Brain Science Podcast Episode 49
An Interview with Pioneering Neuroscientist: Brenda Milner

Transcribed by Lori Wolfson
All errors or omissions responsibility of the transcriber

(Music)

INTRODUCTION

*This is the **Brain Science Podcast** - the podcast for everyone who has a brain - and I'm your host, Dr. Ginger Campbell. On the **Brain Science Podcast** we explore how recent discoveries in neuroscience are unraveling the mysteries of how our brains make us who we are. For more information including Show Notes, links to previous episodes, and information about how to subscribe, please go to the website brainsciencepodcast.com. We also have a Discussion Forum at brainscienceforum.com and you can send me email at docartemis@gmail.com.*

(Music)

This is Episode 49 of the ***Brain Science Podcast***. Today I have a very special guest, pioneering neuroscientist, Dr. Brenda Milner. I have talked about Dr. Milner's work several times on this podcast. Her work has helped to uncover many surprising facts about memory, including the fact that procedural memory is separate from episodic memory. Dr. Milner recently celebrated her 90th birthday, and she is still very active at the Montreal Neurological Institute where she has been on the faculty for over 50 years.

She was recently interviewed by Dr. Marc Pelletier on *Futures in Biotech*, but she agreed to come on **The Brain Science Podcast** to do a follow-up interview. There is some overlap in the content between my interview and Marc's. I strongly encourage you to listen to his interview also. Dr. Milner shares a lot more of her personal story in that interview. It is Episode 33 of *Futures in Biotech*. There will be a link in the Show Notes for his interview. You'll be able to find that at brainsciencepodcast.com.

I want to get right into the interview, but I wanted to mention a couple of things. First I want to thank everyone who is helping to support my work. For those of you who have wanted an alternative to PayPal, you can now send contributions directly to the new mailbox I have set up. The address for this is on the website.

Also, I will be in Washington, D.C. from November 14th through 19th of 2008, for the Society for Neuroscience meeting. If you are going to be there please drop me an email at docartemis@gmail.com so that we can get together. I will be

putting together a special episode about the conference. So, if you are a neuroscientist and would like to tell me and my listeners about your work, be sure to contact me.

Now, let's get on in to Dr. Milner's interview.

(Music)

INTERVIEW

GC: Dr. Milner, I really enjoyed listening to your talk with Marc Pelletier on *Futures in Biotech*. We've talked a lot about your work on this show, so it's a great honor to have you as a guest on the **Brain Science Podcast.**

BM: Well, it's a pleasure to be here.

GC: Thank you. One of the things that I really enjoyed about your interview with Marc was the way that you put your work into historical context. Now, when you first became an experimental psychologist was behaviorism dominant, or was I wrong about that?

BM: Well, you were both wrong and right, because where I was it certainly was not dominant. I was in Cambridge, England at Cambridge University in a strong department of experimental psychology chaired by Professor Bartlett, who was quite famous for his work on remembering.

Well, again, it's what you mean by behaviorism. I should go back to that, because there are two ways of thinking of that expression. I think you're probably using it as a school in psychology where you're emphasizing stimulus and response, and ignoring the state of the mind of the organism that's receiving the stimuli and responding to them. I suppose the prototypical example of that would be Skinnerian psychology. I've certainly, I would say, always been protected from that because that has never been anything that was popular in the departments in which I was working.

And the precursors of that—Clark Hull, and so on; complicated stimulus-response sort of psychology theories—in Cambridge they were somewhat frowned upon, because they took a more biological approach, always, even though we couldn't be looking into the brain at that point. Always the idea that you have to keep in mind what the organism brings to any situation, and stimuli from outside never impinge upon a sort of tabula rasa, or bounce off a person; they impinge on a highly active state of mind, or state of brain, and modify it. But are modified by it, also.

So, behaviorism, in that sort of theoretical sense, was never the school to which I was exposed. And later when I was with Don Hebb in Montreal, of course Hebb, again, was very opposed to Skinner and Skinnerian psychology, and was very much emphasizing what the individual brought to the situation, and the fact that the brain is constantly active, and whatever comes in from outside can only modify what is ongoing activity. So, that was what I was brought up in.

However, if you think of behaviorism in a totally different way, then I suppose I'm a behaviorist in the sense that I'm a behavioral neuroscientist; that what I'm studying and measuring and exploring is in fact behavior. I'm not particularly interested in working on consciousness, and so on. It's very fashionable nowadays.

