human subjects –

but he always says the organism. The organism brings certain things into the situation. These were Bartlettian terms, very vague, organism and situation. The organism brings something. And that's the important thing in my psychology. Later I was to see Head attacking American behaviorism. but this was not the psychology I was brought up on. We used to laugh about it, you know. Henry Hull and all those theories of elaborate stimulus response things. We used to laugh about this.

HR: [B.F.] Skinner?

BM: Well, Skinner was later. Head was anti-Skinner, but Skinner was later. At this time [it] was Hull. Clark Hull, not Henry Hull. As far as Bartlett, Bartlett always felt that psychology was a biological subject, I think. This was, you see, maybe you have to know a little about psychology in England because it wasn't very much developed, but there were two, two important – two very important movements in psychology in England. One was this experimental, biologically-oriented one which came from Cambridge, and then it was professors from Cambridge that populated the developing Oxford department. That was sort of a Cambridge-Oxford thing. And then there was an equally important – I would say equally important – some people would say more important – stream which was the mathematical intelligence testing kind of psychology, which is very respe – very important, which had its origin in Scotland really – Edinburgh – and then London. So there were sort of – Edinburgh/London was very much mathematical psychology which had enormous practical implications too.

HR: That's like IQ testing?

BM: Yes, yes, yes. I mean we had to learn about it, because we had to learn about everything, but it was not what Cambridge and Oxford were, sort of, oriented towards. It was Edinburgh and London. Really the rest of England wasn't doing much in psychology atall. So I was growing up in this biological tradition and it was really from Head. And it was so, that always one was talking about what– there was never, you know, never stimulus/response/reflexive kind of thing. It was always – it wasn't Pavlovian or anything like that – it was always that you were – what

does the organism – the subject as we would call it – bring into the situation, into the psychology of the situation, and there was activity. We always knew it was in the brain. I mean, obviously, we're not talking about measuring anything in the brain, but we were always thinking the brain was relevant, you know, in British psychology – in Cambridge. So something is going on, there is activity. And this is Bartlett's favorite expression – "set." You bring a "set," you bring a sort of expectation. It's not quite like a focused attention, look at the light. It's more an orientation you bring in. And so, then whatever happens, whatever you are shown – and they had lots of simple little experiments where you were shown – in a tachistoscope you were shown briefly – ambiguous figures that could be seen as scenery or it could be seen as something else. And depending upon what your expectation was, you would see it one way or the other. And this is very classical experimental psychology. But this was the emphasis on what the individual brings to the situation. Anything that comes in – the questions from outside – are impinging upon ongoing activity. And he would agree it is activity in the brain but it wasn't helpful for us to be thinking about the brain at that point.

HR: Did you think, does Gestalt psychology have anything to do with that?

BM: Not really, well, Gestalt psychology – I mean, I'm very well versed in that because I had to read it – well [Charles Thomas William, known as Adam] it was Kohle and – well Kohle, Kohle had – I only got to know Kohle years later in the United States. I met him – a couple of times – a very fine man – he was a character – But – and I met [Kurt] Koffka – but the –.they had some – I'm not going to expound here on their physiological views because they were very –

HR: You don't need to go into that. I was just curious if that intersected.

BM: Well it was sort of a – well, yes, I mean Koffka's big book, which I read – still own I think – came out in 1935.[5] So you see, it was, it was contemporaneous with Gestalt psychology. I think that, you see, Gestalt psychology was very close to the philosophical phenomenonology, right? And so on and so on. It was the – and there was a great deal of emphasis on the organization of the – out in the world, of the stimulus, you know in that sense. Not a little

[5] Kurt Koffka, *Principles of Gestalt Psychology*. New York: Harcourt, Brace and Company, 1935.

stimulus, but you know. And it was very vague about what the individual – I would say that we would be a bit suspicious of it. I was quite attracted by it. I remember one of the essays I wrote for Oliver Zangwill, and it was something about perception. And he put something about, "I don't want to ask you what the Gestalt psychologists think of perception; I ask what you think of it, you know." [laughs]

HR: I see – there is one other topic I want you to touch upon because I think [Alexander] Luria did some work. Did you -?

BM: Luria is a *good* friend of mine.

HR: Oh it would be interesting – did you have any knowledge of him at that time – or was it later?

BM: No. no. I knew nothing of Luria at that time.

HR: We'll save him for later then.

BM: No, well, it doesn't matter, it really doesn't matter, since you mentioned Luria. But probably it is better that he comes later. But we shouldn't drop Luria because I really, really like him.

HR: OK. Let's speak about your time after you came to Montreal.

BM: You want to know why I came – don't you?

BWS: Yes, tell us about that transition, then.

BM: Yes, because it was, you know, there I was in Cambridge. I'll try to make this short, but it *was* the war – a little thing like the war [World War II]. [laughs] I was in Cambridge for the first two years of the war. The work I was doing on perception became adopted for a task for the Air

Force. That was the end of this. It was really an exciting time – the Battle of Britain and so on –[6] but I was in Cambridge for all that. But then my scholarship ran out in '41 and I was recruited by C. P. Snow, the writer, you know, who came to Cambridge.[7] He came to Kings College and he interviewed people and he recruited me for a radar research establishment and – which began then somewhere in the South Coast. And I was not very happy there for six months but then we moved over one weekend – because they were afraid the Germans were going to come and take scientists – not little scientists, beginning scientists like me, but big scientists – they were going to take them over to Germany and make them, you know, work for the war effort over there. So, we moved, dramatically, one weekend, to Malvern in the heart of the fruit growing country near Worcester in the heart of England – and on the radar research establishment.

And I lived there, you know, for two-and-a-half years. I had this – a wonderful time there. But, anyway, this was the war. And I was – I was really doing applied psychology there. And this is where I met Peter Milner, whom I married. Because it was important to train radar operators to be able to, you know – they had to get the target – the planes coming over and so on – the missiles and what was the best way of following them. It could be direct or it could be aided laying or it could be velocity control. And so this is strict applied psychology. And so they had me there to test this out on normal subjects that might become – the sort of people that would become – radar operators. And this research establishment was otherwise all physicists and mathematicians.

And I have often thought, I mean – not often – I thought two days ago, I thought, my goodness, everybody asks me, was it strange to come here as a woman, you know – to the Neuro (MNI). You don't seem to realize I was the only woman scientific officer in that huge establishment and everybody was maths and physics. You know, Peter was a physicist and electrical engineer, Peter was the person who had designed the test that I was using. He designed the equipment for mocking up – the mock-up – of an air raid and the following – and so I was constantly having to call him if the scoring machine broke down or something else. That's how I got to know Peter.

[6] Bombardment of English cities by the German Luftwaffe and resulting aerial combat over England during World War II.
[7] Charles Percy Snow, later Baron Snow, known for his work as a chemist and novelist.

But it is because of Peter that I came to Canada because it was near the end of the war. We knew it was near the end of the war in Europe – 1944, the end of '44. And one day we were supposed to be going to the theater in Worcester that day. Peter arrived looking a bit troubled. And he had been called to the director's office to be told that Sir John Cockcroft, the famous physicist who was in Cambridge, was going to take a group of scientists to Canada to set up the beginnings of research on atomic energy in Canada. And so Peter was "asked", which in the wartime – "asked," you know, was "told" – that he should be doing this for one year. And so, you know, we decided on the spur of the moment to get married. We had not – well – you know, well – I had never intended to get married actually, but that's neither here nor there. And so we got married and went back to work and told them and then we sailed. We didn't know where we were going to be in Canada. And we didn't know how we were going – it was all very secret, you see, because of the wartime. All our books went through censorship in Birmingham and we took the train up to Glasgow. And it was dark and wet, you know - blackout. Then we went on the Atlantic – across.

We landed in Boston and that was a very amazing experience for me because we had been in a blackout in England during the war. It was terribly important, you know, not to show a chink of light. And then on the Atlantic because of submarines and everything, we were in a real blackout. I mean really, really blackout for five days across the Atlantic. We got to Boston and the thing I realized, I was light hungry. It seemed so lively, so wonderful. And they took us out to, you know, steak and ice cream. So, that didn't mean anything compared with the light. When I commented on this, they said, "But this is the Boston blackout!" [laughs] And it made you realize all the relativity of everything, you know.

So then we were taking this train up to Canada, and it was Montreal. Now this, you see, is just one *huge* piece of luck. Here is the girl who never dreamed – I never had any money to go to France, never – before the war. We were as poor as church mice. And then during the war, nobody was going off to France and – but I had promised – this is a little promise I made to myself to punish my high school teacher, or something, when she was so cross with me choosing mathematics – I made a promise to myself that during my – while I was studying mathematics, and later psychology, I would never read a book for pleasure except in French. And I kept the

promise to myself and I read lots and lots of French literature just to prove to her, to myself, that I can keep up my French. But, of course, I wasn't using it. And then I find myself in Montreal and the first job I get is at the Université de Montréal – is teaching in French. It was a real challenge because I wasn't used to speaking French anyway. The vocabulary was totally different. I was teaching animal behavior, and you know, this is not the vocabulary you get from Flaubert. [laughs] And I wasn't even used to teaching. I was certainly not used to speaking French. And the accent and the vocabulary, to some extent, was not one I was familiar with. And the accent was very different. But I had a wonderful time and the students would say at the end of something, they would say, "Madame Milner, do you want to learn a new word?" or something. [laughs] It was, well, I am tremendously grateful to the University of Montreal. I worked there for seven years, and during the last two of which I was doing – you know – this work here really and they were supporting me. So I really – I don't ever want any sort of biography of me that leaves out the Université de Montréal because I'm *really, really* grateful to them.

HR: And who do you think really helped you at that time? Was there somebody –? You came to them to teach? Did they need somebody to teach?

BM: Well you see, it was interesting – in – it was really interesting – in Montreal, in this place, in McGill. You see, I think all the English – McGill – really had nobody at that time. People had gone off their various ways during the war. There were a couple of dottery old people who sat in the faculty club drinking, I think, but, you know, everyone at the University does that sort of thing. But it was really practically nonexistent. And when I tried to get jobs – that was the other hilarious thing. You see, in England this – it is not true here – in England, as I've given you the impression I'm sure, what class, how you perform in your B.A. – it was a B.A. at Cambridge – or B.S. – your final exam, your undergraduate exam – determines your whole career. To this day, the British psychological – the Society of Experimental Psychology in England, you know – you get rotations of candidates this year and they always put – to this day – they always put Class 2.1 or Class 1 or Class whatever. It marks you for life – what you do in your undergraduate degree. Whereas a Master's Degree in Cambridge was something that you paid – in those days –ten pounds for – after two years. And it was to prove that you were respectable. I mean, supposing I

got my brilliant-style first and then I robbed a bank or did something – or did something disgraceful, I wouldn't get an M.A. because I wouldn't be recognized as a senior member of the University. What the M.A. does is mean you have the right to vote if you are in the University things, that you respectable. The M.A. means you're respectable. The B.A. tells all about you academically. And so – totally different here. So I came over with glowing letters of recommendations from Kenneth [J. W.] Craik, Bartlett, my tutor at Cambridge, all these things. And nobody paid any – and nobody offered me any job because I didn't have a Master's Degree. And of course, I didn't even understand this. And it was only years later than I processed all this – and at the time, it seemed to me very, very peculiar.

And in the meantime, the Université de Montréal had sprung up as a new – and again, this was incredibly exotic for me because it was run by Dominican priests. Now I was never – never – I'm *not* religious. My parents were what they called in those days – this was the standard term in the late 19th century – they called themselves "free thinkers." And it's by different routes my father and my mother were free thinkers before they met. So they were free thinkers, so obviously I'm a free-thinking child. So I had never been to any religious ceremonies of any kind, you know, in my life, you know, until probably – until I was sixty years old. I never – I wouldn't go to religious ceremonies; I would go to the parties afterwards or something. So I was not –? that was nothing. So that – but leaving that aside, don't forget that this is – that England is – still culturally a Protestant country. I know that the person who helped me – I did a little bit of Greek at Cambridge to please – and the person who was kind enough to teach me was Justine Toynbee, the sister of Arnold Toynbee, and she was Classics Director of Studies at Cambridge. And she was a devout Catholic and every morning – the Catholic church – we used to call it the Catholic church – we didn't call it St. whatever-it-was because there was only one Catholic church – and it was out near the Station – and the bicycles were just outside my window – and I would see Miss Toynbee very early in the morning, at seven o'clock, getting a bicycle to go off to the Catholic church. And I would be tremendously impressed by this because she was a very impressive lady. She was really, really nice and very good to me. But anyway, that was my first little encounter – that you know, this, this strange world of Catholicism, was Miss Toynbee.

But then, here I am in Montreal and it's like the Middle Ages, you know. They were all in white robes. Later on, people, they started going around in black and so on. But the Dominicans – it was very picturesque – these white, white robes. One of them had a pet ferret, or something, and he would – but not in the psychology. But anyway, the head of psychology, Father Noël Mailloux was really, really an amazing man because he was – [Sigmund] Freud was very fashionable – Freud is not fashionable any more over here – but Freud was very fashionable then – and he was teaching Freud in the day and St. Thomas' Beatitudes in the evening to his graduate students. And he was – actually, this is true – they got a little interested in this in Rome and he had to go to Rome and explain. [laughs] But he was allowed to go on. They said it was OK. But he had to go there and explain, you know, how he was reconciling Freud and St. Thomas. He found they went together very well, I think. [laughs] But, anyway, he had no experimental psychology and so he said, you know – he offered me – just to teach – would I give twenty lectures, give twenty lectures –? Somebody once unkindly said to me, "How could you fill twenty lectures on Bartlett's theory of memory?" That was the first job I had – was teaching Bartlett in French. And then, well, would I run an experimental psychology lab? Would I run an animal behavior lab? I had my monkey and my rats. We got the rats from McGill, I remember. They gave me the rats. [laughing]

HR: So he was really a big supporter early on, [then] for you?

BM: Father Mailloux, yes, yes. That's ironic, no? Yes.

HR: And also because his interests were so different. In no way was he a mentor?

BM: Oh. No, no. Facilitator.

HR: And that's important – the institution supported you.

BM: Yes. Well he obviously was a man of vision. He realized – he had all the famous – I mean I can't have their names – but he had Bonaparte – whatever. He had all the famous Freudians come and visit and give seminars and things. And I went to the Psychoanalytic Society

with him, you know. It met somewhere down here near McGill. And that's where I met Dr. [Miguel] Prados. But anyway, I would go to the Psychoanalytic Society with Father Mailloux. [laughs] Teaching experimental psychology in the day and I was – in the meantime, my French was becoming – so you can understand I am grateful.

HR: So when did the transition – talk about the transition then.

BM: Well, the transition, you see – there were two big steps at McGill – and the first step is often overlooked but it was critical – was that McGill realized it had to –

[Aside: I was not having anything to do with McGill then. This was the funny thing, you see, Peter began – in the one wing of the University of Montreal – most people don't remember this – it was devoted to setting up the beginning of this atomic energy. And *then* they started at Chalk River, which you've probably heard of. It is where Canadian atomic energy is now – in Ontario at Chalk River. But it was – for the first year or two – it was in the University of Montreal and then Peter was here. And then Peter moved to – they moved to Chalk River. And I would go there in the summer, but I would stay here – and then – I'll come back to that]

– what was happening at McGill was that the psychology department, you know, knew it had to pull itself together, and they got somebody who was very akin to gestalt psychology but not a gestaltist – Dr. Robert [Alastair?] MacLeod who had studied in Germany with the gestalt psychologists and so on. He was an American, and I think, or maybe he was a Canadian. He came and he loved history. I mean, he really, really loved history of psychology and everything. A very scholarly man. And he had some seminars at McGill and I was invited – I don't know how this came about – I don't remember. I started attending those seminars at McGill in the evenings then. He was given the job of building up the psychology department, and he recruited two very important people to the department of psychology. One was a very brilliant statistician, George Ferguson from Scotland, who came, and he retired – well, he has passed away now – but he retired a few years back. But he was a very, very fine statistician. And he recruited Donald Hebb.

Now Donald Hebb was from Nova Scotia, and his parents were doctors and his sister was quite a well-known physiologist, actually, in Cambridge. But he had been a student of [Karl] Lashley's you know. Well, I don't need to go into the whole Hebb story. But Hebb came with his – what was to be this very famous and influential book – *The Organization of Behavior* – in manuscript.[8] And it was the days when we had the mimeographs that came off on your clothes – you know, the blue – and he had this seminar – fortunately it was in the evening. It was a tradition that seminars were in the evenings – luckily for me because I was teaching in the day. And so I came down – I brought one student with me from the U of M – I was allowed to do that – and we attended this Hebb seminar. Mort [Mortimer] Mishkin was at that seminar and we've been friends since that day. Mort Mishkin was a McGill student – as you may not know. His Ph.D. was at –

HR: Is he at MIT [Massachusetts Institute of Technology] now?

BM: No, no. He is at NIH [National Institutes of Health]. He just got the President's medal this last year.[9] He is a very, very close friend. And we were both in that first Hebb seminar. And so this was just such exciting stuff. And so, Peter was in Chalk River and I wrote these letters to Peter about Hebb, and Peter gave up his job at Chalk River, he came to Montreal, asked Hebb if he could do a Ph.D. with him. I'd already decided I must do the Ph.D. with Hebb. But that – but Peter, it was a little that Peter came. Hebb made him go through physiology and do all sorts of things because he had not had anything like that. I had never had physiology but nobody ever made me do it. But they assumed I had. But Peter hadn't. And so he went on and became a psychologist and head of the department and did a lot of work on motivation. But the thing is – that uh –.

HR: That's your husband, you're saying?

BM: Yes, well, we divorced in 1970, but we are very, very good friends. His wife died some years ago. And we are very, very – he is my *best* friend.

[8] Donald Olding Hebb, *The Organization of Behavior*. New York: John Wiley and Sons, Inc., 1949.
[9] National Medal of Science.

HR: And he must have been very influenced by you, obviously.

BM: Well, obviously, yes, yes, obviously.

HR: He left his career – changed his career.

BM: Yes, well, the change was by Hebb, I would say. I mean, I was writing about Hebb. But I do think you're right. I think Peter was – well – I'm putting it a different way. I think Peter had a natural interest in psychology and this is probably why he, you know, got to know me through this work, you know. But the way we became friends, I'm sure, I mean, he liked me, of course, but I'm sure he –.

HR: You had an intellectual bond.

BM: That's right. That's right. And he always was very, very interested. And so you know, and I think also that he felt – was another thing that I can't quite explore – it's not relevant – but I think that he – his background was really in engineering – and I think he felt – and I mean he was very good or he wouldn't have been sent out in that little team – but I think he felt that the exciting things at Chalk River were for the pure physicists – the really creative – you know – and that – well, sure, while he was extremely good at what he did – but I don't think it was inspiring him. Yes, that's right. And so this Hebb stuff really, really did.

And so the reason how I came here [to the neuro – MNI] – I was still teaching at the U of M, and Hebb had spent one year, in 1937, or a year-and-a-half, here at the Neuro with Dr. Penfield. And he published with him – mainly looking at patients with frontal lobe tumors, a few frontal lobe injuries, who were being operated on for epilepsy. And he published with Penfield. And that had just been a period, and then he had gone back to, you know, to Lashley to get his Ph.D. and so on and so forth. But one of the promises he got when he accepted the job at McGill was that he could send one graduate student to study Dr. Penfield's patients because he really believed it was – you know he was definitely interested in brain and behavior – he'd been working with rats and

some monkeys and so on – but he really believed that you could learn about from patients, you know, and that this was very valuable. And so when Penfield agreed that he could send one graduate student – . Now, in the meantime, I had approached Hebb and said that, you know I would like to – convinced him – that I wanted to do a Ph.D. And I started on some work on congenital form perception in the – in the congenitally blind. I started working on tactile form perception in the congenitally blind – and I had gotten contacts with the blind here, and I had even begun to get a few experiments going with them – unfortunately – never materialized – when Hebb asked me if I would like to come up here and see Penfield's patients. And so, you know, as soon as I got here, I realized that I absolutely loved it. But I was still –but Hebb assumed that when I got my Ph.D., I would – you know that I'd acquired that – that I would still have this – I had a tenured position at the U of M – and that I would be teaching.- Hebb wasn't very fluent in French – that I would be teaching his ideas. Also I'm sure that was a little bit in his mind – at the U of M.

And when I got my degree and I said I wanted to stay on, he said, "You know, You are a fool. No psychologist can survive for long at the Montreal Neurological Institute. You are a fool." And he was a very straight-forward man. And I said, "Well, I'd like to try." Then he said, well he would support me for one year on his grants. He had research grants from Ottawa – based down in the Donner Building, the old Donner Building doesn't exist now – where physiological psychology was – but just for one year. But he said, "I'm warning you. You are a fool. It won't work." And I said, "Well, I'd like to try" and I gave up my job at the U of M. Really, I did do some quite daring things – because I needed the money. Before the end of the year, we had had memory impairment patients here at the Neuro. And I can remember Dr. Penfield, in his very autocratic sort of style saying, "Well, you have to come up. They're here. We need you." And that the great Dr. Penfield would ever say, you know, "We need you." [laughing] And so he gave me a little office. No funds. I never had any funds for equipment or anything like that. But he gave me a little office on the ground floor here. And that was the beginning.

HR: But he supported you with salary at that time?

BM: A very, very, tiny, tiny salary. And then, the person who was really, really helpful to psychology was Dr. Penfield's successor, Dr. [Theodore] Rasmussen, who really – I mean, he was just wonderful. I don't think, I don't think I would have stayed under the – you know, you were supposed to dance around. And not just the psychologists. It could be very – .

HR: It wasn't integrated?

BM: Well, it was. It was.

HR: Hierarchical?

BM: It was very hierarchical. It was not sexist or anything. People always say – and you are going to ask me at some point – and you don't need to go into it because I have never felt discriminated against as a woman. I really haven't in my dealings, haven't. It's the first question I get asked anywhere I go and I say, "No." You know, I haven't.

HR: You had seen that teachers had to leave school [in high school when they got married]?

BM: Oh yes, but that wasn't me.

HR: No, it wasn't you. But it must have made you feel that you didn't want to be like that.

BM: I didn't want to be a school teacher, I don't think.

HR: Or, you know, have to be sacrificing.

BM: I didn't intend to get married, right? I never wanted children. I mean, that was very, very clear. I was a person who would go out on a first date with somebody and manage to introduce this fact into the conversation. [laughs] So people just knew where I stood. And I was going back to Cambridge at the end of the war.

HR: So at this time, then, we were talking about the hierarchical structure.

BM: The institute. It was hierarchical.

HR: Not sexist, but hierarchical.

BM: That's right, it was a pyramid. But, you know, I'm sure, that this would – I imagine that any professor – it was the difference – the psychology department was not like that, the faculty of science was not like that. But I think it was the difference between an ordinary university department and something that's tied to a profession. I'm sure that in the law school it must have even been more so. And I'm sure that in my own country of England, I'm sure that at Queen Square – the Institute of Neurology at Queen Square – it would be more. This place modeled itself on Queen Square and I'm sure it would be more hierarchical. But it definitely was, you know. And so there were things – also when I was a student, you see, I was teaching at the U of M and I had to – Dr. Penfield would be deciding who to operate on – we'd have a conference about three patients and when he was going to be operating on this one. And I was testing them before those [operations] trying to look at the effects of the surgery. So, I mean, the idea that they had something pre-op was later. And so I would be testing the patients before surgery and then after surgery. But it was important, you know, that I get to see them. And I would test them at the weekends, of course, and then before the – but it was important to know – and the chief resident was very pro-psychology and would sort of give me the best guess he could of who Dr. Penfield was going to operate on and I would test that patient. And then Dr. Penfield would call in and say, "I have just decided I want to do Mrs. Jones instead of Mr. Smith." – for perfectly good medical reasons – he had very good medical reasons probably. Nothing – I am not criticizing that. I'm just saying that that was sort of very difficult. But the thing that was more difficult was that in those days they did – there was no MR, no way of looking – they did a pneumoencephalogram and after that, a lot of the patients would be lying flat, you know, and then when they would come down to give them some block design or something like that, they would have to lie flat. And after that, "Oh, oh, oh, Mrs. Milner, I am so sorry." You would take them back. This was just awful.

The whole indication with Dr. Rasmussen – I will *never* forget this. Never. Dr. Rasmussen – he had trained with Dr. Penfield before my time. I didn't know him. And he'd gone to Chicago – he was head of neurosurgery in Chicago and then he came here and took over. Dr. Penfield was still here in that office there and Dr. Rasmussen was here. And Dr. Rasmussen looked after all the money and those things and Dr. Penfield was writing and so on. So, I suddenly get a phone call – I've only met him once, I think. "This is Dr. Rasmussen. I'm on 3 East. Um, I would like to do a pneumogram on Mrs. Smith and so I'm wondering if you could let me know when you are through with the psychology test so that I can plan the pneumogram." And I thought, "My God, the world has changed. It has really, really changed." [laughs] And from then on, we had such enormously collaboration with Dr. Rasmussen about everything. We were very interested in sensory things in those days. No, I mean, as far as I am concerned, I wouldn't have stayed under the other conditions because it was – but this was wonderful for psychology. I really thought the world of him and we all did. I think Sue [Suzanne] Corkin was my student then. We all loved Dr. Rasmussen. But he was the most modest, you know, man. Quiet. Very quiet. He would get these – patients – did very well. He used to say, "Don't jiggle the brain." He was *very* concerned about the blood supply. He was very special for me, was Dr. Rasmussen.

HR: You know, maybe you could speak a little bit about how you were –, your scientific creativity took off at that time. What you saw.

BM: See, I never think of myself as creative.

HR: How you were, how you were gathering data – what the process was. Where you were getting your hypotheses from.

BM: Animal work entirely. And I'm *really* – monkey work – yes. That's why I'm a behavioral neuroscientist. [laughs]

HR: And it was very, uh – at that time, monkey work was *big*, too. It was big – .

BM: Yes, yes. You know that when I wrote my thesis – well, I can remember writing, as I say, I wrote, this historical review to my thesis. I remember this because I took this – I worked hard and I thought it was pretty good, and I took it to Hebb, and then when I went to see him and he said, "Can't read it." And I was so insulted. You know, the thesis was due in August and this was – and I put it aside –I was really insulted.

HR: What did he say? He said – ?

BM: "Can't read it. I can't read it. I can't follow it." And so I was very insulted. And I put the thesis aside for a while. And then I thought, "I'll show him." But then I started to read what I had written and I could see *exactly* what he meant, because – you know how it is when you write – *you* know what's coming next, so to speak. And I wasn't giving the reader the information that the reader needed to follow the next paragraph, so to speak. You know, it's very elementary, this – it's one of the first things you have to teach students – that, that, I was, sort of, projecting my knowledge into the reader's mind, but I hadn't given it to him yet, you know. And so, that's what he meant by "Can't read it." – you see. So I figured this out. And so when I re-wrote it again, he said to me, "This is very good, and it would make a *Bulletin* article." And it did in 1954 –*Psychological Bulletin* – is the historical introduction to my thesis.[10]

HR: We should read that. That's what we should have read. That will tell us what we need to know about the background.

BM: The background with the monkey work. Well there, the whole question – we were all interested in the cortex, there, and how information was getting from area 17 to the inferotemporal cortex – which Mort [Mishkin] was showing was so critical – in spite of patients having very big lesions in pre-striate cortex. You know, this was the question – big lesions in prestriate cortex but you could still get this information. And so I was polling Mort – and Mort was working – Mort went and did his master's here and then went to work with Karl Pribram and was, that was where he did his work on the inferotemporal cortex. And I was calling him and

[10] Milner, Brenda. Intellectual function of the temporal lobes. *Psychological Bulletin*. 51, 42-62, 1954.

asking him about degeneration in the inferior pulvinar and things like that. This was the sort of thing I was interested in. It was the cerebral cortex.

HR: That's interesting, it's very interesting, and I think there is some misperception, I think. When people look at your work with HM, at least that first paper. OK, "here is a neuropsychologist working on this patient," because you are doing this neuropsychological testing. Whereas were you were really coming from the behavioral neurosciences.

BM: Oh, definitely, definitely.

HR: And I think that's important.

BM: Yes.

HR: And it is interesting with all these differences in terms of how people perceive the different categories of training in the sciences.

BM: It is very tricky these days. I do feel very strongly that I'm a behavioral neuroscientist. And so does Michael Petrides, my successor. But, of course, he does work with monkeys – he works with patients too. And he is very good at anatomy so he is more obviously – but I think behavioral neuroscience. And that is what Larry Squire calls himself. I think behavioral neuroscience is what I should be. Nobody has ever told me what I am Dorothy J. Killam Professor of – so I sort of change it from time to time.

HR: Well it's interesting, maybe we could speak about this briefly. This is sort of the juncture where you start establish yourself in the field. So where you were coming from helps us understand, you know, what kind of a scientist you are.

BM: I think you are absolutely right.

HR: And you know, we have all these labels that have been fluid. At the time, you know, I have always been impressed with Luria's work. He's considered one of the neuropsychologists. But, you know, he was interested in a lot of different questions about cognition and measurement.

BM: Yes, and he was very interested in frontal lobes.

HR: Frontal lobes, for sure. And also very patient-oriented. So he came from the patient – to figure out how he could test these people. So he's not just the neuropsychological battery in large numbers of patients.

BM: Oh, no, no. But there was never anything like that in what I was doing.

HR: No, neither, neither. But sometimes people perceive neuropsychologists – .

BM: Well, Yes, that's why, that's why, I don't do it anymore. It's being debased.

HR: There are neuropsychologists. I know some, for example, have studied with Sue Corkin who have come and have done work in neuropsychology and research, you know –.

BM: You know, she was my student, right?

HR: Right, right, right.

BM: – she met HM in my lab.[11]

HR: Could you comment about – maybe we talked a little bit earlier about *behavioral*, and that word, being used in behavioral neuroscientist. And how, now people have wanted to talk about it as *cognitive* neuroscientist as opposed to behavioral neuroscientist; and why that means something different to you?

[11] HM was the subject of pioneering memory studies led by BM.

BM: Well, [laugh] your question is on record, right.

H.M.: Well, we can, we don't have to keep it on –.

BM: No, no, I mean, I don't have to reframe it.

HR: No, no. You can if –.

BM: Americans – and here I make the difference between Europe and – there is a huge difference – in the history of my field – between Europe – I include England with Europe, as part of it – and North America. And there was a tradition on the Continent among French and German and Austrian – scientists, neurologists – not psychologists – to be interested in – to be *reading* psychology, and interested in *methods* used – experimental methods. But people – Henri Hecaen in France, who had a very big influence. And in England, it was not the medical people, and you know, it was Oliver Zangwill. And I was his student, you know, before the war. Elizabeth Warrington was his student just after the war. And it was Oliver Zangwill who really believed that – well, he believed you can learn– it's this whole thing – and Hebb too – that you could learn from the – our *aim* is unlike the medical – our aim is to understand the *normal*. But we believe that by studying the effects of lesions, by studying abnormal, we'll shed light on the normal. The goal is to understand the normal. Like people ask me about, you know – "Is this helpful for Alzheimer?" – or anything. I say, "I'm delighted if anything I do is helpful, of course. But that is not what drives me. What drives me is curiosity and is really – I want to understand the normal brain, and I get light shed on it by what I learn from patients and I'm very grateful to the patients for that." But I got this personally from Oliver Zangwill, who was, during the war, he was going out to a hospital that was about thirty miles outside Cambridge to talk to alcoholics with – amnesic alcoholics – [Sergei] Korsakoff [also spelled Korsakov] patients. He became very interested in memory. But then he moved up to Scotland, to Darts. There was a neurological group that was studying the effects of brain injuries and he was very – and he published some papers on parietal lobe that way. And the same thing with, you know – so I think

that here the interest was definitely in what you could learn from a disordered or a damaged nervous system about the functioning of the – and we were *always* thinking about the brain.

So that when I came to Canada, it was not, this whole American business about looking at behavior and behavior isn't being a dirty word – which well, behavior*ism* may be, but studying behavior is not. That's what I do, you know. And that's why behavioral studies – behavior of animals. You know, there are neuroscientists that work at the bench and they don't study the whole animal. But if you work with the monkey or the rat or the person – it's you know – its –its – you are studying behavior.

HR: And making that distinction between cognitive – ?

BM: Well, cognitive – you see, for *me*, you were mentioning [indecipherable] earlier, cognitive, conative, and affective – those were these words, you know – thinking, striving, and feeling. And I don't work on striving or motivation. Peter actually made this – pleasure centers of the brain – and did all the early work on that with rats. But not me. And I have, I have not worked on feeling either, on affect – except incidentally. I mean if patients have some – you work on what your patient presents to you. Right? The patient tells you what you are supposed to be working on, I think. But I would say I'd always have a bias – I wasn't especially interested in *memory*, I mean, partly because we've got Bartlett and a little bit of this negativism in me towards big figures like that. I liked him but I didn't, you know, I didn't particularly want to work on memory; whereas I'd been so fascinated by Oliver Zangwill and perception, and so on. And perception was what I thought I was interested in. That's why I was interested in what I was finding with the cortex. Then I fell in love with the right hemisphere. And because I have a ? battle on ? – You know, I mean, I'm good verbally, but I think sometimes think my brain is just full of words because I'm really not well-coordinated and so on, and also, I'm also not good spatially – I'm really not good spatially. And so I became very, very fascinated by the right hemisphere, which was very under-privileged, you see. I became good friends with [Roger Wolcott] Sperry and I studied Sperry's patients – but this was way before Sperry or split-brain – that I was having a real fight to prove that patients with right temporal lesions had some mild – had some visual-memory deficits and visual-perception deficits. And I remember going out to a

meeting in New York, the Eastern division of the APA [American Psychological Association] – and we always went to that – and there was no Society for Neuroscience or anything like that in those days—and had *all* the – [Hans L.] Teuber in those days was at NYU [New York University] – and there was all this little gang of people who were objecting to my claim about getting deficits in the right temporal lobe and that – "Maybe the surgeon is just making a bigger removal." "Maybe it's a visual field." "Maybe it's this or maybe it's that." [laughing] You know – anything *but*. And [laughing] you know, I stood my ground, because I was very – you know – always been confident in my data. Nobody's ever proved my data wrong. It's just how you interpret the data, I think.

HR: Yes, but wasn't there precedent in the right – you know, I know that [John] Hughlings Jackson – you know, had publications on patients with –.

BM: Oh there is a lovely, there's a lovely article by Hughlings Jackson – where he mentions one patient, that – yes, yes. [HR: And there must have been more.] It was a hypothesis, though. No, he didn't have – no he didn't have too much. But I don't think he had – and I mean and anyway these were not [HR: not established] not temporal lobe.

HR: They were not temporal lobe. This idea they – was this spatial – dominance in the right hemisphere.

BM: Yes. Oh yes. Yes. Among neurologists, you see, this was the bias. The bias in Europe and the bias that came over actually with Norm [Norman] Geschwind and everybody. The bias to think that what is important is language. You find that the study of aphasia is far, far advanced compared with the study of any of these non-verbal – I'm *sure* this is true.

HR: There is a wonderful book by Anne Harrington – on the two sides of the brain – the historian.[12] – do you know about that? Looking at the right hemisphere. Biases, and perception. I'll send you the reference. But should probably get back to – move on to, some of your

[12] Anne Harrington. *Medicine, Mind, and the Double Brain: A Study in Nineteenth-Century Thought.* Princeton, NJ: Princeton University Press, 1989.

accomplishments. But I do think it's important to set the foundation because that frames how you went about your work.

Well, I guess, that maybe – well, there was this period when you got your Ph.D. until you published the publication with Scoville in '57, was it?

BM: It was in '57.[13]

HR: Where you were doing various things. You were interested in the right hemisphere.

BM: Yes.

HR: And you were doing the temporal lobe.

BM: Yes.

HR: And you also had seen some patients with Penfield who had told you about their memory problems. And you had started to see a few memory problems in people with left temporal lobe lesions.

BM: That's right. Because I wasn't on memory for my thesis. Then I became – that was in parallel with – this was interesting because there was a neurosurgeon in England – I know him but his name is not coming to mind – in London and he was one of the – because it was pioneer work, this temporal lobe surgery. But in London they were doing temporal lobectomies. They published – actually it was first published – the verbal memory deficit from the left temporal lobe lesions. I had these findings at the same time, but I was in my thesis – but in England, too, they found the same thing. It was always emphasized that it was verbal, not because we'd been thinking of it like, you know, the memory things. We were talking the right hemisphere – talking about spatial phenomenon and visual pathways. But it was very clear – and I make this point

[13] W.B. Scoville and B. Milner. Loss of recent memory after bilateral hippocampal lesions. *J. Neurol. Neurosurg. Psychiat.*, 20, 11-21, 1957.

always to the medical students – that it's not modality specific, it's material specific – we use these terms – material specific deficits versus modality specific. It was material specific. It didn't matter whether they were reading or whether they were listening. It was – if the material was verbal, that they had no difficulties with memory for faces or places or you know – and then I was really working to show the corresponding thing from the right side which was much harder to get people to accept.

HR: Well, tell us a little about then the work with Scoville and HM.

BM: Well you know, in all *fairness* to Dr. Penfield, you *have* understand that we did this *here* before I ever saw HM. I mean it is really – this PB. Dr. Penfield never forgave me – [interruption]

[Discuss taking a break, end track 1007, begin track 1008]

HR: It made a difference. I think it is important what you were saying that you did this groundwork here before you saw HM. That's how Scoville *knew* about it.

BM: Yes, that's because of the paper I gave at the ANA [American Neurological Association].

HR: And that hadn't been published? It had just been repo –?

BM: Oh, it's a published abstract. Published, because it was at American Neurological in Chicago in 1954. The ANA, and it's a publication, you know, it's a book.[14]

HR: Something was presented at the Cushing Society, somebody said –?

BM: The American Neurological –

[14] B. Milner and W. Penfield. The effect of hippocampal lesions on recent memory. *Transactions of the American Neurological Association*, 80, 42-48, 1955.

HR: The American Neurological Society – Association. Right.

BM: In Chicago.

BWS: It was in the *Transactions* in 1955. You published in the *Transactions* in 1955.

HR: And that's how Scoville, then, was familiar?

BM: Because Scoville read the abstract – the abstract – and called Penfield and said, "I think I've seen this." And he came and we invited him to discuss the paper. He saw the abstracts before the talk. And he came to the meeting in Chicago and discussed the paper at the meeting. And then at the meeting, and afterwards, according to Penfield, he asked if I would come and study any of his patients I was interested in studying. So, but it was because of the work here. So really, I feel in fairness to Dr. Penfield that – .

BWS: Talk a little bit about your work with PB and how that happened.[15]

BM: Well, you see, we were – essentially what I was describing – what was being described from London too – was a sort of minor problems – compared with having your epilepsy controlled. It's not very good to have a – and also these patients had a little trouble before the surgery. The IQ may go up and you have a little more trouble with your verbal memory. And you know, it's still worth it. But then we had this – when Penfield started here, before my time, he started this work in 1934, when he first started the temporal lobe surgery, he was very careful not to invade the medial structures – not because he thought they had anything to do with memory, he didn't know what they did – but such big, important-looking structures, you don't take them out if you don't have to. And Penfield was always, Penfield was always rather optimistically – I mean, I think Rasmussen found you could rarely do this – but he was very optimistically trying to find a focus, you know, a small area that he could remove, as small as possible. But then some of the patients were coming back, and still having seizures. And by that time, which was when I was there, he had realized that if you were going to cure temporal lobe epilepsy, in most cases –

[15] PB was one of the first memory-loss patients BM worked with.

it is not true of all cases – most cases – you are going to have to go for the medial structures – the amygdala and part of the hippocampus. And so he was completing the procedure in people who came for a second operation.