In all of this work on memory, and the tasks, and so on we are actually measuring and studying behavior in a controlled experimental way. Whereas when we are just mixing with our friends and chatting we are observing behavior and responding to it, but not in a scientific way. But when you're doing a psychology experiment, then you have things designed in a controlled way so that you plan to measure certain outcomes and measure them.

Although we do call our department here cognitive neuroscience, I always think of myself as a behavioral neuroscientist. Nowadays people tend to use that term just to describe work with non-human species, but I think that's narrowing it unnecessarily. And that's what I consider myself to be. But that is not the theoretical school of behaviorism. So, I'm not a theoretical behaviorist, but my methods are focused in the scientific study of behavior and trying to relate that to activity in the brain. Does that answer your question?

GC: Yes, very much. And it really does help. I went to college in the mid 70's and I took one psychology course and I hated it terribly, because basically we went into this room and there were these red lights that came on when they were going to give us these stupid quizzes. I was so turned off to psychology that I don't think I looked at it again for years. That, I think, was too much behaviorism of the Skinner type.

BM: I don't know. It doesn't sound familiar to me at all.

GC: But it's a good reminder that outside of the United States Skinner's influence probably wasn't as much as we might have thought. But you did mention doing experiments. And I remember that you said when you were talking with Marc that when you started out it was rare for experimental psychologists to work with patients. You said you worked with college students and animals. It seems like it would be particularly challenging to design good

experiments with patients. Can you talk a little bit about how one goes about designing a good experiment?

BM: Well, you have to have a hypothesis, I think, to start with. The studies that I was doing with patients was working with patients who had known focal brain injuries; not people who had just something diffusely wrong with the brain, because you can't learn a lot from that I don't think. But particularly I was really very privileged to work with neurosurgical patients. Because these were young adults, they were not having any life-threatening condition.

They had focal epilepsy—which impairs your quality of life—and it was not responding to drugs. And so it's possible to remove an area of the brain which is malfunctioning and causing seizure; but a focal area—part of one temporal lobe, for example. And you can examine them and test them in many ways before and after.

Initially you don't know too much what you're going to find; except if the patients themselves are complaining that they're having trouble with memory, then you think you might be working in the domain of memory. But also I have always gotten a lot of inspiration from other people's work with monkeys and with rats, to get some clues as to what might be profitable hypotheses to test and then to see what might have changed.

What I was talking about there was group data, and you can compare the effects of removal from one part of the brain with the same area on the other hemisphere; you know one would be more in language areas and the other in more non-verbal spatial or perceptual functions. I was saying that all this is group data where you're looking at small effects of planned operations and comparing groups statistically, and so on.

But if you move then to looking at an individual patient—like the patient HM, who lost his memory after bilateral removal of the medial structures of the temporal lobe, sparing the rest of the hemispheres—when you're confronted with a person who is forgetting the events of his life as he lives them – I mean we're familiar with this condition now, of this continuous anterograde amnesia: forgetfulness moving forward and forward in your life so you're not building up your autobiography.

Now, there the challenge from was the beginning to say what can such a person learn. Is it possible? After you've established the magnitude of this effect it becomes a little unsatisfying, actually, to a psychologist just to say this is somebody who can't learn anything. You can't prove the universal negative. You know? So, you have to try to find something that he can learn. And the breakthrough that I had was in showing that he could acquire motor skills.

Well, you're asking how you do such experiments in a sense. This is on one patient, or two or three patients—it's not group data—but patients with very dramatic post-surgical memory loss going on and affecting their lives. Then the challenge is to find some sort of task they might be able to improve on.

And I had the hunch that maybe something in the motor learning tasks, because there's some evidence that motor learning might be mediated somewhat differently than some other kinds of learning. You acquire your motor skills best when you're quite young. They're remarkably stable from season to season. You have to practice doing them—your tennis, or whatever your skills.

And then you can't really introspect and say what you've learned. I mean this is very striking, that if you were to be, with practice, improving your stroke at tennis, or at golf, or whatever, and somebody says, 'Well, what have you learned? You're performing better, what have you learned?' You cannot possibly put into words what it is that you're doing slightly differently. And also the attempt to do so spoils your performance.