And so, PB was a civil engineer. See, I like to make this contrast too, because HM never had a life really. But PB was a real functioning person in society. He was head of his own office somewhere in New Jersey. He had a wife and grown-up children and so on. He was interested in the theater and had a set of friends – a *real life* – in spite of the epilepsy which was just a little absence attacks or that sort of thing. Not absence, but, you know, dreamy states, and so on. So he came back and I tested him thoroughly. He was a very bright man. I tested him before the surgery. And then, after, he said to people, "What have you people done to my memory?" And he was a bit sarcastic about this, understandably. And he couldn't remember, you know, whether his wife had been to see him that day, what he'd had for breakfast. He knew Dr. Penfield because Dr. Penfield had operated on him before, but he didn't know me. I had been working with him – I'd worked with him for three hours the day before his surgery. He didn't know who I was *atall*. So he had some retrograde amnesia. But the anterograde was the impressive thing. It was in PB that we did write the paper – we have the paper about PB. It is in the *Archives.*[16] It was there that I could show *so* convincingly that while the IQ was unchanged, because I tested him before and after, and he had this very good digit span and there was nothing wrong with immediate memory and primary memory – I won't use the expression "short-term memory" – it confuses people, but there was nothing wrong with that – the short-term memory atall. But as soon as his attention was diverted, he couldn't recover what went before. And this was the whole pattern. So we – Dr.[Herbert] Jasper, who was another very important influence, right – Herbert Jasper was brilliant – it was because of Jasper that Hebb used to send his students here, not for Penfield. So Jasper and Penfield and I – we scratched our heads and said why did this happen? This is a devastating thing to happen to a surgeon when it is elective surgery. It is quite different if it is life-saving. You can say. "Well, you've lost your memory, but you're alive." But this is elective, to *improve* the quality of your life. And so, Jasper said, "Well, you don't know" because we couldn't see in his brain – except the part that was exposed. Maybe there is something wrong in

[16] Wilder Penfield and Brenda Milner. Memory deficit produced by bilateral lesions in the hippocampal zone. *A.M.A. Arch. Neurol. Psychiat.*, 79, 475-497, 1958.

the brain stem. Who knows with this particular patient? And then a week or so later, we had another patient – FC. A young glove-cutter, twenty. Fortunately he was able to go on cutting gloves. He came and had a one-stage left anterior temporal lobectomy including the hippocampus and had the same effect. And so then Penfield was, you know, really, really very upset. And we said we have to write these cases up to warn people and all the rest of it. And so we started writing this up, and we presented these findings in Chicago. And it was me – I gave the talk, and it was Milner and Penfield, and I *wrote* this, but we – Penfield – we would send notes back and forth about these patients. He would write these long-hand, back and forth. And apparently those copies are still around, because Scoville's grandson looked at the originals of this correspondence last week when he was here. I don't know where those things are.

But anyway, I was getting increasingly upset because I don't have the same views about memory as Dr. Penfield had. Dr. Penfield believed on the basis of his stimulation studies that there was a continuous record – like a tape recorder – of the stream-of-consciousness – and that, uh – and I had been brought up by Bartlett to think of memory as reconstructive rather than reproductive. It is a totally different sort of view. And I used to argue with Penfield. He would say, "We're not talking about memory as you psychologists understand it. We're talking about something you don't have access to normally, and now my electrode does, you see." But anyway, as we were trying to write the discussion to this paper, I was getting more and more uncomfortable – and I, especially with the – and not with the description – we both *agreed* – I mean, there was no problem about the *facts* – we never disagreed about the facts. And I turned to Hebb, and he gave me bad advice. I said, "I don't know what to do," and he said, "Oh, It's very simple. Just make him the senior author and nobody will blame you." That was *terribly* bad advice. Terribly bad advice, but I took it. So that's why it was Milner and Penfield – not Penfield and Milner – when the paper was published, not the abstract. Scoville was a little bit different because I wrote the paper, except he [wrote] the neurosurgical part. When I was writing with Dr. Rasmussen, Rasmussen was the most, sort of, self-effacing person you could possibly imagine, but mainly the idea is what you do is – I had never felt awkward publishing with neurosurgeons whoever came first or – not because I thought it is obvious the neurosurgeon has done the surgery and I've done the rest. But it is a very simple-minded approach because, of course, people don't do it that way, they count and so on. But anyway, this is what I thought. It was a small field in those days.

But Scoville was just – you know, he said, "Well, you know, this means more to me than it will to you." We didn't realize that this was being done – general neurology and neurosurgery – I have to write a little thing because apparently it is the *second*-most cited paper, they told me, of all the papers that they have published in the Journal of Neurology and Neurosurgery [*Journal of Neurology, Neurosurgery &Psychiatry]* – the green journal. I knew it had a very high citation, but I didn't know it was the second most cited. They are going to have little comments on there now – the ten most-cited papers. So they are asking me to write them. Scoville's attitude was – "I'm a busy surgeon and you are going to be writing papers for the rest of your life, you know, and so it means more to me than to you, kind of thing." And I liked Scoville. So it ended up Scoville and Milner.[17] Dr. Jasper was critical about that. He looked at me and said, "Oh, This is a very good paper. It should be Milner and Scoville." [laughs] But, you know, but – that's why – those two papers – I'm second author on. But I still think – it was trickier with Penfield. I mean with Penfield there were some ideas – a little bit of formulation in that that I'm not a hundred percent comfortable with. Not – not with the rest of it, but just a little bit. A nuance. So, that's the history of those two papers.

But, of course, this is – PB was a wonderful patient, and he – but then after this, after FC, we then said, "Well, Why has this happened?" And that was when we speculated that what we were seeing – this is what we said in our Academy [sic, ANA] thing – we were speculating that we were seeing the effects of a bilateral damage to the hippocampus. Now you might say, "Well, why hippocampus?" And this is because in *PB's* case, we'd only seen the memory impairment after the second operation. There was, no, there was a little bit of verbal memory difficulty, but nothing else, with the neocortical removal. But when we extended it we – we [laughing] – to the medial surface, we got this memory impairment.

HR: So it was the additional piece.

BM: That's right. So, of course, there is a flaw in that logic because you could say maybe it is the two together, but in fact, it drew our attention – .

[17] W.B. Scoville and B. Milner. Loss of recent memory after bilateral hippocampal lesions. J. Neurol. Neurosurg. Psychiat., 20, 11-21, 1957.

HR: At least it was necessary.

BM: Necessary, yes. And so we speculated, we always said the hippocampal *region*. We knew about the entorhinal cortex – all this sort of thing. We just meant that we were focusing on that [the hippocampal region]. That was our hypothesis. And now Dr. Scoville had developed these surgeries, as you probably know, for seriously ill schizophrenic patients, and then offered it to HM. When you know, when his position was so – he didn't have like temporal lobe epilepsy, neither in the pattern nor in the EEG nor anything. But it is amazing. It just shows how much these structures can sort of stir things up, if there is something to stir, because it had a very beneficial effect on his epilepsy, there is no question about that. Now, sorry – where was I going with this?

HR: Well, we were just talking about what discoveries you made *before* that led to your involvement later with HM.

BM: Well, it was finding with these patients.

HR: Right, very well described with PB. Where you had already developed those hypotheses. And then, and then, you wrote this paper which neither of you *predicted* would be – so influential.

BM: The one with Scoville. No. We thought it was important, of course.

HR: The aftermath of that, though, is interesting, too. The fact that – ?

BM: It took so *long* to get it accepted [accepted by scientists for its ideas - after it was published]. Well, I mean this was interesting. This is because we go back to your question about what was thought about memory. There was this huge influence of Lashley – his work in the rat which can be very easily explained in other terms as [Walter] Hunter did, you know. Lashley took out different parts and quantities of rat's cortex and tested the effect on their learning mazes

– and found it didn't matter where he took this brain from – this cortex – it was a question of the mass removed. I think he made some little exception for the occipital cortex, actually. And this could be countered by, as Hunter did, by saying that there are so many sensory areas in a rat and that, you know, you are depriving the rat, you are depriving the rat gradually of so many sensory systems, that these really help the rat in exploring the mazes. And so, you know, there are ways of handling this. But anyway, on the basis of these studies, Lashley, who wrote extremely well – he was a very eloquent man – he really wrote beautifully and spoke beautifully – I heard him once and he visited us in McGill once – that he wrote about equipotentiality and the action as a mass. And I read that when I was an undergraduate at Cambridge, you know, I mean, this – was 1928 or something – this was published. I mean, in my final exams at Cambridge, going back to that [laughing] – to that exam I did so well in, there was one day which was just an essay, and you had a choice of four or five topics, and I wrote on cortical localization of function in 1939. What was I writing about? I must have written about sensory motor systems, I think – I don't know what else there was to write about. But I wrote about it anyway. So, well, maybe I was attacking it – I don't know – Lashleyan-like – *maybe*, maybe both, you know, I don't know. I don't know what I wrote in that essay, but it did serve me well. But anyway, so, but I mean – I had that interest long before I came to Canada, from Cambridge.

HR: But the dominant view, really, about memory – ?

BM: – about memory – was that it was a function of the whole of the cerebral cortex, definitely – but the whole, you know, and no part. And this is why people were not working – memory was very *unfashionable* in psychology, you know – perception has always been fashionable, and sort of thought processes, and problem-solving, and all those things were *gaining*. But there was very little interest in memory. It's funny because it is an industry now, you know, memory is everywhere. I mean, not just among physiological psychologists.

But, yes, so it was very difficult to get across the idea that, of course with HM it was a pretty big lesion, but such a circumscribed lesion, was having such a huge sort of general effect, you know, on the life, on everything. And so it must be – may be – in HM, we couldn't see in his brain. And, you know, of course, we did have our other patients here. But you know, maybe there was

something sort of peculiar about those patients. Or maybe he didn't really have a memory impairment – or something ridiculous. It was very important to get an animal model. And there it took a *long* time because – even Scoville came up here and operated on a monkey – it's – it's you know, with a human being you can instruct, you can instruct and so you can – an animal has to find out what the problem is for himself. And so this is something they are learning over multiple trials. So, you know, I would say the essence of this is that – I mean, what you see was very impressive – was the way – and I've told this many times – but the way a patient like HM, for example, could hold on to a piece of information by rehearsing it, you know, like a number. He can keep playing with it if he's told to remember it and then after twenty minutes, can give it to you. So this was impressive. And probably I underestimated the importance of actually verbal rehearsal. I think I thought you could rehearse in your imagination, or something. I don't know what I thought. I thought there would be a good visual representation you could play with too. And so, but essentially, animals, it was Mort who pointed out – you know, I would say, "Well, get an animal, get a monkey going and learning a task, and then before it is properly learned, interrupt it with a different task, and then it should have forgotten the first task." But the monkeys hadn't. They trotted on just as well – you know, they carried on. And so, this was really, "How come?" And it was Mort who pointed out, "Well, look, what the monkeys are learning" is "habit" – what Larry [Squire] described – coined the term "procedural learning." But Mort said they were just learning a habit and it was a different kind of learning. When you are learning over multiple trials like that, by trial and error, it is a *different kind* of learning from when you can just say, "Remember this," and you hold it in your mind.

And so, it was quite a lot later – we had evidence from some work here that – using the [Jerzy] Konorsky kind of task – that HM actually couldn't maintain a representation – a visual representation – over more than about thirty seconds. But the most elegant demonstration of that came from Boston, actually, from Stulich and Moore – there is a reference to it – in Boston – who did an ellipse-matching paradigm. They would show you an ellipse of a particular ratio and then after an interval, you would see about eight or nine ellipses of different axis ratio and you would pick out the one that matches. For that, you really have to keep a visual representation in your mind, and HM failed after thirty seconds. There was no relationship between them.

HR: It was not just a verbal memory problem. It was clearly a visual – .

BM: Oh, that's not the point. No, we're not saying verbal memory. Of course, it wasn't verbal because he's forgetting the events of his life. The point is how does he bridge the interval. And the only way that he was bridging the interval was by verbal rehearsal. And I have some slides that I have in my lecture now in which I show a picture of a blue bucket and I say, "I haven't done this with HM but –."

HR: So you think that was critical. He couldn't do it through any other means.

BM: He couldn't do it anyway but by – and you would see, if he was by himself, you would see his lips moving – he was constantly rehearsing. But he couldn't maintain the visual representation without the aid of verbal rehearsal, which the monkey can't do. And so the monkeys were failing [in the Delayed Non-Matching to Sample Task]. It was twenty-five years after our papers, Mort's paper on a paradigm that he got from David Geffin in Oxford who had been visiting, spent a sabbatical year at NIH and introduced Mort to this paradigm I think. But then Mort showed it – and Mort Mishkin – and then put it with – it was 1976, or something like that. And then the world accepted it – that the monkeys –

HR: The delayed non-matching to sample – that was the critical piece.

BM: Yes, that's right.

HR: It's so interesting, how long that took, [BM: Yes] – before – of course, now everybody, of course – the hippocampus. But – you know, the structures around it are important. But I don't think people realized, in general, that it took from, you know, the time you were working on it in the '50s to the '70s [to be accepted] – they wanted the animal confirmation [BM: that's right, Yes]. It was really critical.

BM: Yes, it was, it was. Well, I think that's right. I agree with that. Because we couldn't look at [the brain].

HR: It took a long time. And there are so many other things you've done. I'm very interested in the Wada experiments.

BM: Yes. On the frontal lobes, as far as Luria was concerned. But Luria wrote – well, he invited me to Moscow in '66 for the International Congress of Psychology. That's where I met him. He was so hospitable to all of us. A very delightful man. Then I met him at various other conferences. I have little presents from him. But when we published this memory stuff – after that he wrote me a lovely note saying "Memory was the sleeping beauty of the brain and now she is awake," which I thought was very, very nice.

BWS: Are we at a point where we should stop now?

HR: There are some things we could touch on in a phone conversation. We don't want to tire you out?

BM: You are not tiring me.

HR: I think we should take a break.

BWS: Maybe we can reconvene this afternoon.

BM: You probably want to reflect on what we have covered.

[break in interview, end track 1008, begin track 1009]

BM: The frontal lobe story goes back, as most of these stories do, before my time. Because there was, well, in the years I was growing up in psychology, there were a lot of – there were a lot of exaggerated claims made about the frontal lobes – from the study of patients with very advanced brain tumors; and you know, there was [Ward] Halstead making some rather ridiculous claims with the ten little tests, which temporal lobe patients here actually did much worse on

than the frontals. But that's neither here nor there. The thing is that there was a lot there. Hull was saying you know – "The frontal lobes are man's hope for the future." And a great deal being written. And Hebb, among others, pointed out that these things were really not valid because these frontal lobe tumors declared themselves later than some of the posterior tumors and you were comparing, you know, rather big lesions with what were smaller lesions and all the rest. There were a lot of ways of criticizing. And then we had, at the same time, the Columbia-Greystone Project. That was a huge – it was because it was bad old days of frontal lobotomies and at Columbia, they, psychiatry, I suppose, they threw practically every neuropsychological test or psychological test, I suppose, in the book, at patients who were undergoing lobotomies. And they produced another of these big handbooks with the result that it was splendidly null because, and I think, it was a combination of the fact that they – after their lobotomies – the patients were a little more tractable – a little more cooperative – and so, and it balanced out. If there were a small deficit – if they were down on some things – they were going up because they were working better or whatever. There was nothing significant. It was a book this size and they found nothing significant about the effects, cognitive effects, of these lobotomies. And at the same time, Hebb, in 1937, was here [McGill] for a year and the patients he saw, Dr. Penfield's, and these were individual case studies – they were individual papers that they published – were patients with frontal lobe, you know, post-traumatic, or relatively slow-growing, tumors because Penfield started doing those before he embarked on the temporal lobe epilepsy surgery, you know. Again, Hebb tested them before and after, and they were showing normal performance on these using Stanford-Binet and various things. Anyway, and these, so both Hebb and Penfield came to the conclusion – which really Ritchie Russell quite independently came to in England – a neurologist in England – that of course you can't have this – think this – great big mass of tissue is doing nothing. But perhaps the frontal lobes really were important when you were growing up, when you were acquiring your abilities and your skills. But that once you had got to, sort of, cruising level, they were not so important, you know. You'd built up what you needed to build up, because there was really very little left evidence of effects. And so, on the one hand, you had these people like Luria – actually was one of them – but these were various people – making these huge claims for the frontal lobes on a slightly shaky basis, and Hebb and Penfield – very impressed by the negative findings.

HR: It was fascinating – you said also you were using patients with frontal lobe tumors as controls with frontal lobe tumors – ?

BM: Well, as the brain-damaged controls. Yes, it was obvious, I had to have brain-damaged controls. That was psychology – I'm a psychologist. Yes, I can't just test temporal lobe patients and say, "This is the deficit you see in the temporal lobes." I had to have a group of patients with comparable epilepsy and comparable surgery and all the rest of it who were not showing these deficits. I mean, that's elementary design.

HR: I see, you weren't necessarily saying they had no deficits, but they were not going to have the same deficits as – ?

BM: They did not have the deficit. They were my control patients, right? – brain-injured control. Otherwise I had no right to claim it's the temporal lobes.

HR: Right.

BM: The right-lefts are a very good controls for each other, but for temporal versus the rest, you need another part of the brain. And then, I started – I was following all the monkey literature – as it always inspired me – and I knew there were lots of deficits. The monkeys, you have to have the bilateral lesions except for some rather difficult auditory tests – the monkeys don't like doing audition – you have to have bilateral lesions to see the effects in a monkey, really. And then you see them. And so there were all sorts of frontal effects which I knew must be there in the patients if I had no means of testing them. One of the things, of course, was this difficulty with reversal learning with certain frontal lesions. So there were around the same time in the *Journal of [Clinical and] Experimental Psychology* – [David] Grant and [Esta] Berg had published from Wisconsin, from where [Harry F.] Harlow was doing the monkey work – this task where normal subjects – this Wisconsin card-sorting task which was about changing categories and – I read this in JP and I said, "Well, this sort of task I should be doing in frontal patients."

HR: That test was developed by a woman graduate student.

BM: That's right – Berg in Wisconsin. But it was inspired by monkey work, right. You have to realize the debt we owe to animal work. So I wrote to Grant – I didn't know him but I wrote and introduced myself, and I said that I was working here and – of course, I had no money. You realize I literally didn't get enough money to buy a pad of paper from Penfield or from anybody and I had no grants – nothing like that. I was just there with a small personal salary. So I wrote to him and he said – wrote back immediately – "I am so delighted." He said, "You know, I thought that this task would be of use with frontal lobes and I went to Columbia" – I think it was to Columbia – it was to some big hospital in New York City. And he said, "I went there to look at the records," and he said, "I gave up." He said "The medical records were so bad, I couldn't have deduced what part of the frontal lobes – what had been removed and so on. So it was useless." You know, this is more-or-less – you know, the same thing with Sperry's split-brain patients, because Laghi [Laughlin] Taylor and I went there, you know. Sperry didn't want to touch anything medical – "Oh, no, leave that to the doctors." – but we were used to the Montreal method where we went and we palled up [made friends] with the surgeon and we got to look at the notes there. And they were not useless, but they were very, you know – here – we always had the beautiful drawings and the measurements and everything – I mean we couldn't have done any of this work if we didn't have Penfield's drawings and Rasmussen's drawings and detailed operation records and so on. Electocorticography and so on. So he was delighted and he sent me all this material free. And I cut it up and made my cards and I started. And the results were *so* strong and so beautiful. And that was the paper – that's the really funniest thing. I published this in *Archives of Neurology*, which was the place to publish in those days for this kind of thing. But it took me so long. They were arguing with me about the size of the lesions, everything. I was consulting Mort – and I said, "I don't know what to do. I've never had such *big* effects. My effects with the temporal lobe patients are really, unilateral – are *so* small." I said, "The verbal memory deficit is a little stronger than the other – but they are *so* small – some of them .05 and so on – but I've never had any trouble getting my papers accepted. *Never*. And here is something with such a *huge* effect, such an incredible effect, you stand by and you watch these patients behaving, and you can't really believe you weren't color blind or something. I mean, really huge effects." I was having a problem. "Well are the lesions a little bigger, are they – or something?"

[laughs] So Mort said, "Well why don't you" – you know I had quite a quite a nice group of frontals by then – "Why don't you just cut them down to the ones with the smaller lesions that match your temporal controls?" – because the temporals were my controls in this study. And I did that, and the paper was accepted in 1974 [sic]. And that is a *main, important* publication of mine. And *that,* you know, is the beginning of all this – wave of frontal lobe injuries.[18]

So, then the next thing was the fluency when I had this – I still remember that – the patient – the young woman – who had had a left frontal – superior frontal lesion, you know – it was for epilepsy and she was fine. She wasn't aphasic or anything like that. Her verbal intelligence was fine. But she was, she was a very pleasant woman and yet I noticed that she wasn't talking. And she had complained to me – she complained to me later that she had just had a baby and she had hoped that people would come and see her, but they had stopped because she wasn't talking to them. And they thought she didn't want to see them. Whereas she just didn't have any, much, spontaneous speech, you know. And it was the same in writing, and some of them would say. I thought, well, this lack of expression, when their vocabulary – when you ask them to define something – the meaning of words, they know the meaning – you give them memory tests and they pass all the memory tests – the verbal memory tests the left temporals they were having with it – but here they are with no spontaneous speech and yet they are cooperative. I mean, they are cooperative and friendly. They would not say, "Arrgh, psychology." They were – so that is when I started giving them Thurstone Word Fluency and got these huge effects.

And I can remember very clearly the day when I had this young man. I love frontal patients. I would rather work with frontal patients than with – but I remember this young man and he had a left frontal removal – a limited one, you know. I gave him this word fluency and one of them is four-letter words beginning with C. And I thought, and I saw, "What's happening?" – because all the way down the page he was writing. [laughs] He had written "can't" all the way down the page. And so, he was very preservative. But anyway, this frontal lobe stuff was very much fun because one of the cases that Penfield and Hebb made a great deal of – they wrote two big articles about it in the *Archives,* I think, was a patient – was really our Phineas Gage, you know, because he was a man – this was before my time – a man from Nova Scotia who had had a really

[18] B. Milner. Effects of different brain lesions on card sorting. Arch. Neurol., 9, 90-100, 1963.

Phineas Gage-type accident, an accident at work that had damaged the anterior one-third of both frontal lobes. And he'd had very, very bad epilepsy and also a change of personality like Gage, you know. He had been an ordinary sort of workman. But he developed very bad epilepsy – post-traumatic epilepsy. And so he came to Montreal and Dr. Penfield had sort of cleaned it up. Really, I mean, it was about one-thirds, but he operated on both frontal anteriorly – I mean, Dr. Penfield didn't operate in both hemispheres but this was already – operated on this man and cleaned it up. The patient hasn't had – this was years and years ago – hadn't had a seizure since – it was completely successful for epilepsy, though he never worked again. He had a pension, I presume. And anyway, Hebb did all these tests on this patient – IQ and all sorts of things – with no change. This was a beautiful case for them, you know, to prove that the frontal lobe is really overrated. So we were able, years later, to get – I say this, I think Sue [Corkin] was here then – I and my students – we were able to get him back on a follow-up. And I repeated every single one of Hebb's tests. I mean, the literal tests, not just – because I was using slightly different tests. I used the tests that Hebb had used. Absolutely confirmed everything he got. And his visual memory was excellent. I mean, I have a nice slide that I showed at the Academy of Neurology – the Association – and the face memory tests that HM and FC and PB had all had, and absolutely at chance on – and he'd shown a beautiful disassociation. But his card-sorting was absolutely – I think he sorted one-hundred-and-twenty cards to *form* or something like that. It was a beautiful, visual instance of there being nothing wrong with what Penfield and Hebb reported. But they had not used the right tests essentially. And so, where the influence of the animal work was so strong.

But Hebb was very funny because he was my mentor. He was that. And he had to put me in for certain honors like you do – you put your students in for things. And the Royal Society of London, which he was a fellow of, and which I am now, but he put me in for that – and that's not something you get into on first try usually – it takes – you hung, as they say, for up to five years and then, if necessary, they take you down for three years and put you up again. That's not very optimistic at that point. But they do put you up and you can wait a few years. And so Hebb wanted to know all about my work because he was writing the thing for me. And then your job is, if you're the sponsor, is, you know, to see that the file is up-to-date. And so Hebb would write to me and say, "I'm doing this." And it would always be – "I think I know about your temporal lobe work, but it seems to me you found something out about the frontal lobes, but I am

blessed if I can remember what it is." [laughs] So each year I would tell him what I was doing with frontal lobes and he would dutifully report it and then forget it. It was very, very funny.

And then everybody got so excited about the frontal lobes and I got some wonderful recruits here like Thomas Powers – who now has a big position in Toronto – from the Czech Republic – because I met him at a frontal lobe meeting in Amsterdam. And Adrian Owen that we got from Cambridge is now at the University Western in Ontario. He's done this work on unconscious patients – you know – coma – patients in a coma – getting responses, imaging stuff. Anyway, but the people were all coming here – Michael Petrides who is my sort of, successor, administratively, in our little group – came and worked with frontal and temporal lobes in monkeys in Cambridge and then was referred here. We were all doing – it's this huge link – between work with monkeys and work with people, that has – and of course, with the hippocampus – there is more work now, also with rats, it is more feasible. But the, yes, the huge – I couldn't exist without the animal work and the feedback.

HR: When you talk about verbal fluency, I just had a question. You know, I know Luria talked about the transcortical motor aphasia – what did he call it [adynamic aphasia] – he had a particular name for the aphasia. But he was very interested in that.

BM: That's right. Yes. He loved the frontal lobes, Luria did.

HR: Yes, Yes, Yes. And I know way back when, [Ludwig] Lichtheim talked about the transcortical motor aphasia – difficulty with production without other linguistic problems –

BM: Yes, yes.

HR: So there had been some – it's interesting to hear you – I understand the background in the memory work and how, at the time when you described your work here with Penfield, and discovering those patients, it wasn't really known that the hippocampus was important for memory, or the adjacent structures at all.

BM: Well, it was 1896 – there was some, there was some – the problem with the, with *those* studies was – they didn't use tests, right, they were just [HR: descriptive] using an exam. But the real problem was that – the gold standard was clinical pathological correlation. But they would people who would be dementing over a period of years, or people who had more than one kind of neurological episode, and then you would see this at some phase, but the pathology – it was very difficult to *prove* – you could say, well it did seem as though the hippocampus was – .

HR: But the surgical lesions really helped you.

BM: Oh, *of course, of course*. This was the beauty of this place. I should do justice to the Neuro. What was *special* about the Neuro was that here was Penfield turning a huge bone flap in the skull – you see, everybody was operating through little holes, right? Like they do in your stomach now. They were all doing it through little holes. And they couldn't really see what they were doing. I mean, I guess they could see the structures they were targeting. But Penfield didn't know exactly where the seizures were coming from, so he had to have a huge bone flap. This was essential to the epilepsy surgery, that he would turn a big bone flap and then do – and that's when he did all this mapping – because no two cortices are exactly the same and you want to know – and you don't want to be taking out a bit of the speech area. So you have to map sensory and motor – and so you have to map all these landmarks. You can't just look at the brain and know where they are. And now this, this, *now* a lot of this is done with pre-surgical mapping, with imaging and so on, but *then* it was being done right there in the operating room. And *that* was what made this such an amazing place because you were seeing so much of the brain and you were getting these measurements and things, whereas other surgeons were operating through holes. And so, it was [HR: it was unique] – it was unique.

BM: Penfield studied with a surgeon called [Otfrid] Foerster in Germany, who was the first person to do the cortical stimulation, I think. Penfield trotted over to Germany and said, "I want to observe what you are doing." And then he came back here, invented some new instruments, and so on and so forth.

HR: So what I was interested in is – that is really an important point you just made – but I was less familiar with the sort of context – or the overall impression at the time – that the frontal lobes were not so important.

BM: Because it had been – because such exaggerated claims had been made on very poor evidence – really, really poor evidence – on what we call messy lesions, you know. And then the Columbia-Greystones thing,[19] which I *really* think – I mean, I had nothing to do with that but it had a *huge* influence in the United States. It must have cost a lot of money to fund. And lobotomies were all over the place – I mean, being done all over the place. And it was huge volume. I am not exaggerating, it was real tome, you know, that came out. And they gave every test they had almost ever heard of to these patients. And you know, expecting to find something.

HR: And then, there was sort of a swing back.

BM: That's right.

HR: And you were part of the more – differentiation of what the frontal lobes were actually important for.

BM: That's right. As I said, because Hebb, when he had seen one right temporal lobe patient, he did make one interesting comment about her – and it was a woman – about her sort of social interactions not being very good – or something – or her sense of humor – he made some critical observation of that kind, which was probably valid. I mean, valid for more than her. But the temporal lobe – it was all the monkey work that was inspiring us. And the field was wide open. Of course, the frontal lobes had been very thoroughly – they are often damaged – and there's a lot – but there had been this swing the other way. And Hebb's *own* experience with Penfield – it was not just Hebb, it was Penfield – the two of them – that they [HR: They weren't seeing it] – they were not seeing it. Well, you know, they were seeing first of all that – that they were seeing that Penfield's operations were not making the patients worse, which I think is true and is valid. But the *second* thing is that they were giving – you know, they were giving – intelligence tests –

[19] 1947 study evaluating the effectiveness of lobotomies.

and they were not finding that these people were outside the normal range over handicapped, which is also *true*, you see. But this patient – I still call him our Phineas Gage – he really *was* the bilateral – the frontal pole – bilaterally.

HR: So, I didn't realize that *you* were one of the first people – I remember you mentioned, Wisconsin card-sort, putting the WCST in, sort of, clinical context.

BM: Definitely. Yes, Yes, Yes.

HR: You know, The Wisconsin card-sort test is probably the most widely published-on frontal lobe test.

BM: But only since *my* time.

HR: Since your time, exactly.

BM: 1973. *Please.* [laughing]

HR: That's what I mean to say. You started a landslide.

BM: 1974. I mean, that test was based on – it was a task, not a test – that was based on monkey work – from Wisconsin. It was someone's – it was Berg's Ph.D. thesis. It was published in *JEP* [Journal of Experimental Psychology] as an interesting method. Not about frontal lobes.

HR: Yes, I see.

BM: And I read the – I read *JEP*. I used to read my psychology journals in those days. And this was something that – that I knew that the monkeys were having trouble switching. They didn't have trouble in the first acquisition, but they had trouble switching.

HR: But all I was saying was that, that *application,* that you started at that point, became so –

BM: It was so important.

HM: It was so important.

BM: It is something I am proud of.

HR: Right, right.

BM: I am in some ways prouder of my frontal lobe work than my temporal lobe work. But it's all, it's all been swamped by the interest in HM as a person in the popular press, you know, in the world.

HR: There is an interesting aspect to communication of science – and – when you have a person that represents an example – it communicates it in a way that other things can't. And so, that's maybe part of the reason that – .

BM: That is why I said Phineas Gage about Hebb's patient – because it was a similar lesion and everybody quite knows about Phineas Gage.

HR: You mentioned Carl Jenson, oh, no, is it Carl Jasper [BM: No, Herb Jasper]. That's what I meant.

BM: Herb Jasper – I really admired him. He was the one – that the psychology – that Hebb admired, and we all came here because of him and his lectures. Well, he, Herb, Herbert Jasper, actually started life as a psychologist which is probably one reason why he was quite critical of psychologists – he gave us a hard time, but in a nice way. But he was a complicated personality. Herbert Jasper – he was at Brown University and he began in psychology – he was, he was a pupil of [Carl] Seashore – the man who invented the music tests, and so on. And then he changed fields from psychology to physiology; and he was involved in the very, very *early* days of EEG. Really early days. There was the other chap in Los Angeles – these two people – they

were a little bit rivals. But, anyway, Herb Jasper was here in the beginnings of EEG and the founding of the EEG journal. And the IBRO, you know, you probably don't know that, was founded by an act of the Canadian Parliament. The International Brain Research Organization [IBRO] was founded and Jasper put it and got it through the Canadian Parliament. So we all joined IBRO because of the ground roots because of this – it was a Canadian thing. But anyway, he was – so Dr. Penfield started here with his colleague in neurosurgery, William Cone, who was doing more back surgery and things like that. And he, Penfield, recruited Herbert Jasper. I don't know how he knew – Penfield was American, of course, you know before he became a Canadian and so did Jasper, but they were both Americans. And – Rasmussen never changed his citizenship – he was American too – but these other two did. And Penfield recruited Dr. Jasper to Montreal to help because he needed, absolutely, the electrical recording, in surgery for epilepsy. It is essential. And so Jasper was a neurophysiologist and did some of the very early single cell recordings here, and so on. And he ran the, sort of, scientific seminar that we had every week – a much smaller group in this Institute then. We had the floor above here, and these seminars every week. And we were all – he could be very sarcastic – and we were all frightened of what Jasper might say.

[break in interview, end track 1009, begin track 1010]

HR: And so, I think that we were just speaking about Herb Jasper (s).

BM: Herb Jasper. Yes. It's not Jaspers, it's Jasper.

HR: Yes, Jasper. OK

BM: Yes, yes, I was saying he created our IBRO. Yes, that he was the – yeah, I know what I was going to tell you was that uh – [laughing] I remember the – one's vivid memories – he was on my thesis, at my thesis oral, you know. The external examiner is not there, but the internal examiner was there – and Dr. Jasper and the head of McGill or whatever. And so it was Hebb and Jasper essentially. And I had a finding in my thesis which *I* thought would please epilepsy people. It *was* that – whatever I was finding – this is the unilateral lesions left and right –

whatever I was finding – post-op – was only – was at most an *exaggeration* of what was present pre-operatively. That pre-operatively – I mean – this is how these things become useful diagnostically, of course, in epilepsy and lateralization. But they were – so I *thought* that the idea that – I mean, the surgeons certainly in those days were wanting to be reassured that they were not doing anything bad – and here you were having people's IQs sometimes go up, and it certainly doesn't go down. And these little deficits in memory that they had were evident pre-operatively, and now merely exaggerated. So I thought, "Well, this should, you know, should please Dr.Jasper," – I was thinking. And then he looks at me and he says, "You know," he said, "those removals that Dr. Penfield made were quite large. If this is all you are finding, then I suggest that you are using the wrong tests." [laughs] So that was what Dr. Jasper was like. He had this little smile at the end. But he became so *mellow* in old age. And people who only knew Dr. Jasper in his older years would not realize what he was like. He had this, this, it was a sort of mischievous streak that made him – you know, they often, because the Neuro was very hierarchical, it wasn't just anybody that got to speak at big – we had a neurological society meeting every week – we don't have those any more – in the amphitheater here. And people came from the other, from the French side, from the other hospitals – they came to this. So it was a big audience. And then, you know, Dr. Penfield would introduce the speaker and then somebody would be designated to give the thanks. And it was always Dr. Jasper. Even if there were a few other things, Dr. Jasper would do the final thing. [comment – closing door to room] And *always*, he would get up and there would be this – and he was meaning – and I am sure in a way he was in a way meaning to say something *nice,* you see. But *always* some *little* thing would slip out! [laughing] We were always waiting to hear what Dr. Jasper was going to say. But, as I say, when he got older, he mellowed a great deal. But, of course, he left here – that was – some – well, that would be off the topic, really off the topic, of me atall. But there was a policy dispute – something – in which Dr. Jasper, you know, said something in public – that was unacceptable, I think – to Penfield. And Penfield was not director anymore, but he was present. And the glasses came off – whenever Dr. Penfield was annoyed, the glasses came off. [laughs] This gesture. And Dr. Jasper, you know, left the Institute. But he went to the – he was perfectly bilingual – and he went to the University of Montreal. And he had a second *career* there, really. They absolutely loved him. And see, we've both got *ties* to the University of Montreal. But he started coming

back here. And I remember, you know, a few years later when he was really back, he said, "Oh, you know, I love the old place." [laughs] And he lived to be 92.

HR: It sounds though, that you feel, very fondly of – it sounds like you had a very fond relationship [BM: I'm very fond of Herb Jasper, yes] with Herb Jasper. And he had this *psychology* background, but had become a physiologisy person. [BM: that's true] Very important – for neurosurgical

BM: But it made him critical of psychology because he knew what he was talking about.

HR: Right, but he kind of bridged the gap in a way.

BM: He did. He did bridge. Yes.

HR: And you were working with Penfield.

BM: Yes, but Jasper was in the gallery. Jasper was doing all the electrocorticagrophy.

HR: Did he – was he involved in your discussions?

BM: Oh yes, always, yes, always. We had these – we called them EEG conferences. They were seizure conferences in which we had the patient and we had Dr. Jasper and Dr. [D.L.] McCrae, the head of radiology, and Dr. Penfield and resi – and the patient, and the resident.

HR: So, in your publications, though early on at least, it was Penfield and yourself, but not Jasper?

BM: But Jasper had nothing – he didn't have anything to do with the testing of the patients.

HR: He didn't?

BM: No. No. Nothing.

HR: Or the discussion?

BM: But I had friends, I had friends from the psychology department who went up to work with Dr. Jasper and published with him. But they were working with – unit recording in animals and things like that. They were doing electrophysiology.

HR: Where do you think, you know – this is sort of an open-ended question – but, sometimes you talk about these meetings that took place on a weekly basis.

BM: That was to discuss a patient, sort of patient-oriented.

HR: So it was clinically oriented.

BM: Oh, entirely. It was to decide whether somebody should have the surgery and, you know, so where the – where it *was*, you know. And that was where – see, that's a side of Dr. Penfield I did admire very much. He was a *very* good clinician. The resident, the resident's job was – [Aside: Well, we used to look at three patients. Now – you have one patient in less than three weeks, but then it was three patients. Because *now* we have so many – they have so many ways of testing them. I don't mean psychology, I mean, you know, implanted electrodes and all the rest of it.] But *then*, everything really – because we couldn't see into the brain, and because EEG was a bit primitive – we were really *very* dependent on clinical hunches really, whether to operate on a particular patient or not. And, this [meeting] was when the resident – the chief resident – would see the patients when they were admitted at the weekend and would work them up, take their history, talk to their family, talk to the patient, how the seizures began – all these things that are so critical – and then would *present* the patient. And then Dr. Penfield would get up and start questioning the patient himself, and questioning them. And saying, "Was it like this? Was it like that?" and so on. And Dr. Penfield – sometimes he corroborated what we'd been told – but sometimes you got a totally different picture at the end. And he, he had – this is a side, you see – he could be arrogant – arrogant it's not a nice word, I don't mean arrogant, but he

could be a little *authoritarian* – is the right word – with respect to his own staff and the people in the Institute. But he was never like that when he was listening to a patient in the operating room. He was extremely respectful. It was Dr. Penfield before nature, if you like, or whatever. As you say, what you were saying, about how you feel about a patient – here is the patient and he was listening, you know, as though he was listening to the tablets coming down from on high, or whatever – and would be transcribing and, make sure, well he would dictate immediately, and the secretary in the gallery would transcribe, and then we would get that printed report – published – report. But you see, that's why – one thing – you may disagree with Dr. Penfield's theorizing or agree with it or modify it – but the – or he might modify himself – but you *cannot* disagree with the facts because they were *meticulously* recorded. And he listened *so* respectfully to his patients. And this is what I admire about him because I don't find that all doctors do that.

HR: And he also meticulously documented what he did?

BM: That's right. Oh, Yes, Yes.

HR: You had very careful – .

BM: Beautiful operation reports, yes.

HR: So, I was wondering if there was a forum where you felt intellectually stimulated by the *team* in some way. Did you have meetings where you discussed *your* findings with them, or was it more one-on-one?

BM: Yes, no, no, I would – but you see, pre-operatively, we would be talking about a particular patient. And it would be pre-operative. It wouldn't be what – .

HR: What you found afterwards.

BM: No, no, no.

HR: What the deficits were. That took place, those discussions took place - ?

BM: In a conference room, a bit bigger than this one because there were – .

HR: There would be Penfield?

BM: It would around the table. I have a slide when I got promoted to the table. I was seated at the back at the beginning. And then eventually – and they would turn around and ask for my comments about something. And then there was a time when I got around the table. The table was the radiologist, it was Dr. Jasper, was Dr. Penfield, and then Dr. Rasmussen when he came. Dr. Rasmussen came back for a year before he took over as head of the department. And so Dr. Rasmussen and Dr. Penfield. And then later when Dr. Penfield retired, it would just be Dr. Rasmussen and Dr. Jasper.

HR: So this, this kind of solidified your relationship with this group, but it wasn't the place that you discussed your findings in your work so much?

BM: No.

HR: This was the clinical.

BM: If something was relevant to whether they should be operated on, yes, yes, yes.

HR: But, was there another forum where you were able to meet with people to talk about your work –?

BM: Well, I could meet with them every day.

HR: Just, Informally.

BM: Oh, Yes. Yes. Well, well, of course, the day that we had our first bilateral speech case, and we were so excited about it, and it was so funny because we had gone – as soon as we'd get any observation on the patient after the surgery, we would go and write it in his chart, right – would have to do that. I had to train my students to do that. And so, but after this particular patient, we were going every day. And I was going, and Dr. Rasmussen was going, and the chief resident was going – we were all writing our little notes in the chart – a very well-studied patient because that – we were very excited about bilateral speech.

BWS: Heidi was talking to you a little about the atmosphere around here, I think too, and about when you started especially. Was there a real encouragement – and you know, did you find that there was that kind of collegial atmosphere that encouraged you to continue to do the work that you wanted to do?

BM Oh well, of course, it was *always* – I mean, what we are proud of today is that through a sequence of directors – and we have had several – and they are not all – they have come from different places and so on – they've not all been in the Neuro tradition when they came except for Dr. Rasmussen – well, Dr. Rasmussen and Dr. [William] Feindel were surgeons, but the others have been, you know, from outside or from – and in spite of that, they must have picked up the Penfield mystique or something because there is very definite – we celebrated our, what was it, our 75th anniversary a couple of years ago. It was founded in 1934, this place – and I think this, this, feeling – well, Penfield's idea – see I think this is what his big contribution was – more than the surgery was that – the surgery was the raison d'étre – I think that he had this idea of bringing people from all parts of the world. We had people from China, and India – and Dr. Rasmussen continued that because he had been in India during the war [WWII], and people, of course, from the United States and Britain and Canada – and that people from all over. We had many languages going on, around the Institute. We had all this, you know, all focusing on this idea of the brain sciences. And this is, of course, what inspires the Institute to this day, but he had it very early, right – this vision. And we had to keep adding little bits to this small building that it started with. We are still – very overcrowded—it has always been very overcrowded. But the spirit was there from the beginning. And I think he had a vision. He was a man of vision in that way, very definitely, Dr. Penfield, I would say.