So, it just looked at these descriptive ways that one might think that this was a domain of skills that might be mediated differently, without you having an idea of how or where. But it made it important to sample the motor learning thing, which I did with the mirror drawing with HM, and then got this wonderful exciting moment when he was showing this beautiful learning of a skill without any memory or awareness that he'd ever done it before. I mean total amnesia for the practice he'd done on it, but the improvement, which showed you could have really dissociable different memory systems in the brain.

But I suppose it starts from having a – not a strong hypothesis that this would be so; I mean this was really serendipitous in that sense. I was absolutely delighted and astonished to see this. But when you're trying to decide what sort of tasks to use, well this might be tapping into different systems in the brain, just because the rules of normal motor learning are somewhat different from the rules of some other kinds of learning.

GC: I really appreciated the fact that you mentioned that if you're going to do a good experiment you have to have some sort of hypothesis, even if the hypothesis is, at the beginning, as simple as there might be different kinds of memory.

BM: Yes.

GC: And then you have your information—like you mentioned the hunch—that procedural memory might be different. And then you go from there.

BM: Yes.

GC: On my show one of the things I try to do—I have non-scientist listeners; probably more non-scientist listeners than Marc does, because my show's not quite as technical as his—and one of the things I try to share with people is just kind of a sense of how science is really done. And that's the reason why I asked you that, because I think sometimes people don't understand what makes a science experiment compared to, say, when we're just goofing around in our garage to see what will happen.

BM: Yes. I think it's more the controlled conditions. You know? That you have to be very careful that you're not giving the animal or person an extra cue, or a little nudge. To draw very rigorously how many trials you're going to do, how your experimental conditions are going to be, and so on. And if you're doing this in a group of participants, that you have maintained these conditions constant. It's really a lot about control.

You have your hypothesis, and you want to set it up so that when you get your results you can interpret them. Or, maybe you can get results and say, well this is interesting. I suppose the process then is to go to the next stage. You get some finding. Now, how do you interpret this finding? It could be A, or it could be B. And in order to do that you have to devise some further experiment to put these in opposition, to have a prediction that such and such an outcome will support hypothesis one, and another outcome would support another hypothesis.

GC: Right.

BM: It's really a matter of control and methodology. But it's systematic common sense, really. It starts with common sense, I think. The hunches are common sense in a way, or soaking yourself in the field, being interested.

GC: For example, I was listening to someone criticize the famous Stanford prison experiment because of a lot of different methodological concerns, including the question of whether or not that was truly a controlled experiment.

BM: I saw a German film, and then I was told that that was modeled on – was this where they assigned some people to prisoner roles and some people to –? Right. And really it was showing that the role you're playing in those circumstances makes you behave in ways that you wouldn't think you would be capable of behaving. Is that the study?

GC: Right.

BM: People behaved very brutally when they were put in control as warders, or something, and yet you wouldn't think a priori, perhaps, that these people would be capable of that. That was the kind of study they did in Stanford wasn't it?

GC: Yes.

BM: I haven't read the original experiment. I saw a German movie a few years ago at a film festival here—I think it was called 'The Experiment' or something—in which they took people and assigned them arbitrarily to these roles. I found it very disturbing to watch, actually; extremely disturbing. Especially the humiliations they put people through. Right?

GC: Right. The original experiment was done in the 70's—I don't know exactly what year—and I think that it obviously would never be able to be replicated because now the ethical concerns would prevent that.

BM: That's right.

GC: It's just an example of how difficult some things could be to study in a controlled way.

BM: Well, you see, I think you're absolutely right, that you've taken there an example from social psychology—that would be called social psychology, I think—whereas I've always been working with groups of people, but not on social phenomena, nor on emotional phenomena.

I've really worked very austerely on – I've chosen to, I'm more comfortable working – and I may be intellectualizing what I do, or whatever. It says something about me. But I have never wanted to work on emotion and I have never wanted to work in social psychology at all. It's a very legitimate field, but it's not one that has ever attracted me.

But I think there's no question that once you get into those fields you are getting so many variables, and that just methodologically they must be incredibly difficult to control compared with just seeing if somebody can learn a list of words. You know? It's not the same thing at all.

And I can see that such a study would lend itself to a lot of criticism, even though the criticisms might not all be valid. But it would invite criticism because it's so multifactorial in a way, a study like that. There's so much involved. The more things that are involved in your experimental setting the more difficult it is, of course, to control it, I think. So, I wouldn't question that there would be things that one would criticize.