BWS: Is that what helped, what inspired or excited you about coming over here when you decided to make the switch?

HR: No, it was the *patients*. I wanted to know about the temporal lobes. [laughing]

BWS: Ok, so you knew that that – it didn't have anything to do with, you know – who?

BM: No I wanted – my *home* was psychology, you know. I mean, I was much more at home with Hebb and the crowd down there than I was with the crowd up here. Much more, I mean. Here, I was, I got to know the people I was working with immediately, but I was not part of this medical world atall, ever. I was at home in science. But it's right just down the street, right? That was home, not here.

BWS: That was what I was wondering.

BM: I built it up here, you know. I built up what we are here. [psychology at the Neuro]

HR: Can I ask you just about Donald Hebb, then. Was there something really unique that you want to mention about him that inspired you? Or you talked about how he helped you learn to write, but … what was your relationship like with him? Was there something unique you want to mention about how he was as a mentor to you?

BM: I would say, you know, very little in this. That Hebb gave you a completely free hand. When he asked me – well, he treated me always as rather grown-up because I was older than the average student – I was about 30 [years old] or something – and because I had been, you know, in England and I had been through the war and I was teaching at the U of M, so I had probably that kind of – I certainly was better educated in the sort of things we were talking about today – psychology and all that sort of stuff – than the students here were. There is no question. Not that Hebb necessarily thought this was *useful* knowledge, but I had the education. I was better educated – or differently educated – put it that way. But Hebb was a – basically a very shy man, I

think. He, Yeah, he was, he was quite a shy man. He was also very blunt. All the examples I've given to you are of somebody who is very outspoken and blunt. He wrote very well. He had very great respect for the English language. But he didn't really – it's funny – he had a lot of – he had quite a lot of women students, but – which is a fact – but the funny thing is that I don't think that he was very comfortable with most women. And particularly not the sort of women who would be standing up to him – I don't know. He had a sort of teasing, sort of, relationship at times, but I never felt that he was totally comfortable with women. I don't know, I mean. But you see, it is very funny. Mort Mishkin, I probably shouldn't be quoting him – or you shouldn't be quoting him, maybe, because it is hearsay, because he didn't feel that Hebb really appreciated all this work – this *lovely* monkey work – he was doing. But I'm sure, I think that Hebb *did.* But I think Hebb was at heart very, very interested in education. He started life as a schoolteacher. I think that he had all sorts of – he was *very* Canadian and very caring about Canada. I think he had all sorts of issues – educational issues, things like that – that interested him. He really took his 101 – which was a sell-out, his introductory psychology – I think a lot of psychologists are like that and probably in other fields too – that they like to put their imprint on the incoming student. And so that, not just at graduate student level, but, at really, the undergraduate level – he became very, very interested in the undergraduate teaching process and Psychology 101. And I think that he felt that, that we were – he had a very definitely the idea that you are grown up now. I mean, it was very, very funny, once you got your Ph.D., you were supposed to call him Don. But calling him Don because we never did – we always called him Hebb – but the fact is that, *that* meant to him that you are grown up now and you don't come to me for advice or anything. The one time I did, he gave me bad advice, about the Penfield and where I should have kept my name first on that paper.

HR: Let me ask you, though, then, you have talked about Mort Mishkin.

BM: A good friend.

HR: And, certainly, you mentioned Larry Squire a few times.

BM: Well, he's junior I mean, you think of him now, as, because he does a lot, but I remember him when he was a student around MIT. And he was trying to get a – everybody was looking for amnesic patients at that point, at that point, you know – and he had this patient with a Parinaud's syndrome from a wound, a fencing wound. And he kept saying, Larry kept saying that this patient was amnesic. He obviously wasn't – he had a verbal memory deficit. He wasn't amnesic. And Sue Corkin took him away – and Sue Corkin is a very meticulous tester – and she tested him. She was a post-doc then at MIT, and she tested him and she came in and she said, "He's got a verbal memory deficit." And I said, "Well, That's what I told Larry, he's got a verbal memory deficit. He's not amnesic." [laughs] And Larry then wanted to publish this is in *Nueropsychologia.* I was on the editorial board there and I said, "But this, this isn't – Larry, it's not, you know –" In the end, the paper was published, and it was certainly – you know, it is very *Neuropsychologia*, I'm not saying the case shouldn't be published because it was Parinaud's syndrome, you know, and he'd been very well tested, but this wasn't a – probably, Larry might admit it to this day, I don't know – but it is just to show you, not just about Larry, but about everybody – that certainly after people having said, "Oh you know, it can't be," suddenly well, maybe am – maybe amnesia would be interesting – maybe I should get my amnesiacs. And of course, we saw it very clearly in England with my colleagues and friends – Larry [Lawrence] Weiskrantz, Elizabeth Warrington – that they went very seriously – the very, very good psychologists. They went very seriously into this with the herpes encephalitis which they realized was leaving – a very pure syndrome, really, of this kind. And they did some very innovative work. But I think I made it fashionable. [laughs]

HR: Right, right. Well, you know, somebody said, I think it was [Eric] Kandel, right, who said some very nice –.

BM: He said some very nice things.

HR: About you. And he talked about you as *founding* or establishing the field of behavior – I think that bringing the patients into the neuroscience equation. This impact that you are talking about right now.

BM: You know, I am beginning to think, I am beginning to think that there is a little bit of truth in this. I didn't found a field – because there is clinical neurology in Europe, I assure you, is very different than – those people like Henri Heacaen and Oliver Zangwill, psychologists – were certainly leaders there. But in terms – but they were individuals, you know, and Henri's students did not really follow in – show the promise that he'd hoped, I don't think. But it's Oliver Zangwill that got psychology established at Queen Square through Elizabeth. He put Elizabeth – he got, first of all, the habit of going once a week himself to Queen Square, just looking at the patients, and not doing any testing really, but a little bedside testing. But then he got Elizabeth to go there and she stayed, of course. And there have always been psychologists – neuropsychology – at Queen Square since then.

But I suppose, in terms of time and dates, I was ahead of that, because I was here. But I think it just goes from Dr. Penfield saying, "We *need* you." And that was speaking for itself – that there was a role for a psychologist. I think somebody like Dr. Penfield who was, you know, obviously a very intelligent man and very, very observant, as I've told you, with his patients, and who was collecting all these data in the operating room – I think he assumed that he could be his own psychologist. Psychology – he didn't think it was a bad thing; he had Hebb here after all. He probably thought it was common sense, a bit – psychology – and that he had lots of common sense. He does have – I mean, Dr. Penfield had a lot of common sense, I would say. It is not that common around, you know, around – [laughs]. And I think he was finding that he could manage very nicely, thank you. But Dr. Penfield, because of his openness to people coming from all over – Dr. Jasper explained this to me once. He said, "You know Dr. Penfield, if he was interested in something, he would just get on a plane and go there and see it." This is what he did when he went to work Otfrid Foerster, he wanted to see how this recording was done in the operating room. So he just went there and said, "I want to study it." And Dr. Penfield was very welcoming – sometimes I thought almost too welcoming – of anybody who arrived on the doorstep and wanted to observe, and do something. He approved of that. He was very, very open. And so, it was in this spirit that he had Hebb here, and that he had me here, and everything. And then suddenly the tables turned when he, when he got these memory loss cases, which *really* troubled him, because he didn't understand them, he wasn't sure how he was going to assess these risks. But if these risks were real and could not be avoided, he would have to stop that kind of surgery

because – and warn other people of it. And so, that was when he thought, "Well, maybe there is something in the actual discipline of psychology which is adding something to my own good sense," you know. And so, that was how I was encouraged and then, of course, with all the data we published and the people I trained, it became fashionable, then I suppose.

HR: It's interesting – you know – I wonder about – you said – that they let you be very independent.

BM: That's true.

HR: And I think sometimes that's good if you have the ability to be independent. You're not being told you *have* to do this and have to do that. You can create your own space a little bit – as long as you are protected from other – so it's not always – sometimes that's a good way to be able to establish yourself. It sounds like rather than being led and supported, and here – "look at this, look at this – specifically."

BM: All Hebb said, to me, when I went up here, he gave two pieces of advice. The first one was perhaps – I didn't need to have given – and therefore was perhaps not useful, not good. He said, "Don't get in anybody's way." And I think I went around clinging to the corridors. That was not good. But the other was very important. He said, "Make yourself as useful as you can." Those are the only – he never told me anything else. He never suggested what I might work on or anything. He said don't – about my thesis or anything. He didn't tell me what to work on. I got my ideas from the monkey work. It was, more "Would you like to go up there and study the patients? And make yourself as useful as you can." And so, this was how I always felt – that anything that I found that was about the patients, that it was relevant. I tried to be part of the assessment of the patient – you know, everything with the team. Now this is exactly the opposite of what my very good friend, Roger Sperry, did, you know. He took a totally different attitude down at Cal Tech. He said, "Just keep out of all that medical stuff. That's nothing to do – just go there – you've got these questions you're interested in, and this – but just keep away from the medical stuff." And that was when Laghi Taylor and I went out to study – my colleague who passed away – went out to study – at Sperry's invitation – to study his patients with our own

questions that we were bringing from Montreal. When we went to get the reports that we were used to having here, there were no reports. And we had to go back – and the surgeon was very collaborative with us – and we went back – and the notes were not very good, but then we added these notes. And I was very interested in major publications from the Sperry lab, they would give these little histories that we published first. But he never had any histories, really, of these patients. And he selected these two – and we were studying other patients – there were a group or several. And Sperry would say, "Oh, you can't work with *that* one, you know, He's not very bright – or he has this and the other." And we worked with them – we were used to working with patients, you know.

HR: So in some ways, you were integrated with the clinical operation.

BM: Yes.

HR: You were seeing patients for clinical purposes.

BM: That's right.

HR: As *part* of your work.

BM: That's right.

HR: And then, in addition, you were seeing doing your own – you know, having your own questions. And some of the questions came from – from Penfield, too, when he discovered these patients – you discovered these patients. And he was concerned about – their well-being.

BM: That's right. Yes. Yes. Back and forth.

HR: It was a different model. When you talk about the monkey work, who's, exactly, work was influencing you at that time?

BM: Well, there was all this work coming from Karl Pribram's lab. I don't necessarily agree with Karl on all the things he – he's quite a character. A friend of mine, but quite a character. And Mort went from Montreal to Karl Pribram. And then Howard Rosvold, who had been at McGill as an assistant professor, went – was asked to set up an institute at NIH – and he had known Mort in Montreal, and invited Mort to go to NIH. And then Mort's really taken it, and Mort, of course, worked through the patients in London, too, as well as with monkeys, you know. And so, I think this idea of working with patients and monkeys is a good Montreal tradition.

HR: So Mort, you knew, Mort – when he was here in Montreal – early on.

BM: Yes. We were both Hebb's students.

HR: OK. So then that collaboration…

BM: He's nine years younger than me because I had been – you know, I'm older than most people because I graduated before the war.

HR: And then you took the teaching position before this department really started to grow.

BM: That's right.

HR: His – ?

BM: He came straight out of the Army. That was very funny, too, because Hebb, as I told you, was a frugal, sort of, Nova Scotian. And Mort came out with all the GI things [GI Bill for veterans after WWII]. And he was a middle-class Boston person before, but he came, you know, with the perks that went with the GIs. And so he had a grant to buy books. But Hebb had to approve. And Hebb said, "Nonsense, you don't want to read those." He wouldn't approve the ones that he thought were a waste of money. [laughs]

HR: [I have this list here of questions] I think we should go through some of these, just to catch a few. How did people view neuropsychology at that time? Not that you identify yourself as a neuropsychologist now.

BM: Oh, those didn't exist – I mean, those people didn't exist then – because that's an application of what research was doing.

HR: So maybe that was interpreted later – I mean, you know, the kinds of testing, so the Wechsler [Adult Intelligence] Scales – and the IQ tests – and that sort of thing.

BM: They were not being used particularly for – in a neurosurgical setting. They were being used, however intelligence tests are used – in school – they used all of them – and then in normal populations, no? That's how they're established – is in a normal populations.

HR: When you were testing, say, HM and you used tools like that to assess – ?

BM: Well I was – I'd be using them here. Hebb used the Stanford-Binet, which was the thing before that, and was very caustic about the Wechsler. But I started using the Wechsler, though I had to make up my own digit symbol forms, and so on. You know, it was just to have an idea of where a patient stood.

HR: Relative to normal?

BM: Right.

HR: And so it was just a tool. It wasn't considered a central thing or a discipline in any kind of way. It was like you had talked about, in England, there was this branch of psychology on measurement.

BM: Yes, well *that*, that of course, was – I suppose you'd get all the factor analysis stuff.

HR: Oh a lot of statistical analysis…

BM: Lots of statistical – very statistical.

HR: Not just the measurement?

BM: No, very statistical. Very theoretical, you know, about what the nature of intelligence was and so on.

HR: And then – let's see – we have down here – you've described yourself – I guess this is related to sort of – what you think is unique about your perspective? I thought it was interesting when you talked about Penfield having common sense. A lot of common sense – is not very common. But I think that is something important in order to see the forest for the trees when you are looking at a patient. But you've described yourself as someone who *notices* things.

BM: Well yes, that you see, I'm not theo – well this is very basic about me – I'm *not* theoretical, you know. There are different ways of being driven. I'm very much – as much as I admire pure mathematicians, I admire people who really can have, you know, a *theory* or some *concept*, and from *that* they say, "Well then, you get your hypotheses," and then,"Well, how would we test that?" And then, "Oh well, maybe with this patient group would help or something." But if you have some very definite idea – a specific idea about memory or – but you go in with a real idea and then you see how you can adapt it to the tools you have. But I come the other way – I am empirical. Something that intrigues me – like this girl who was so quiet although she was friendly, and not aphasic, and so why was she quiet, because she – you know? And just little things. I mean, that's a trivial thing. But odd things that a patient might do or something will excite – will excite my curiosity. And then it will be, "Well, you know, how can I measure this?" And that's what I always tell students again, that's what the experimental psychologist brings – tools which the neurologists in general don't have – they may be acquiring them now – but they don't have – they have the medical tools, but they don't have the – really the ways of studying behavior in my opinion. I mean they are acquiring it, but it's not part of the medical discipline. And a part of our discipline from the beginning is how to set up an

experiment, you know, and how to test something or design a task. And that, I suppose is – but I would be doing that – to answer my question about why is this person behaving in this way.

HR: I think that is so fascinating because – basically you are taking the patient's anomaly or presentation and trying to – that's asking the question. You know – [BM: yes]. You know, but sometimes you are presented, you know, with a natural experiment. [BM: That's right.] Which is what you *see* in patients - patients present that way. And so, it gives you a different window.

BM: Yes, well of course, a huge amnesic case, like an amnesic, you get the question of what *can* this person do? You know, you can't have the universal negative. You can't say HM can't learn anything because they will say, "Have you tried to teach him anything?" And then what do you choose? What do you select to use, you know? But, but, yes, the patient presents the problem. And so it did, the patients who said they have trouble, you know, with words, with remembering words. I mean, the patient tells you something is bothering them, and you try to see what they are talking about because they are probably not describing it correctly.

HR: And it is interesting because – well, well this is a little bit off the topic – and my interest, particularly, but you know when you are driven by a theoretical model, sometimes then, um you don't see other things, because – [BM; Oh yes, yes, that's right] – you are looking for certain kinds of things. [BM: that's right, that's right] You are asking certain kinds of questions. When you see the patients present with these questions – and I think it's interesting from the Neurology side of things, as a Neurologist – and you know we are recording this for a Neurology society – that, you know, Neurologists have this unique access to these patients who present themselves with these kinds of *questions*. And what *you* were able to do was interface with patients in such a way that – you know that with your knowledge of how to test – and also some theoretical background – could ask – you know, *answer* the questions that the patients present themselves with.

BM: Of course, for me, the other thing that was valuable, really, as an experimental psychologist, was the fact that – I suppose I'm grateful to the temporal lobe epilepsy because it is so repetitive, it is really an entity. But because I was getting these people who were coming from

all over the world to Dr. Penfield – that we were getting – I was getting groups – for me, I still like to have group data. I mean, I really have great respect of the individual case, I mean I don't want to have lots of amnesiacs because that would be a terrible thing – I didn't want to tell Dr. Scoville to go and get me some more patients – oh, no. [laughs] But the thing is, that when you do have an orderly series of patients like this that you can actually ask questions of right versus left without worrying about individual differences because your N is going to be big enough to take care of that – that is a luxury and I like that.

HR: So interesting, the scale, though is interesting, you know because subsequently when neuropsychology became a field and those who are neuropsychologists today of some types, you know, want an N of a hundred. Now that's really hard to get, and when you're looking at – ?

BM You don't need –

HR: You shouldn't need an N of a hundred [BM: Of course not, no], because in order to get – if you were to actually see something that is significant, you should see it in a few less patients. [BM: Oh my God, yes] So there are differences in scale.[BM: Yes, Yes, Yes] But of course, in stroke trials, they want six thousand. We're not talking *that* big. [BM: Depends what you are looking for.] But more than single patients. [BM: …depends what you are looking for]. A group large enough that you can make some conclusions about it.

HR: Go ahead, Barbara.

BWS: As we start to look back on your career too, is there anything else, you'd – I'd like to ask you a little bit about your roles as teacher, researcher, and philanthropist. And so that would be taking us in a different direction.

BM: I think you have covered – I wanted you to get the frontal lobe story. And that was important. Because I'm proud of it.

BWS: Is there anything else on that – before we?

HR: What about the Wada testing? Do you want to mention that?

BM: Oh, that's a whole other story. I mean that was wonderful, but that's a whole other story. I can tell– I could – well –

HR: Do you want to tell us?

BM: I could tell you something very quickly. Not the language – the language part we were very excited – the Wada – you know – this was actually fun. It was one of those seizure conferences, where, I was going to say, we were sitting around the table. But then you must visualize the lecture room *behind*, and about ten rows of people sitting and again in these typical Montreal tradition of those days – the junior residents – and so the junior people would be further back. You know, as I say, I graduated to the table. But, at one of these meetings, you see, we had a patient – I remember this very well – it was a patient with - who was left-handed. And he had seizures coming from – not the usual place – it was coming from back here you know – more posterior temporal, left hemisphere. And he didn't, we didn't have any postictal dysphasia or anything like that. And obviously, you know, Penfield didn't want – occasionally he did, just occasionally, after opening a patient and then close them because you know he couldn't operate. – but you don't like that because they get a lot of edema, and anyway, it's expensive and unpleasant. And so Penfield looked around and, rather rhetorically, said, "Wouldn't it be wonderful if there were a way of knowing beforehand whether this was the dominant hemisphere or not?" And then this little Japanese fellow out of the back said, "But, but, sir, but sir, there *is* a way." *Off* came the glasses. "*Nonsense*," says Penfield. So then Dr. Wilder said, "Persist." And he said, "But in Japan, you know, before the war, we did such-and-such, and we injected" – they'd injected using various drugs– not just amytal – and he explained the logic of the amytal procedure. You know, you knock out one hemisphere and test speech. And so Penfield, immediately, as he always did about things like this, he turned to Dr. Rasmussen and said, "Well, well, perhaps you'd like to look into the safety of this." you know. And so Dr. Rasmussen proceeded to do a lot of studies with monkeys here in Montreal – injecting deliberately into the vertebral and seeing what would happen and so on, anything that could go wrong. Just before

they would ever begin to do it with humans. And then we began to do these speech tests with humans. And that's a whole other story which is really not for today. It would take – about bilateral speech. We were all very, very excited in it, but it's – you know, you remember when we celebrated our thousandth patient, all that. But it was very exciting. And that was the collaboration with Dr. Rasmussen and his residents –.

But so, time passed, and this was all *pre*-HM, right, and then after HM, we came back into – always debating in this little conference room. Penfield liked to think in public, I think. It's good that, actually. It's quite good. And so he was worrying about how – he said, "Well, from now on, I am not going to operate on anybody who comes in with, you know, any bilateral abnormality in the temporal lobes," you know, because of this memory thing. You know, "It's too dangerous." And so, but a lot of patients come in bi-temporal, you know. So Rasmussen said, "Well, I don't know." He said, "I think that Brenda could adapt the amytal speech test, you know, to test memory before surgery and screen." Now this was incredibly *challenging*, first of all because – in those days – I mean nowadays we do it all with electrographic control – we do it in the X-Ray department – we do everything with every possible control. But in those days, we were injecting in the dressing room into the common carotid artery, not the internal. So we didn't even know whether the drug – well, we knew because we got a paralysis – but we didn't know about the distribution. You've also got a watershed right there and you don't know whether you are going to irrigate – you know you've got the speech areas because when you get the paralysis, you've certainly got the speech areas if they are there. But you don't know whether you've got the hippocampus or not because of the watershed area. And the other thing is, that memory of its nature, requires a little bit of time – a window of time. You have to show them all, give them something to remember, and distract them, and test recall. And you have to have that window of time. And the whole thing – the whole drug was wearing off in six-ten minutes and we had to do the speech testing and everything. So -it was quite challenging, you know. And I was not confident atall. Rasmussen was sure that Brenda could this, but – [laughs]. And so you know we did work out amytal memory procedures and now, of course, it's a lot easier to do. It was taken on again – this was taken up by other centers and with little bits of variation – quite legitimately – but by other epilepsy centers. They took up this idea of pre-operative memory testing.

HR: It became standard.

BM: Well now it's done in different ways. We do an E-som here, because they stop producing amytal, you know, and so on. But anyway, it's done always before our surgeries. So these are ways in which we have actually influenced – [HR: the practice] – the process, yes. It was all very exciting.

HR: You have done so many things there that were important.

BM: And nothing whatever to do with HM [laughing]

HR: I'm glad we got a chance to touch upon some of those other things. The Wisconsin card sort, the frontal lobe work, your verbal fluency patient, and the Wada testing. For the historical record, I think that's important. Let me ask you some – these are things I'm curious about. What do you think has been most satisfying to you about your career? I mean, what do you, I mean is it – I mean there are so many things.

BM: Well, I put in the end – the last sentence of that little biography you read [Brenda Milner in *The History of Neuroscience in Autobiography*, vol. 2, Larry Squire, ed.] – that to have established – I think to have proved Hebb – I mean I have great admiration for Hebb – to have proved – you know, he said it couldn't be done and I *did* it. He said to establish psychology in such unpromising soil – because it *was* unpromising soil – I mean, it was a place with wonderful opportunities but with the assumption, you know, that psychology was very peripheral. That Penfield was delighted to have his patients studied – very welcoming – not cold and unfriendly. But that, except maybe in a few extreme cases, they were going along very nicely, thank you. And Jasper, with his ambivalence. I mean, I loved Jasper but he was very ambivalent – the field he had given up. And anyway I think Dr. Jasper – I think it was his personality. I think he was ambivalent about most things in life, you know. And so I wouldn't say that it was particularly easy. The other thing is – and I'm sure people nowadays don't realize – that I was considered – you see a lot of psychologists from my generation and a little younger, say to me, "But Brenda, you were so *lucky*." Now *I* think I was lucky; I don't disagree with that for a moment. But what

is amusing is that nobody thought I was lucky at that time, beginning with Hebb who thought I was a fool. [laughs] Most people thought I was a fool, because in those days, you could do your psychology – depending on your temperament – whether you liked to work with rats or with undergraduate students – you took your twenty undergraduates or you took your twenty rats – and you did your experiment, and you analyzed the data, and you wrote it up. And you could do that. And you could count on getting rats – rats were not as expensive then as they are now. [laughs] You could count on having your twenty rats or even forty rats and you could count on having your undergraduate subjects, you know. And here you come and you don't choose who is going to have epilepsy and you don't go and say, "Here is somebody – he's got the right IQ level and the right age – I'm going to make a hole in his temporal lobe." A person has to have epilepsy. And epilepsy is no nice respecter of borders always, you know. So it was a gamble and people thought I was foolish, as I say. Hebb thought I was foolish because this was such a tough place. Other people thought I was foolish because it's not the way to do a nice neat experiment.

HR: Yes, you know, it *is* more challenging to work with patients so you know people who work in psychology who have their N of twenty and they have their series of four experiments and their undergraduate normal subjects. And you can get them all in. But if you are trying to get a patient population, just to get to twenty can take you years.

BM: Well, not here, not in the neurosurgical setting, because they were of a kind, really.

HR: Right, so you could get more. But still it is a different type of a challenge.

BM: Yes. Well, to get them cooperative and to get them coming back for follow-up.

HR: And you were integrating yourself into a clinical setting where they didn't see necessarily – you were a little peripheral. [BM: That's right]. And so, that was part of that challenge as well.

BM: Yes, I gradually became more central. You can see what a role Dr. Rasmussen played. You can see, just the few things I've told you.

HR: And now, remind me, had he had some background in psychology as well?

BM: No. No, he was a surgeon. He was not – it is a little bit dangerous when the surgeon thinks they be a little bit of a psychologist, you see. I mean, I don't go around saying I'm a bit of a surgeon, right? No. [laughs]

HR: But what about, I also think it's fascinating, that as you mentioned in Europe, there was a tradition where *neurologists* were, at least, [BM: Oh, definitely] *familiar* [with psychological approaches]. You know, you mentioned Hecaen – .

BM: Yes, and also the Salpêtrière Hospital [Paris] in language, you know.

HR: Yes.

BM: I went to the – I was invited to the Salpêtrière for a month.

HR: And I think the German schools [BM; Oh yes, Definitely, definitely] there were a lot of neurologists who were interested in – .

BM: Oh, but just before the war, you know, because of Hitler and so on we were not so involved with Germany.

HR: Right, but then, in terms of the neurologists in North America, that was *less* common.

BM: Oh, they were not – they were into clinical pathological correlations and so on. So, no, I would say though – I didn't meet neurologists, really, until – it was only when, only Norman Geschwind – he did become – he was very interested in language, but he was – .

HR: What other neurologists – you know, as opposed to neurosurgeons – did you have any interactions with?

BM: I didn't – you see – this was – not very much. You see, this is really a neurosurgical place. And Dr. Penfield, legend has it – I mean, I wasn't Dr. Penfield's confidant – that he did at one time try to invite – or invite – an eminent neurologist from the United States – I don't know who, I really don't know who – I'm not being coy – who said no. And of course, naturally, they said no. This place is really built around neurosurgery for epilepsy. What neurologist worth his salt – I mean a young neurologist is different – but neurologist worth his salt is going to want to come as a partner to Dr. Penfield in the neurosurgical –? You can call it – Penfield was very careful to call this a neurological institute – he never wanted to throw off neurology as some neurosurgeons – Scoville was much more of a neurosurgeon in that sense. Scoville was very proud of creating the American Society for Neurosurgery or something – or international society – which didn't exist. It was one of the things he did and was proud of. But Penfield felt he had – would like to have had a senior neurologist but they were not attracted – they were not attracted. We were never very strong in neurology, and this is, I think this is why. It was so neurosurgical.

HR: When you look from your perspective at other Centers then in the States, or even here –

BM: Which I don't know very well, right?

HR: That's fine. So, you were familiar with Geschwind.

BM: Oh yes, I was good friends with him. We both liked flying and we both took a thing that is totally out of date now. We took this manual, again, that came out about the airline guide – we both subscribed to the *Airline Guide.* And we loved, we loved, looking at routes to things. And often when I booked my flights somewhere, they would say, they thought I was a travel agent. I really loved it and Norman Geschwind loved it, and we just – and I was so heartbroken when there were no longer any airline guides, because, of course, the whole system is different now. [laughs] We were both very verbal, you know. Norm was very loquacious and very, you know, enthusiastic, and we got along very well. We didn't always agree. Doreen Kimura, my first graduate student, had terrific rows with Geschwind because she thought he was really – well, because she - her maiden name was Haug and her first language was German, and she could read all the German literature easily. And she said that Norman Geschwind's interpretations and

translations of some of the German literature were wrong on some very many important points. [laughs] and I would trust Doreen. But I still think he was a good influence in American neurology.

HR: And certainly he sort of, rekindled the interest in behavior ([BM: Oh, definitely] and localization [BM: Definitely] that had been kind of lost. [BM: That's right.] And so there was a whole school of people that were trained, sort of in his footsteps. And he was influential in pioneering and enthusiastic – and so there was sort of an era –

BM: Yes, yes. It was very, very sad when he died. I actually went – I don't normally go to people's funerals – but [Michael L.] Macklin and I, we flew down to Boston to his funeral.

HR: And were there other cognitive behavioral neurologists – ?

BM: You noticed she was saying cognitive?

HR: I noticed, that all through that she called herself a cognitive – so, you know, I'm sort of divided allegiance.

BM: I definitely am.

HR: Because I trained with somebody who came out of Geschwind's mold and who was affiliated with that term of behavioral as opposed to cognitive. But anyway, have you followed some of the more well-known behavioral neurologists? I don't know – we talked about [S.] Damasio who is a big thinker. We talked about – .

BM: I don't really follow them.

HR: Bruce Miller sort of built up the Center at UCSF, now recently, in the last decade or so.

BM: No, I don't relate to neurology very much.

HR: Interesting.

BM: I relate to, you know, the basic neurosciences.

HR: That is more your affiliation.

BM; Yes. Yes.

HR: You were so – as you put it – you made yourself useful in the clinical setting and became very intimately aware of the patients' needs and what they had to go through and their risks and assessments from the neurosurgical connections.

BM: Yes, that's right.

HR: That gave you a different perspective on the patient related work. You mentioned that in contrast with Sperry's work. So you did have this clinical side, but it was mostly interfaced with the neurosurgeons.

BM: Yes, yes, definitely the neurosurgeons. And I had visitors come, psychologists from England, actually. And I can remember their sitting and saying, "Brenda, how is it you get on so well with neurosurgeons? We have a terrible time with neurosurgeons." And I said, "Well, neurosurgeons are my best friends, really. We try to help them. They think we – they find us helpful, and we find them helpful, you know." [laughs]

HR: It's mutual, mutual appreciation. And so, I think we should talk a little bit about your feelings about teaching. You are a wonderful speaker. [BM:I love teaching.] – and so you, I'm sure – during the many years you've been in an academic position –

BM: I've had to do very little. You see, I'm very lucky because I – well, I served my seven years, we always say seven years – this is a non-Biblical person quoting the Bible. [laughs] But

seven years – I worked for seven years at the University of Montreal and I taught and I taught in French, and I taught, you know, the regular way. You teach a course and you teach a lab and you did – you know, I did all those things for seven years. I also taught during that time – I taught a night course at – you know, Concordia is another English-speaking university in Montreal but it was made up of two bits. One was Sir George Williams College which was really like, I think, a city college must be – downtown, people from all sort of nationalities working during the day and coming in at night – that sort of college. And then there was a very fancy Catholic sort of junior college called Loyola out there West somewhere. And they both applied for university status at the same time. And the provincial government said they had already made a new French university as well as the U of M – a really bad one, but anyway two French universities – and they said, "We can't have three English universities and two French, so Loyola and Sir George Williams must combine." And that's where the name Concordia comes from – Concordia University – putting together Loyola – this Catholic, upper-middle-class college – and this little vibrant city college downtown and making them one university. They've got these two campuses; they go back and forth. And it is doing very well – Concordia. But when it was Sir George Williams downtown I was invited to give an evening course in comparative psychology. And I insisted on making them do physiological as a prerequisite to comparative. That was my Hebb training. So I taught physiological and comparative psychology. Dalbir Bindra's wife, Jane [Stewart], inherited that course from me when I had too much to do here and gave it up. But that was a lot of fun, too, and was quite different. Because the main problem was I was required to read the attendance role and the names – this was, you know, after the war, but the early days after the war, and there were the names from all over Eastern Europe. So I didn't know how to – I really don't know how – you say you speak Czech – I admire that. I don't know how to pronounce – I can pronounce German, but I don't know how to pronounce these Eastern European names. So that was what I remember. But anyway, I did my stint. I served my seven years and so on.

But, here, it's quite different because in the medical school most of the teaching seems to be – Well I supposed I was doing that with my students – seems to be on the practical side, showing them how to do things. But the medical school teaching itself, there has been a course called Central Nervous Systems, Systems Nerveaux Centrale, for years and years and years. There was

psychology in it. There were a few courses – hours of psychology that Hebb used to teach. And when Hebb got tired of teaching it, I started teaching it. And then they changed the curriculum. They kept moving it. Sometimes it was at the end of the year when the students were just about to write their exams, but when they knew a lot. And then they said, "That's not good. This is so exciting, it should -" and so now it is as the beginning of the year. It is almost the very first thing they get before they have even seen a hippocampus, is this. I teach – now I only teach three hours and Michael [Petrides] teaches two or three. But at first I taught seven hours in that and it was reduced to five because they were condensing the whole course. So, I do these three hours in the spring. But they are always very successful. I always get, we get student ratings, I always get a rating above the average. [laughs] So I quite enjoy that.

HR: It seems mostly that you didn't have as much teaching responsibility here but you've had graduate students who worked with you. And certainly, you influenced all of those students.

BM: And the graduate students – I have always, I have never told people what to do. I've never had a program, you know that. I've never in my life had a program in which I can tick off. I mean I've got my grants and things. But I've never in my life had a program where I say, "Well, there is this experiment that needs to be done, and here is Jones-Smith who is a bright student. Why don't you do that?" I always told them to read what we'd done and go away and think, and come up with what they want to work on. And all of my students have done that. You see, Sue Corkin arrived and said – and she was so keen, she came in, and she couldn't get – in those days – see, now it is harder to get in clinical than it is in experimental – but in those days, it was much harder to get into the psychology department in experimental than in clinical. And so she came up from Smith College – an American girl – and she got into the clinical program and immediately wanted to change – not interested in clinical. So she came over to me and she sat in there – I had a big armchair, that sort of upholstery with big armchair – she sat in there and she said – with almost, almost, she didn't have tears in her eyes, but almost – and she said, "I want to work on Summer's thesis." And I was so delighted because I wanted somebody to work on Summer's thesis. And she did beautiful work on Summer's thesis. That was Sue. But before that, I was even luckier because, you know, I had myself done the basic – Seashore, the music, had shown this big right temporal deficit which has held up over the years for the pattern –

giving little Seashore tests. But I have told you I am tone-deaf. I am no good atall in music. I am not the natural person to go on doing music work. And Doreen had done the dichotic – but that is something else. And suddenly, I get Robert Zatorre, who had begun – but the thing about Robert Zatorre was that I was already prejudiced in his favor because there had been a paper that I hadn't liked that I had seen published somewhere by Tom [Thomas] Weber – but anyway, it was a paper I thought I didn't really agree with and Robert had just published in *Neuropsychologia* a very nice rebuttal of this paper and its experiment. So I knew Robert Zatorre's name from the fact that he had shown something against this article that I had not been convinced by. And then he comes and wants to be my post-doc – you know – to work on audition. You know, he had got the ideal background because his degree at Brown was half music – he is an organist – and half experimental psychology. So he has gone on – he has built up a really good lab here within our group. And so, in this way – so this is the kind of – .

HR: So people have come to you with certain interests that meshed in with – ?

BM: Meshed in with what I felt we were needing. We needed the audition and we got the audition. I was needing the Summer's thesis and there was Sue saying "I want to work on the Summer's thesis."

HR: But you know it's interesting that you, It sounds as if you are describing a little bit of an environment where you let people kind of – you had a rich array of things you had been interested in and then they came and came up with their own ideas about what to do and test.

BM: That's right.

HR: Many of those people have been very successful.

BM: That's right.

HR: And that seems to be – also maybe that is a little bit what Donald Hebb did with you.

BM: Yes, it is. It is. He gave me the environment.

HR: And that protection. Maybe that's something that actually fosters creativity and independence.

BM: It depends on the individual, I'm sure. Though, I must say that these people are very different from me, and from each other. They are very different people who have come.

HR: So when you look back, what is satisfying to you is – your achievements – ?

BM: Being here, being here. I think the breakthrough does please me.

HR: The breakthrough and then, certainly, probably your accomplishments in terms of your science.

BM: Oh, yes. Oh yes. Yes. Yes.

HR: And how much – you know, your relationships with others – how valuable has that been for you?

BM: It's very important. Oh yes. It is tremendously important.

HR: And that's been primarily here at the MNI – your students?

BM: Well, in psychology, right, I mean, you know

HR – and throughout the field?

BM: –at McGill University – and throughout the field. You know, I can't quite put up a bridge map like the surgeons can of where their people are. But I have people scattered quite a bit and I have friends, you know – I mean, I have built whole Italian thing, you know, because I started

learning Italian during the war. And then I started capturing Italians – I met a couple in Sperry's lab. But then, I would capture Italians, Italians would write and ask me, you know – somebody from the University of Bologna would like to come here. I would answer, "Why do you answer me in Italian?" I write back in Italian you see. I want to capture my Italians. [laugh] And so, I have lots of Italian friends. And I go to Italy and lecture in Italian, and so on. And so this is, you know – I try to combine the language with – the people I'm interested in are interested in the same things I am interested in, but I love changing languages and you know, so –.

HR: It is interesting in Italy in particular – I was in Boston and they had an Italian – they have a very rich behavioral neurology tradition – .

BM: Oh, yes. Oh, they do. They do. Some very good friends of mine, yes.

HR: In Germany, the neurologists don't work in the behavioral neurology field. They have the neuropsychologists.

BM: Yes. Yes. It is more like England.

HR: And in Spain, there is a behavioral neurology tradition.

BM: Yes.

HR: And so it's interesting how these different countries have different. You know – the people are all doing the same things, but different professionals align differently – [BM: That's right] asking the questions about patients anyway. Ok, what else do we have on our list? We are almost to the end. We are coming close to the end. Let's – .

BM: You had questions about the Academy –?

HR: – of course you were invited to be keynote speaker – ?

BM: I remember the beginnings of the Academy. And I remember – and now this has become very commonplace – but I think this society was an innovator in having these teaching sessions before – I don't know if you still have them – before the meeting. I have a couple of times – I went to Chicago once – been invited to give lectures before the American Academy meeting. As I say I think a lot of copy cats and things have sprung up. But I think this is excellent. I was impressed by that. But I don't normally go to Neurology meetings unless I'm invited because it is not my field. But when I was in Toronto, I hadn't been for years and years. And I was - well, it was very well run. I was impressed by that. It was very well run.

But the – what surprised me – well of course, it's happening more and more, I think – but it did surprise me there – I didn't realize there would be so many very young – well, of course, I am older but it is not because I am old because I go to different places and I notice when– a group of surprisingly young – and women – flocking to me. I am used to this happening at the Society for Neuroscience but I was not – I was quite surprised – I really didn't expect anybody – especially – you know, this symposium – it was a symposium on different topics – it wasn't a symposium, I guess, we were all keynote speakers – it was the President's symposium or something – but it was very good and it was very well run, and I was very interested in the other talks but they were not related one to another anymore than mine was, you know. And so, but then there were all these young women and they were flocking around me, like they do at the Society for Neuroscience, and I was sort of surprised at that. It was rather nice. It was very surprising. They seemed very young and very eager and very – I don't know. That surprised me.

HR: It is interesting in Neurology. I don't know too much about the demographics, but certainly in academic Neurology, there are not as many women as men. I think, in Neurosurgery, it would be even moreso. But there are probably more and more. I always am curious when you speak with medical students, how *few* of them want to go into neurology. I mean if you look at medical student class, you would think everybody would – I mean, if you have an interest in the brain – everybody should want to go into neurology.

BM: They all want to go into neuroscience.

HR: Maybe from the psychology side, but from medicine, not as many people go into neurology as you might expect, you know. Four or so in a year in the classes that graduate.

BM: I don't know, it seems to me that neuroscience is just growing at an incredible rate. Now this is not people who want to come and work with patients. [HR: No, no.] These people really want to do molecular stuff.

HR: That's certainly big too. And then if they are interested in the psychology side of things, neuroimaging has become the –

BM: Oh yes, but that's a tool.

HR: It's a tool, but it has become a way for people who are not seeing patients to be able to look at the brain.

BM: Yes, that's true.

HR: I think that's had a huge – promoted the field, really. So anything else you want to say? You know, I am just so impressed with how well you are doing.

BM: Well, did you expect? Did you think I'd be on a bed or something [laughing]?

HR: Oh, no! No, not at all. But I know I am not going to be in your shoes at your age.

BM: People are living longer now.

HR: That's true. That's true, but no, it's very impressive, and you must have an enjoyable life here in Montreal.

BM: I do enjoy life. Yes, I do enjoy it.

HR: Did you come to the office during the week?

BM: Well, I'm not retired!

HR: So you come every day.

BM: Well, you know, it it's going to be very icy, I won't. Or I'll get a cab or something. Or if I've got something I have to do, I don't feel compelled. I can do it at home. It's not that, because I live ten minutes away, you know. But I certainly, you know, put in a proper week's work. Yes, yes.