But yes, I think it would be unquestionably unethical nowadays. You know there are all sorts of studies—individual studies too—where the ethics now, I think, would be questioned. Where you can show that people are led to believe that they're increasing the pain that their subject experiences, that they still will go on and do it. People will do quite nasty things.

GC: I don't think we can argue with that. I want to get back to your work, Dr. Milner. Of course you're famous for your work on uncovering various kinds of memory, but when you were talking with Marc you mentioned that your original interest was in perception. And I was wondering if you've ever had a chance to pursue that interest.

BM: Not really. Yes, I've always been interested in the interaction of different sense modalities. And we were working on, really, applied psychology, during the War, trying to say whether airmen should be fighters or bombers. But we were using the sort of tasks that we were interested in doing research on, and this business of whether you can trust your instruments or not, and how information from different sensory modalities—vision, and posture, and so on—interact. Now, these are sort of a borderline between senses.

I was going to – and I really regret that I never was able to do this. When I came to Montreal after I'd been teaching at the University of Montreal, French University, when I decided to do a Ph.D. at McGill with Donald Hebb, before I ever got involved with patients or the Montreal Neurological Institute, my project then was to look at tactile form perception—this is where I'm talking about perception—in the congenitally blind.

If you're not congenitally blind, if you become blind later in life, you have built up all sorts of visual imagery of what you're doing, and so your approach to many things may be like a sighted person. But if you're congenitally blind you have no experience of vision, you'll be using your visual areas for something else. Then it's very, very interesting.

I wanted to see how they were forming concepts. I had different sort of nonsensical shapes, and so on, that had some common feature. And you arrive at tactile concept formation by exploring by touch. As you explore these forms you arrive at a notion of what is the common feature.

Now, a person who has vision, even though they're doing this with their eyes closed—they're doing it by palpation—people tend to approach with big sweeping movements of their hand as they explore a raised form on a cardboard sheet. We would. You would, if you were doing it now. I'm doing it now automatically.

But the congenitally blind person has a totally different approach. They start just feeling the little edges and the little broken-up bits, as you do with braille or something; a totally different way of actually exploring this. And I was very interested in seeing how they would arrive at common concepts using such a totally different approach. And so, this is a kind of interest in perception that I had.

But, as I say, I never pursued that. As soon as I got this opportunity to come to the Montreal Neurological Institute—as Hebb asked me if I would like to—I got so fascinated by the patients here. And, although I started here with some visual perceptual tasks, it was very clear that the profitable way to go—I mean profitable in terms of generating results—was to go through the field of memory. And so, I stayed with memory ever since. It wasn't that I started life in psychology thinking memory was what I wanted to work on. So, I've never really gone back to perception.

GC: And I guess that's probably a common experience for many scientists who end up in a different place than where they thought they were going.

BM: Oh, yes. And especially, of course, if you find yourself with a patient population that you're working with, because you get the clues from the patients. You know if the patient says they have a problem with memory, maybe they're wrong. A lot of people think they have a problem with memory and they've really got a problem with attention. But the thing is that if the patient believes they have a problem with memory you have to investigate that.

As a medical doctor, if the patient says he has a headache or something, you have to explore that. You mustn't start looking at something totally different, or you're not going to help the patient or find out more about the patient. And also you're not going to help yourself, because you're not going to gather interesting or illuminating data. So, the situation you're in is going to guide the questions that you ask.

(Music)

BM: On the other hand you choose your situation. I mean there are many situations in which I would not have been happy. I personally would not have been happy working in a psychiatric department, where many psychologists work. This would not appeal to me.

I would not be very good at working with very small children, whereas actually wonderful psychology work goes on developmentally. I mean it's terribly important to know how all these skills develop, and how the brain develops. It's a

magnificent field of research. But it wouldn't do for me. I would not be at ease in it. You know? You do what comes naturally to you.

GC: Absolutely. Well, thanks to your work, and the work of many others of course, experimental psychology is now an essential part of cognitive neuroscience. I was wondering if you have any thoughts about the increasingly interdisciplinary nature of neuroscience.

BM: Oh, I think it's wonderful. Absolutely wonderful. Of course, I'm privileged to be in the Montreal Neurological Institute. Because from the very beginning, years ago—and this institute was founded 75 years ago—Dr. Penfield, a neurosurgeon who founded this with a great interest in the surgical treatment of epilepsy, he was pioneering this. But he always wanted it to be as multidisciplinary as possible, and also as multinational as possible, which was very nice.

He always encouraged people to come and study and work here from all the quarters of the world you could think of. He had Japanese, and Europeans, and Americans, and all kinds of people from all kinds of places combining their interests. And even in those early days when neuroscience was still pretty simple in a sense, or pretty young, we still had room for biochemists, and electrophysiologists, and psychologists, and of course, surgeons and neurologists.

And then, as the field itself has grown, we've got representatives of the molecular, and the systems, the people who want to look at the small details of what is happening in individual nerve cells, the people who are looking at whole populations of neurons, and aspects of behavior. They're all under one roof in one building, and so you can talk to these people back and forth.

You know it's very, very enriching that you can move around in a place which also has the surgery, and the hospital, the patients, and the basic science all together. It's enriching for everybody. So, I feel very happy about this melding of disciplines. It remains a challenge to bring the really molecular, the really nitty-gritty details, into meaningful conversation and dialogue with the people working at a systems lab, or something. Nowadays, of course, students going in taking degrees in neuroscience, as distinct from individual fields, are learning all these fields.

Like genetics is tremendously important. I really am a total ignoramus in genetics, and I can go to some of these molecular lectures and I just don't have the vocabulary. But I'm also quite sure that many of those people go to some of the systems lectures and they don't have the vocabulary either. And now young people are being taught both, which is great.

This won't still totally resolve this, because I think there are some people who are happier working on a bench at the molecular level, or chemistry level, or something, and people who are happier working with individuals—individual animals or individual people—doing functional imaging and things like that. And they are very different kinds of personalities. And so, I don't think it's going to be so easy to get the two sides combining in one head. But that's where we have to go.

GC: Yes, I was talking to Dr. Michael Arbib about this—he's in charge of the graduate program, I think, at USC—and he said that it's a challenge to give students today both sufficient breadth and depth in a single area; enough depth that they can then make a contribution.

BM: He's absolutely right. Yes, it's very difficult.

GC: So, do you think there are any particular contributions that psychology makes or can make?

BM: Oh, I hope so. Yes. Well, I think first of all we pose some of the important questions about different kinds of learning. What are the questions that we want people to tackle at a molecular level? We still have to do this experimental analysis of behavior.

And now, of course, we have the tools of functional neuro-imaging, so it's possible to study activity—once removed, of course—in the brain of a normal, healthy, living person while they solve problems, they do memory things, they do perceptual tasks, or whatever. And this has been sort of fruitful. But I think the psychologists are having to produce the questions, I would say.

And we also need all these technical people with their insights into breaking our questions down in a finer way by what they learn from the more detailed analysis. But I think we'll always need the psychologists to bring some of the sort of basic questions if our goal is to understand brain behavior relations as neuroscientists. We are trying to do that, no?

GC: Yes. I interviewed a couple of people about neuro-imaging a few weeks ago and one of the things that they pointed out was that it's a team effort; that the psychologist is very important to designing the experiments properly. Otherwise imagining in and of itself is not enough.

BM: Oh, of course. Exactly. Imaging is a tool. Right? It's a wonderful tool, getting more and more refined. It's an incredibly refined tool. But you have to have a question. You know it's no good having a wonderful tool if you don't know what you want to use it for.

GC: Right. Thirty years ago when I was thinking about going to medical school, medicine was just starting to open up for women, so we didn't really have many female role models. What was it like for you as a woman trying to get in to your Ph.D. program in that period right after World War II?

BM: Oh, well, I've never felt any difficulty about being a woman. I mean I've never felt discriminated against being a woman. Of course, I came from England and was teaching, actually, at the University of Montreal. In England before the War nobody did Ph.D.'s. They did research—the important degree.

And even to this day, although they do Ph.D.'s, the important degree in England is that first degree, how well you do. Whether you get a first-class degree or just a scrape-by degree, or whatever, it determines your whole future career. It's very, very important. Those exams that you write when you're 21 determines really your future life in England.

And the professors—and there was only one professor in the departments in those days—the head of a major department like Oxford or Cambridge, would not have a Ph.D. because it was not part of the system. You did your research and you published, and you became known. But it was not part of the system.