HR: Stay active, that's wonderful. And you should see her office too, because has a little old – little old stool is where she sits.

BM: Well, it has a back (the stool)!

BWS: Is there anything else you would like to add? A very open-ended question for you.

HR: We've covered a lot –.

BM: I think I've about given more than I thought I was going to talk about – [BWS: Oh, good] because I always wander off.

BWS: That's good. We sometimes ask if there was anything you would have done differently?

BM: Well, certainly I am not sorry I took my mathematics and tried it out because I had a wonderful time at Cambridge and I don't wish I had gone to Oxford and done languages. I have often thought about that. If I had gone in modern languages, I might have been dropped over Germany during the war, with, you know, speaking German, and come to a very sticky end. [laughs] I mean we were reserved – all scientists were in reserve professions in England during the war. We didn't go, we s – .

HR: What about going back to England? Did you spend much time back there?

BM: I did a sabbatical in 1970 because Oliver Zangwill wanted me to be back for a year at Cambridge. It was lovely socially, but I didn't like it work-wise. I guess, I came – this is really silly – I didn't have to have to do this – I came back to teach my three – that was five lectures, then, to the medical students.

HR: Oh, how dedicated of you!

BM: No, no. I was homesick. Homesick, for a more stimulating mental environment in some way. Cambridge was just lovely as far as my social life went. It was beautiful.

HR: You were very integrated here.

BM: I think that – I do think that North America – well, I'm not going to talk about Germany – I'm sure, I think this is probably a German influence in North America or something – I don't know – the English are little – you know the English are a bit lazy during m – I'm making these stupid remarks, but the English are really great in wartime or in crisis. They really are. They are very, very good. But when there is no crisis, which fortunately is most of the time, they are a bit laid back – a bit more laid back than I am – although I'm British.

HR: Passionate - less passionate?

BM: I don't know about that.

HR: You talked about how passionate you are.

BM: I don't know. What do you think?

HR: No, Absolutely.

BM: I can't judge. You can judge.

HR: No, you mentioned so many times you were passionate about pursuing this – or passionate about pursuing that – .

BM: – Well I do miss the soccer and the cricket.[laughing] That's true. I do miss the sports. That I do miss. I was really muttering about the one part of the Commonwealth we had to come and they didn't have cricket. That was tough.

HR: You have covered so much today. I hope that this documentation, you know, for people who are trying to understand – .

BM: Well, what happens to it now? What happens, I don't really know what this is all about, except that I felt flattered that you wanted me as a non-neurologist.

BWS: What happens now is that we will go back and make a full transcript of all of this. So you will see everything in writing as well. And then we'll send that to you and you can review just for accuracy's sake to make sure there are no problems and the spelling is correct.

BM: And if I said something that – I made the odd remark about individuals which is not relevant to my story really – ?

HR: You can change it. I might change a few things too, some of my errors [laughing]

BM: It is Jasper, not Jaspers. [HR: Right, right]

[break in interview, end track 1010, begin track 1011]

BWS: This again, is the Medical Research Council.[20]

[20] Dr. Milner was appointed a Career Investigator by the Medical Research Council of Canada in 1964.

BM: And they decided to carry on with the people that they had, and they finally closed the program in – when was it – in 1990. I think I had this from 1964-1990.

BWS: Was that part of the support that you received?

BM: That was my salary. Grants – I applied for in the usual way. This was the salary and then when that closed the Institute took me over, you know. But it was wonderful, that.

BWS: So that was quite a long period of time, actually, that you were – ?

BM: Yes, 34 years. And I didn't *lose* it – they closed the program. [BWS: Hmm?] I didn't *lose* it – they closed the program.

HR: It is interesting how, you know, without obviously, some economic support – you can't really keep going.

BM: Well, No. no, no.

HR: And that was an important resource for you – during that time.

BM: No, it was wonderful. This first one here to get it was Liam Wolfe – who died a while ago – who was a neurochemist that Dr. Penfield brought in from – Cambridge-educated South African neurochemist. And Neurochemistry was pretty new in those days. And he was the first person from the Neuro to get one of these. Then Rasmussen put me in for this and I got it. It was wonderful, in '64, I can remember that. That was really wonderful. [laughing]

BWS: So there was the gap between the 1950s to 1964?

BM: Well, that was difficult. I was on a small – a very small – well, first a year from Hebb and then this very small salary from Dr. Penfield from the Neuro. It was very insecure. Very, very insecure.

BWS: Look at the work that you were doing – during that time.

HR: That was like your very rich period too – even when you weren't getting much.

BM: That's right, that's right.

BWS: You accomplished a lot during that time.

BM: No, I had no financial security.

HR: – But you were willing to put up with it and continue to do work. [BM: Well, of course, of course, yes] You had to do that.

BM: Yes, yes. No I get very sa – well, this is, you asked me about philanthropy. But I , but I – you know, and the reason I wanted to give money to post-doctoral – is that I think this is the stage at which people have difficulties. I mean, it's not – maybe getting more difficult in other ways in the States. In Canada it's still not difficult to fund people, to get funding for graduate students, nor even for, yes, for students and then up to the Ph.D. But then as you get a post-doc, it's apt to be, you know, limited – to two years, three years – and that's when they get tempted to go into industry or do various things. And I'd rather they'd stay in academe if they can, but they are not going to get – you know it used to be when you finished your post-doc, you could walk into an assistant professorship somewhere, but this is not the case.

HR: It is *very* tough.

BM: And so, it's very tough. And so, this is what my foundation is going to be when I die. It's going to be for post-doctoral scholarships. From anywhere in the world too, I hate these

restrictive ones – they have to be Canadian or they have to be this or they have to be that. You know, competitive.

HR: We so appreciate your coming. Thank you.

BWS: Thank you.

[break in interview, end track 1011, begin track 1012]

BWS: Just, say that again.

BM: Well, as I recall it – as I recall it, Dr. Jasper was the only person who called Dr. Penfield – Wilder – you know – his first name. Even Dr. Rasmussen would refer to him as Dr. Penfield. And the rest of us called him "The Chief." He was known as The Chief.

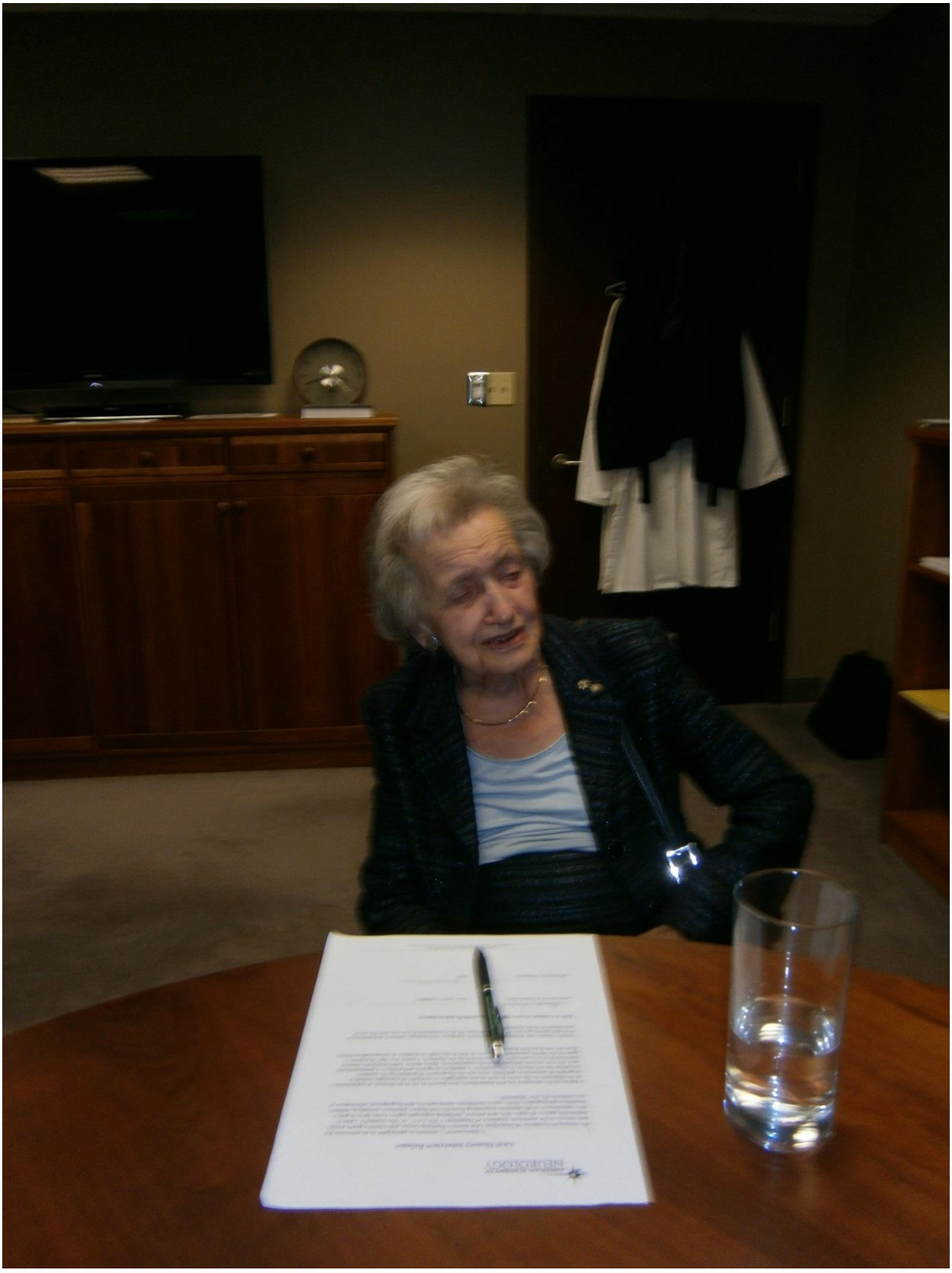
HR: He was – [BM: Yes, he was known as The Chief] he probably liked that.

BM: Yes, I think so. No, it's true. "Have you seen The Chief," is what we would say. Not, "Have you seen Dr. Penfield?" We would say, "Have you seen The Chief?"

End of Interview.
Total interview time: 173.42 minutes/2.9 hours

mi

L-R: Dr. Lesley Fellows – MNI, Dr. Brenda Milner – MNI and AAN interviewee, Dr. Heidi Roth – Department of Neurology, University of North Carolina School of Medicine and AAN interviewer



Dr. Brenda Milner during the interview

Dr. Brenda Milner in her office at the Montreal Neurological Institute

CURRICULUM VITAE

Brenda Milner (née Langford)

Born:	July 15, 1918
Place of Birth:	Manchester, England
Citizenship:	British & Canadian
Languages:	English, French & Italian

PRESENT POSITION:

Dorothy J. Killam Professor of Psychology, Montreal Neurological Institute and Professor, Department of Neurology and Neurosurgery, McGill University.

EARNED DEGREES:

B.A.	1939	University of Cambridge, Experimental Psychology Class: 1*
M.A.	1949	University of Cambridge, Experimental Psychology
Ph.D.	1952	McGill University, Physiological Psychology Thesis title: “Intellectual effects of temporal-lobe damage in man." Thesis adviser: D.O. Hebb
Sc.D.	1972	University of Cambridge, Experimental Psychology

HONORARY DEGREES:

LL.D.	1980	Queen's University
D.Sc.	1982	University of Manitoba
D.Sc.	1986	University of Lethbridge
D.Sc.	1986	Mount Holyoke College
D.Sc.Soc.	1987	Laval University
D.Sc.	1987	University of Toronto
D.Hum.L.	1988	Mount Saint Vincent University
D.	1988	Université de Montréal
D.Sc.	1991	McGill University
D.Sc.	1991	Wesleyan University
D.Sc.	1991	Acadia University
D.Sc.	1992	University of St. Andrews
D.Sc.	1997	University of Hartford
D. Sc.	1999	McMaster University
L.L.D.	2000	University of Cambridge
D.Sc.	2002	Memorial University of Newfoundland
D.Sc.	2002	Columbia University
D.Ps.	2002	Second University of Naples
D.U.	2004	University of Ottawa
D.Sc.	2008	Ryerson University

D.Sci.Hum. 2010 l'Université du Québec à Montréal
D. 2010 l'Université du Québec en Outaouais

ACADEMIC APPOINTMENTS:

1944-1952	Professeur agrégé, Université de Montréal Institut de psychologie
1952-1953	Research Associate, McGill University Psychology Department
1953-1960	Lecturer
1960-1964	Assistant Professor
1964-1970	Associate Professor
1970-	Professor, McGill University Department of Neurology & Neurosurgery

RESEARCH EXPERIENCE:

1939-1941	Sarah Smithson Research Student Newnham College, Cambridge Perceptual research in the Psychological Laboratory under F.C. Bartlett and K.J.W. Craik
1941-1944	Experimental Officer to U.K. Ministry of Supply Radar research: Problems of display and control
1950-	Montreal Neurological Institute Research on behavioural effects of human brain lesions In collaboration with Drs. Wilder Penfield, Theodore Rasmussen, William Feindel, André Olivier and associates.

MEMBERSHIP IN LEARNED SOCIETIES:

Fellow	American Association for the Advancement of Science American Psychological Association (Divisions 3 & 6) American Psychological Society Canadian Psychological Association Society of Experimental Psychologists, USA
Member	Academy of Aphasia American Academy of Neurology American Epilepsy Society American Neurological Association Association de Psychologie Scientifique de Langue Française British Society of Experimental Psychology Eastern Psychological Association European Brain and Behaviour Society International Brain Research Organization International Neuropsychology Symposium

MEMBERSHIP IN LEARNED SOCIETIES (cont.):

	Psychonomic Society
	Society for Neuroscience
Associate	Association for Research in Nervous and Mental Diseases
Affiliate	Royal Society of Medicine

MEMBERSHIP IN PROFESSIONAL ASSOCIATION:

Member of Corporation of Psychologists of the Province of Quebec - Licence #01801-71

HONOURS & AWARDS:

1964-2000	Career Investigator Medical Research Council of Canada
1971	Kathleen Stott Prize for Medical & Scientific Research Newnham College, Cambridge
1971-1972	Clothworkers Fellowship for Scientific Research Girton College, Cambridge
1972-1974	President, Division 6, Physiological & Comparative Psychology American Psychological Association
1973	Distinguished Scientific Contribution Award of the American Psychological Association
1974	Lennox Lecturer American Epilepsy Society
1975	Distinguished Scientist Address Eastern Psychological Association
1975	Fellow, Royal Society of Canada
1976	Foreign Associate National Academy of Science, USA
1977-1978	Chairman, Psychology Section American Association for the Advancement of Science
1977	Invited Plenary Address European Brain and Behaviour Society, Brussels
1978-1980	National Sigma Xi Lecturer, USA

HONOURS & AWARDS (cont.)

1979	Fellow, Royal Society of London
1979	Karl Spencer Lashley Award American Philosophical Society
1979	Hughlings Jackson Lecturer Montreal Neurological Institute
1981	Canadian Psychological Association Award for Distinguished Contributions to Psychology as a Science
1981	Donald D. Matson Lecturer, Harvard Medical School "Cognitive function and frontal lobes"
1981	2nd Delos D. Wickens Lecturer, Ohio State University. "Brain and human memory: Some evidence for separate systems"
1982	Plenary speaker, 1st World Congress of the International Brain Research Organization Lausanne, Switzerland. "Mechanisms of memory"
1983	Izaak Walton Killam Memorial Prize The Canada Council
1983	11th Sir Frederic Bartlett Lecturer Experimental Psychology Society, U.K.
1983	CIL Inc. Distinguished Visiting Lectureship in the Sciences Mount Saint Vincent University, Halifax, Nova Scotia
1984	Penfield Award, Canadian League Against Epilepsy
1984	Officer of the Order of Canada
1984	Hermann Von Helmholtz Prize Cognitive Neuroscience Institute, USA
1985	Grand Dame of Merit of the Sovereign Military Order of Saint John of Jerusalem, Knights/Dames of Malta
1985	Mérite annuel l985 Corporation professionnelle des psychologues du Québec
1985	'Femme de l'Année en Sciences', Québec For distinguished scientific achievements

HONOURS & AWARDS (cont.)

1985	Officier de l'Ordre national du Québec
1985	Falconbridge Lecturer Laurentian University, Sudbury, Ontario
1986	W.C.D. Pacey Lecturer University of New Brunswick
1986	Honorary Life Member The New York Academy of Sciences, USA
1987	Tenth Annual Ralph W. Gerard Prize Society for Neuroscience, USA
1987	L'Académie des Grands Montréalais: Grande Montréalaise l987
1987	Killam Lecturer, Dalhousie University Halifax, Nova Scotia
1987	Abe Black Memorial Lecture McMaster University, Hamilton, Ontario
1988	Fellow, Society of Experimental Psychologists, USA
1989	William James Fellow, American Psychological Society
1989	Humphry Davy Lecturer of the Royal Society Académie des Sciences, Paris, France
1989	Honorary Fellow, Newnham College, Cambridge, U.K.
1992	Corresponding Member, Swiss League against Epilepsy
1992	History of Neuroscience Lecture Society for Neuroscience, Anaheim, California, USA
1993	Dorothy J, Killam Professor Montreal Neurological Institute
1993	First recipient of the Wilder Penfield Prize for Biomedical Research from the Province of Quebec
1994	Eighth Annual Rita G. Rudel/Lucy G. Moses Lecturer Columbia University, USA “Some thoughts on memory and the human brain”

HONOURS & AWARDS (cont.)

1994	Honorary Guest Speaker, 5ième Festival International du Film Scientifique du Québec
1994	Distinguished Neuropsychologist Award National Academy of Neuropsychology
1995	McLaughlin Medal Royal Society of Canada
1995	Neural Plasticity Prize, Fondation IPSEN, Paris (shared with J. Mehler and M. Mishkin)
1995	Grass Lecturer, Society for Neuroscience, USA "Hemispheric specialization, memory, and the human brain: Old and new perspectives"
1995	Gordon G. Lennox Award, American Epilepsy Society
1997	Metropolitan Life Foundation, Senior Award for Medical Research
1997	Elected to Canadian Medical Hall of Fame
1997	Quad-L Lecture, Department of Psychology University of New Mexico, Albuquerque, USA "Memory and the human brain: Perspectives on the past and future"
1999	Honorary Member, European Brain and Behaviour Society
2001	John P. McGovern Award in the Behavioural Sciences American Association for the Advancement of Science
2001	D.O. Hebb Award Canadian Society for Brain Behaviour and Cognitive Science
2001	YWCA Woman of Distinction in Science and Technology
2002	Golden Jubilee Medal Her Majesty Queen Elizabeth II
2003	Michel Sarrazin Award Club de recherches cliniques du Quebec
2004	National Academy of Sciences Award in the Neurosciences
2004	Promotion to Companion of the Order of Canada

HONOURS & AWARDS (cont.)

2005	Foreign Member, American Academy of Arts and Sciences
2005	Gairdner Foundation International Award in Health Research
2005	Penfield Award, Canadian League Against Epilepsy
2005	Honorary Member, Golden Key International Honour Society
2009	Award of Excellence, Natural Sciences and Engineering Research Council of Canada
2009	Promotion to Grande Officière de l'Ordre national du Québec
2009	Patricia Goldman-Rakic NARSAD Award in Cognitive Neuroscience
2009	Balzan Foundation Award in Cognitive Neuroscience
2010	Norman Anderson Lifetime Achievement Award, Society of Experimental Psychologists
2010	Award of Excellence, Natural Sciences and Engineering Research Council of Canada

ADMINISTRATIVE POSITIONS HELD:

1969-1976	Member, Executive Committee International Neuropsychology Symposium
1973-1994	Editorial Board, Neuropsychologia
1974-1979	Member, Grants Committee for Behavioural Sciences Medical Research Council of Canada (Chairman 1976-1979)
1980-1988	Editorial Board, Behavioral Brain Research
1980-1985	Tenure Committee, McGill University
1982-1984	Member, Board of Scientific Counselors National Institute of Mental Health, USA
1984-1988	Member, Standing Committee on Ethics in Experimentation Medical Research Council of Canada
1984-1989	Overseas Consultant, British Journal of Psychology

ADMINISTRATIVE POSITIONS HELD (cont.)