That was another reason why I decided I had to do a Ph.D. I was able to teach at the University of Montreal—the French University—because I spoke French, and they were so glad to have somebody that could teach experimental psychology in French. But, though I enjoyed myself there, I did not want to do that for the rest of my days. And I realized then that I should go ahead and get a Ph.D.

You phrased it as getting a Ph.D. I don't think that would be a problem. Perhaps the question you were really meaning to ask was what was it like coming to the Neurological Institute, or some medical environment, where there were not that many women, it's true. Or even during the War when I was on a radar research establishment there was a woman librarian, and otherwise there were no women officers in the place. They were all men, and they were physicists, and so on.

I've never felt discriminated against. And here at the Neurological Institute; in 1950 this institute—as I'm sure the National Hospital, Queen Square in London would have been even more so—was very hierarchically organized. It was a very authoritarian environment. It isn't now, but it was then. I imagine any medical school was like that.

GC: Yes.

BM: The psychology department wasn't, but medical schools, yes. And so, Dr. Penfield, who was very definitely the chief—we used to call him The Chief—and he was the head of everything, and there was this sort of pyramid. But it was not based on gender lines. I never saw any evidence of gender attitudes in Dr. Penfield. If you were a student you were a bit lower in the hierarchy, and so on and so forth. But it was never a gender thing.

I mean the only time in my life when I really felt handicapped by being a woman was when I was competing with other women; when I decided in my high school that I absolutely wanted to go to Cambridge, and that I had to get scholarships because I had no money, and that I wanted to do it in mathematics. And mathematics wasn't particularly well taught. I went to a girl's school. It was a very good school, but they sent me to Manchester University to get extra coaching in mathematics to get in and to get this place in Cambridge.

At that time in Cambridge University—and Oxford was the same I'm sure—there were two women's colleges. And of course the men's colleges were entirely for men. Now, it takes three years to get a degree. So, those three years summed up across the two colleges, there could not be more than four hundred women in the university at any one time. And to get a place there you were competing with women across the whole of England.

This was the time in my life when you could say that it was a handicap being a woman. It would have been much easier to get in if I had been a boy. But once I was in Cambridge it was up to me. I never felt any discrimination anywhere.

GC: Do you have any advice for women scientists today, or women interested in science today?

BM: Well, I mean, go ahead! Women are doing so well in everything. Right? I'm sure it's the same in North America; I think this is true here also. I teach in the medical school here, and there are more women than men in the medical school every year. Go ahead, and you can do it. There's no reason why you shouldn't be able to do it, if you want to do it.

Now, the question is, is it going to appeal to you and are there difficulties later on. You know in terms of family and all the rest of it—all these practical questions. But as far as doing the science, as far as the intellectual part of it, go ahead and do it.

GC: What about young people in general that are considering a career in science? Or maybe I should ask you about neuroscience, because I get a lot of emails from young people that are in college and they're getting turned on to the brain, they say. I just got one from a physicist who said that he really wanted to

get into neuroscience but he wasn't sure how, coming from physics. But it seems like everyone I talk to says it doesn't matter what science field, if you really want to get into neuroscience you've got something to offer.

BM: Oh, you definitely have. And coming from physics, through imaging and those things – I mean you were talking about multidisciplinary. We have a young man in the next office to mine here at the Neurological Institute. Just a stone's throw from where I'm talking to you now is a 35-year-old physicist from England who has just come here as a tenured position with a really important role to play in improving our positron emission tomography, improving the images, the technique. You can get interested in whatever aspect you want. Physicists are highly valued and are playing very key roles. So, coming from physics is certainly no difficulty.

Where I see a problem, where I really do worry about neuroscience, is because it is so very attractive and we are producing so many young talented people, and then we have to find the jobs for them. A lot of us feel like helping to support post-doctoral fellows and so on. It's fine. Students have no trouble, if they're good, getting funding right up through the Ph.D. and then maybe for a year or two after that.

But then, if they want to go further—they can of course get siphoned off into industry or something like that—but supposing that they really have a feel for academic life, it is very, very difficult. However bright you are, it's quite difficult to find a nice niche in a university to get started on an academic career. As I say, you do your post-doc and then the money runs out. Obviously you're not supposed to be a post-doc forever.