1985-1992	Member, Advisory Board Montreal Neurological Institute
1985-1992	Representative-at-large, Central Council of IBRO
1986-1989	Member, Committee on Scientific Awards American Psychological Association
1988-1991	Council Delegate of Electorate J (Psychology) American Association for the Advancement of Science
1988-1989	Member, International Peer Review Committee Networks of Centres of Excellence, Government of Canada
1989- 1996	Editorial Board, Hippocampus
1990-	Member, International Advisory Board, NeuroReport
1990-2001	Member, Promotions Committee Department of Neurology & Neurosurgery McGill University
1990-	Editorial Board Glutamate, Mort Cellulaire et Memoire
1991- 1996	Consulting Editor, Neuropsychology American Psychological Association
1992-	Member, PET Working Committee Montreal Neurological Institute
1993-1994	Honorary President, Canadian Psychological Association
1993-1997	Executive Secretary of IBRO International Brain Research Organization

TALKS 1990-present:

October 6, 1990	Keynote Speaker 6th P.U.N.Y./N.U.N.Y. Conference Colgate University, NY, USA **Frontal and temporal lobe contributions to human memory**

TALKS 1990-present (cont.):

October 26, 1990 — Invited Discussant
Memory Disorders Research Society
Boston, MA, USA
Theories of memory and amnesia

November 26, 1990 — Journée de réflexion sur la recherche universitaire du Québec
Le Ministre de l'Enseignement Supérieur et de la Science
Gouvernement du Québec
Témoignage

January 10, 1991 — Invited Speaker
Neurobiology Seminar Series
University of California
San Francisco, CA, USA
Temporal lobes and memory

January 20-22, 1991 — Presenter
First Annual Meeting of the McDonnell-Pew Program in Cognitive Neuroscience
San Diego, CA, USA
Progress report on the work of the MNI McDonnell-Pew Cognitive Neuroscience Center

March 20, 1991 — Special Guest Speaker
Frontal Lobes: Theoretical and Clinical Perspectives
Baycrest Centre for Geriatric Care, in association with the Department of Neurology, UCLA School of Medicine
Toronto, ON, Canada
The challenges of the future in frontal-lobe research

April 4, 1991 — Invited Speaker McDonnell-Pew Cognitive Neuroscience Colloquium Series
Massachusetts Institute of Technology
Cambridge, MA, USA
Temporal lobes and human memory

April 18, 1991 — Invited Speaker
Minicolloque: Mémoire et comportement:
Concepts et méthodes d'analyse
La Société des Neurosciences
Université Louis Pasteur
Strasbourg, France
Neuropsychologie de la mémoire: Histoire des concepts

August 5, 1991 — Invited Speaker
Workshop: Temporal lobe mechanisms of memory:

TALKS 1990-present (cont.):

From neuron to behavior in primates
Third IBRO World Congress of Neuroscience
Montreal, QC, Canada
Hippocampal-neocortical interaction in memory processes

October 2, 1991 Invited Speaker
3rd International School of Neuroscience
Praglia Abbey, Italy
Cognitive effects of frontal-lobe lesions in man

October 9, 1991 Invited Speaker
Institute of Neurophysiology
University of Verona
Verona, Italy
Recent studies of human temporal-lobe function.
(In Italian)

November 19, 1991 Invited Speaker
Journée de neuropsychologie Jean-Louis Signoret
Pitié - Salpétrière
Paris, France
H.M., Jean-Louis, et la recherche sur la mémoire

January 10-13, 1992 Presenter
Second Annual Meeting of the McDonnell-Pew
Program in Cognitive Neuroscience
Biscayne, FL, USA
Progress report on the work of the MNI
McDonnell-Pew Cognitive Neuroscience Center

April 30, 1992 Discussant
The Hebb Synapse: Cells, Circuits, and Concepts
Dalhousie University
Halifax, NS, Canada
The Hebb legacy: A personal perspective

October 15, 1992 Invited Address
Journées scientifiques du secteur de neuropsychologie
Département de psychologie
Université de Montréal
Montreal, QC, Canada
Une histoire montréalaise de la neuropsychologie

October 27, 1992 History of Neuroscience Lecture
Society for Neuroscience
22nd Annual Meeting

TALKS 1990-present (cont.):

	Anaheim, CA, USA **Neuropsychology: Whence and whither?**
March 25-26, 1993	Keynote Speaker The 8th Annual Red RiverValley Psychology Conference Moorehead State University Moorehead, MN, USA **Memory and the temporal lobes of the human brain**
April 2, 1993	Invited Address Centre de Recherche en Science Neurologiques Université de Montréal Montreal, QC, Canada **Aspects of human frontal-lobe function**
April 15-17, 1993	Invited Address FESN Study Group 1993: Evolution and Neurology of Language Geneva, Switzerland **Carotid-Amytal studies of speech lateralization and gesture control**
May 5, 1993	Invited Address State University of New York Health Science Center Brooklyn, NY, USA **Reflections on memory and the human brain**
July 14-17, 1993	Invited Address St. Vincent in Valle d'Aosta Italy Workshop on Epilepsy and the Functional Anatomy of the Frontal Lobe **Behavioural effects of frontal-lobe lesions in man**
August 1-6, 1993	Invited Speaker International Congress of Physiological Sciences Glasgow, Scotland Organizer of Session I: Disorders of learning and memory Sponsored by IBRO **Frontal lobe contributions to memory processes**
August 20, 1993	Invited Speaker American Psychological Association Science Weekend **Hebb's impact: Past and present**
April 20, 1994	Invited Address Yale University

TALKS 1990-present (cont.):

	Department of Psychology **Memory and the human frontal cortex**
April 29, 1994	Invited Speaker Université Laval Senior Women Academic Administrators of Canada La Table Ronde **A la recherche du cerveau**
May 10, 1994	Keynote Speaker Columbia University Department of Neurology Eighth Annual Rita G. Rudel/Lucy G. Moses Lecture **Some thoughts on memory and the human brain**
June 3, 1994	Invited Speaker Memorial Observance for Roger W. Sperry California Institute of Technology **Eulogy for Roger W. Sperry in Engineering & Science, Vol. LVII, pp. 33-34, Summer 1994**
August 26, 1994	Invited Speaker American Psychological Association Tribute to Roger Sperry Los Angeles, CA, USA **Hemispheric specialization in learning and memory**
Sept 22-23, 1994	Honorary Guest Speaker 5ième Festival International du Film Scientifique du Québec Montreal and Quebec City **Invited address on the film: Memory: The Weaver of Life**
November 4, 1994	Keynote Speaker National Academy of Neuropsychology Orlando, FL, USA **Some thoughts on memory and the human brain**
March 22, 1995	Georgetown University Washington, D.C., USA Series: Remains of the Day: A psychology for the 21st century **Lecture 1: Some thoughts on memory and the human brain** **Lecture 2: Frontal lobes**
May 5-6, 1995	International Meeting on Leonardo Bianchi and the Functions of the Frontal Lobes Naples, Italy

TALKS 1990-present (cont.):

	President of Congress and Invited Speaker **Cognitive functions and frontal lobes**
May 23, 1995	Invited Address Research Symposium in Honour of Dean Richard L. Cruess Retiring Dean of Medicine McGill University **Neural substrates of word generation in bilingual subjects**
May 27-28, 1995	Invited Speaker 1st International Kyoto Life Science Symposium on Learning and Memory Kyoto, Japan **Hemispheric-specialization effects in human memory**
June 6, 1995	Invited Address Massachusetts Neuropsychological Society Fourth Annual Scientific Symposium Cambridge, MA, USA **History and evolution of neuropsychology**
July 15-16, 1995	Invited Address Satellite Symposium of Fourth IBRO World Congress of Neuroscience: Functions and Clinical Relevance of the Hippocampus Kyoto, Japan **Plenary lecture: Hippocampus and memory: Then and now**
Nov 11-16, 1995	Grass Lecturer Society for Neuroscience 25th Annual Meeting San Diego, CA, USA **Hemispheric specialization, memory, and the human brain: Old and new perspectives**
January 9, 1996	Invited Speaker University of Toronto Annual Undergraduate Medical Student Research Day **Brain and behaviour: Yesterday and today**
March 14, 1996	Invited Speaker Colloquium Series Vanderbilt University Department of Psychology Nashville, TN, USA **Hemispheric specialization and the human brain: Old and new perspectives**

TALKS 1990-present (cont.):

March 27-28, 1996	Closing remarks Discussion Meeting of the Royal Society London, England **Executive and cognitive functions of the prefrontal cortex**
March 19-20, 1997	Invited Speaker Discussion Meeting of the Royal Society**:** What are the Parietal and Hippocampal Contributions to Spatial Cognition? London, England **Right medial temporal-lobe contribution to object-location memory**
April 25, 1997	Invited Speaker Colloquium University of Rochester Rochester, NY, USA **Cortical activation studies of memory and language:** **Convergence with findings from brain lesions**
May 30, 1997	Keynote Speaker The Canadian Medical Hall of Fame Canada Trust Youth Symposium A Celebration of Excellence London, ON, Canada **The brain and human memory**
December 5, 1997	Quad-L Lecture, Department of Psychology University of New Mexico Albuquerque, USA **Memory and the human brain: Perspectives on the past and future**
March 17, 1998	McDonnell-Pew Lecture Department of Experimental Psychology University of Oxford **Probing the brain for memories**
March 20, 1998	Litchfield Lecture Department of Clinical Neurology Radcliffe Infirmary University of Oxford **Functional neuroimaging of language: Observations from** **the study of normal volunteers and presurgical patients**
March 25, 1998	Guest of honour Course on Neuropsychology of the Epilepsies Gargnano, Italy

TALKS 1990-present (cont.):

	Past, present and future of the neuropsychology of epilepsy
June 8, 1998	Talairach Lecture 4th International Conference on Functional Mapping of the Human Brain Montreal, QC, Canada **Reflections on memory and the human brain**
June 18, 1998	Invited Speaker Memoria e Memorie Fra Umanismo e Scienza Rome, Italy **Lobi temporali e memoria**
March 26, 1999	Invited Speaker Morrell Symposium on Brain Plasticity and Epilepsy Marine Biological Laboratory, Woods Hole, MA, USA **Normal processes of learning and memory**
June 2, 1999	Invited Address Psychology Department McMaster University Hamilton, ON, Canada **Right temporal-lobe contribution to object-location memory**
October 2, 1999	Invited Plenary Speaker European Brain and Behaviour Society Rome, Italy **Functional imaging of language:** **Data from normal volunteers and presurgical patients**
May 10, 2000	Harold Schlosberg Lecture Brown University Providence, RI, USA **Temporal-lobe contribution to memory processes**
July 12, 2000	Invited Address Psychological Laboratory University of Cambridge Cambridge, England **Functional imaging of language**
February 17, 2001	John P. McGovern Lecture American Association for the Advancement of Science San Francisco, CA, USA **Memory research: Past and future**
June 24, 2001	D.O. Hebb Lecture

TALKS 1990-present (cont.):

Canadian Society for Brain, Behaviour and Cognitive Science
Quebec City, QC, Canada
Memory and the right temporal lobe:
From patients to imaging

October 12, 2001 Invited Address, 41st Annual Meeting
Society for Psychophysiological Research
Montreal, QC, Canada
Neuroimaging studies of language

May 16, 2002 Carl Duncan Lecture
Northwestern University
Evanston, IL, USA
Human memory and the medial temporal region

October 3, 2002 Convocation Address
Second University of Naples
Naples, Italy
Lobi temporali e memoria

April 3, 2003 Repole Memorial Lecture
University of Vermont
Burlington, VT, USA
Memory and the temporal lobes revisited

April 11, 2003 Inaugural Lecture
Canadian Centre for Behavioural Neuroscience
University of Lethbridge
Lethbridge, AB, Canada
Temporal lobes and memory revisited

August 7, 2003 Division 40 Invited address
Annual Meeting of the American Psychological Association
Toronto, ON, Canada
Temporal lobes and memory revisited

April 16, 2004 Symposium Speaker
Celebrating a Century of Hebb: A 100-Year Legacy
McGill Psychology Department
Hebb's years at the MNI

May 19, 2004 Keynote Speaker
Canadian Medical Hall of Fame Discovery Day in the Health Sciences
Queen's University, Kingston, ON
Memory and the temporal lobes of the human brain

TALKS 1990-present (cont.):

June 6, 2004 — Convocation address in the Health Sciences
University of Ottawa
Ottawa, ON

June 26, 2004 — Symposium Speaker
Donald O. Hebb Seminar
Annual meeting of the International Society for the History of Neuroscience
Montreal, QC
Hebb and neuropsychology at the MNI

July 20, 2004 — Invited Speaker
Ground Rounds
Mount Sinai School of Medicine
New York, NY
The hippocampal amnesic syndrome

October 24, 2004 — Invited Speaker
Neurons and Memory
Neuron Satellite Meeting, Society for Neuroscience
San Diego, CA
Temporal lobes and memory revisited

October 25, 2004 — Symposium Speaker
D.O Hebb's Legacy
Annual Meeting, Society for Neuroscience
San Diego, CA
Hebb and neuropsychology at the MNI

November 17, 2004 — Invited Speaker
Frontiers in Research Lectures on Neurobiology, Learning and Cognition
University of Ottawa
Ottawa, Ontario
Memory and the temporal lobes
(bilingual presentation)

March 5, 2005 — Invited Speaker
History of Neuroscience Course
Center for the Neurobiology of Learning and Memory
University of California
Irvine, CA
A personal perspective on memory, learning and the brain

May 19, 2005 — Inaugural Lecturer
Governor General's Lecture Series for the Royal Society of Canada
Queen's University

TALKS 1990-present (cont.):

Kingston, ON
The many faces of memory

May 26, 2005 Inaugural Lecturer
Governor General's Lecture Series for the Royal Society of Canada
University of Manitoba,
Winnipeg, MB
The many faces of memory

June 3, 2005 Keynote Address
Inaugural Symposium, The neural bases of language: From evolution to treatment
Centre for Research on Language, Mind and Brain
McGill University
Montreal, QC
Hemispheric specialization and memory revisited

October 5, 2005 Inaugural Lecturer
Governor General's Lecture Series for the Royal Society of Canada
University of New Brunswick
Fredericton, NB
The many faces of memory

October 24, 2005 Gairdner Memory Symposium 2005
Montreal Neurological Institute
McGill University
Shared memories: The problem of hemispheric interaction

October 25, 2005 Gairdner Memory Symposium 2005
University of Ottawa
Shared memories: The problem of hemispheric interaction

October 27, 2005 Gairdner 2005 International Symposium
University of Toronto, ON
Early clues to the cerebral organization of memory

December 2, 2005 Symposium Speaker
Looking Back, Looking Forward: Shaping Neuroscience and Cognitive Science Symposium, Department of Brain and Cognitive Sciences
Massachusetts Institute of Technology,
Cambridge, MA, USA
The first prong: Brain and behavior, then and now

December 14, 2005 Inaugural Lecturer
Governor General's Lecture Series for the Royal Society of Canada
Université Laval

TALKS 1990-present (cont.):

	Québec City, QC **Les nombreuses facettes de la mémoire**
April 12, 2006	4th Annual Clarence and Beverly Chandran Distinguished Lecturer Duke University Raleigh, NC **The many faces of memory**
September 9, 2006	Keynote Speaker 20th Annual Retreat, Wheeling, WV, USA Center for Neuroscience University of Pittsburgh Pittsburgh, PA **Hemispheric specialization and memory revisited**
October 3, 2006	Frank Morrell Memorial Lecturer Department of Neurological Science Rush University Medical Center Chicago, IL, USA **The many faces of memory**
November 7, 2006	Keynote Speaker 8th Annual Neuroscience Research Day Centre for Neuroscience University of Alberta Edmonton, AB **The many faces of memory**
May 3, 2007	Featured Speaker Applause 2007 McGill University **Reflections on the field of brain and memory**
May 15, 2007	Closing Address 29th International Symposium (The Essence of Memory) GRSNC/CRSN University of Montreal **Reflecting on memory**
May 25, 2007	Plenary Lecture: First Annual Meeting Canadian Association for Neuroscience Toronto, ON **Reflections on the field of brain and memory**
October 29, 2007	Neuropsychology Symposium to honour Laughlin B. Taylor Montreal Neurological Institute, McGill University

TALKS 1990-present (cont.):

Montreal, QC
Remembering Laughlin: Highlights from a 40-year history

November 5, 2007 Guest Speaker
40th Anniversary Symposium
L'Institut de recherches cliniques de Montréal
Reflections on the field of brain and memory

February 16, 2008 John Kershman Lecturer
Eastern EEG Society
Saint Sauveur, QC
Reflecting on memory

April 18, 2008 23rd Annual D.O. Hebb Memorial Lecture
Dalhousie University
Halifax, NS
My life in cognitive neuroscience

May 21, 2008 Keynote Speaker
Toronto Western Research Institute Research Day
Toronto, ON
Hemispheric specialization and memory revisited

Nov. 18, 2008 History of Neuroscience Lecture
Society for Neuroscience
Washington, DC, USA
Reflections on the field of brain and memory

Dec 12, 2008 Invited Lecturer
MUHC Grand Rounds, Dept. of Psychiatry
Alan Memorial Instiute, McGill University
Reflections on the field of brain and memory

May 28, 2009 Raviola Memorial Lecture
Dept. of Anatomy and Neurobiology
Boston University School of Medicine
Reflections on the field of brain and memory

June 3, 2009 Gairdner Symposium Speaker
University of Ottawa
Boston University School of Medicine
Reflections on the field of brain and memory

June 9, 2009 23rd Rita G. Rudel/Lucy G. Moses Lecture
Columbia University Medical Center
Reflections on the field of brain and memory

TALKS 1990-present (cont.):

Sept. 24, 2009 — Keynote Speaker – Memory Disorders Research Group – Duke University
Memory and memories: A tribute to HM

Jan. 15, 2010 — Invited Lecturer
Centre de recherche en neuropsychologie et cognition, Université de Montréal
Reflections on the field of brain and memory

Apr. 15, 2010 — Invited Lecturer
American Academy of Neurology 62nd Annual Meeting
Plenary Session, Frontiers of Neuroscience, Toronto
Reflections on Memory: What neurosurgical patients have taught us

May 21, 2010 — Annual Lecture in Psychology
University of Calgary
Reflecting on the field of brain and memory

Jul. 23, 2010 — Goldman-Rakic/NARSAD Distinguished Lecture
Yale University School of Medicine,
New Haven, CT
Reflecting on the field of brain and memory

May 1, 2010 — Invited Lecturer
Meeting of the Society of Experimental Psychologists
Philadelphia, PA
My life in Science: The excitement of discovery

Oct. 22, 2010 — Trafalgar Ross Lecturer
Trafalgar School for Girls
Montreal, QC
My life in Science: The excitement of discovery

Nov. 18, 2010 — Public Lecture
Concordia University Science College, QC
Brain and memory: Lessons from neurosurgical Patients

PUBLICATIONS: Articles in refereed Journals

MILNER, BRENDA. Intellectual function of the temporal lobes. Psychological Bulletin. 51, 42-62, 1954.

MILNER, B. and PENFIELD, W. The effect of hippocampal lesions on recent memory. Transactions of the American Neurological Association, 80, 42-48, 1955.

PUBLICATIONS: Articles in refereed journals (cont.)

SCOVILLE, W.B. and MILNER, B. Loss of recent memory after bilateral hippocampal lesions. J. Neurol. Neurosurg. Psychiat., 20, 11-21, 1957.

PENFIELD, WILDER and MILNER, BRENDA. Memory deficit produced by bilateral lesions in the hippocampal zone. A.M.A. Arch. Neurol. Psychiat., 79, 475-497, 1958.

ORBACH, J., MILNER, B. and RASMUSSEN, T. Learning and retention in monkeys after amygdala-hippocampus resection. Arch. Neurol., 3, 230-251, 1960.

MILNER, B., BRANCH, C. and RASMUSSEN, T. Study of short-term memory after intracarotid injection of sodium Amytal. Transactions of the American Neurological Association, 87, 224-226-, 1962.

MILNER, BRENDA. Effects of different brain lesions on card sorting. Arch. Neurol., 9, 90-100, 1963.

BRANCH, C., MILNER, B., and RASMUSSEN, T. Intracarotid Sodium Amytal for the lateralization of cerebral speech dominance. J. Neurosurg., 21, 399-405, 1964.

MILNER, BRENDA. Visually-guided maze learning in man: Effects of bilateral hippocampal, bilateral frontal, and unilateral cerebral lesions. Neuropsychologia, 3, 317-338, 1965.

MILNER, BRENDA. Disorders of memory after brain lesions in man. PREFACE: Material-specific and generalized memory loss. Neuropsychologia, 6, 175-179, 1968.

MILNER, B. Visual recognition and recall after right temporal-lobe excision in man. Neuropsychologia, 6, 191-209, 1968.

MILNER, B., CORKIN, S. and TEUBER, H-L. Further analysis of the hippocampal amnesic syndrome: l4-year follow-up of H.M. Neuropsychologia, 6, 215-234, l968.

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