And I feel that perhaps we do need to give some thought to this. Are we overproducing these bright young people? Or do we want to try to devote more funds and attention to that necessarily rather long period in which they have to more or less build up a little reputation for themselves before they even get an academic job? Whereas in the old days, once you got your Ph.D. it was pretty easy. There were universities crying out for teachers, for research workers. You know? So, that is a problem. That shouldn't discourage you from going into science, but it's still a little disturbing.

GC: Yes. I think they did a survey of a class—I think it was Yale, the students that got Ph.D.'s in molecular biology maybe 15 years ago—and they discovered that the majority of them are not in academic positions now.

BM: That's true. I think there was an article in *Science* about that quite recently. Yes, I think that's so.

GC: Yes, I worry about losing so much talent to industry when the knowledge then becomes proprietary and doesn't get shared the way.

BM: Right. I agree with you. Yes, it is worrying.

(Music)

GC: Today's episode of the **Brain Science Podcast** is sponsored by audible.com. You can get a free audio book download for signing up as a new member at audiblepodcast.com/brainscience.

(Music)

GC: If you have a few more minutes, I wanted to ask you a question that I saved for in case we had enough time. I was wondering if you would talk a little bit about your work with the split brain patients.

BM: Oh. Yes, with pleasure. That was fascinating and that was such a privilege. Really, it was. That's really taking me back. What I was really interested in, when I first came to the Neurological Institute, and before I encountered the amnesia—you know, when you encounter these patients with huge memory impairments then, as I say, you get absorbed in that—but I've always been very, very interested in the hemispheric differences, hemispheric specialization in the human brain.

And I've always been very, very interested in the right hemisphere. If you take a typical human being—not for all of us, of course, but for the typical right-handed human being—the left hemisphere is the one that is dominant for language, and for programming of voluntary action, and so on. And so it used to be called the dominant hemisphere; not the dominant hemisphere for speech, but just the dominant hemisphere.

And when I first went into this field there was such an obvious contempt among neurologists—it was quite disgusting—for the silent right hemisphere. Almost as though it were a spare tire; it was a very good thing that if you had the misfortune of having your left hemisphere damaged or something at birth, that you had this spare tire that would take over. You know?

But otherwise, apart from controlling the movement of the opposite side of the body, and receiving the opposite visual field, and so on, it was really rather despised. Which was ridiculous because it's just as big, and obviously would be just as important. So, I was always particularly interested in this right

hemisphere. That's why I was perhaps looking at some of these perceptual and spatial abilities, and so on.

And I was doing memory studies on patients with unilateral lesions. These are small deficits associated with epilepsy, in the left hemisphere compared with the right, and so on. And that was when I met Roger Sperry, who went on to get the Nobel Prize for his work on the split brain population. I met him at a memory meeting in New York City. And we talked, and then he sent me a note afterwards and said, 'If you're really interested in knowing what goes on on the right side of the faults—that's in the right hemisphere—hop on a plane and come out.'

And so that was when I started going with a former colleague of mine, Laughlin Taylor. We would go out for a week or so to Los Angeles—to Pasadena, to Cal Tech—and work with these patients, intensively for a week. And we were always working on something that was a little different from what Sperry's own students were doing. We tested the patients in their homes, and he provided us with a car, and so on and so forth. And he obviously wasn't doing that in order for us to duplicate what his own students were doing. So, we always went with some special question that we had.

Yes, it was absolutely fascinating. And science owes patients with seizures a great debt. But these were patients with seizures which were not just coming from one small area that could be removed. It was difficult to localize. And the goal of this rather drastic surgical procedure was to reduce the incidence of really major convulsive attacks that these patients might have. The idea was that if you interrupt connections between the two halves of the brain you prevent the spread of activity. Because an epileptic seizure can begin locally, but then if it spreads to involve both hemispheres rapidly that is when you're apt to see the major convulsions which are so disturbing to the patients and to everybody else.

So, this procedure was just to sever the connections of the corpus callosum—this huge fiber tract which connects the hemispheres together—and of course the lower parts of the brain are still unified. And also your conjugate movements of your eyes, as you walk around in the world the two hemispheres are exposed, really, to the same input in a sense, except in an experimental situation. This gave Dr. Sperry and his students this amazing possibility of comparing the functions of the two sides of the brain of the same individual.

Now, you realize from a scientific point of view this is a wonderful control experiment. You're always trying to match your subjects to have in different conditions. But here you have the two halves of the same individual brain. You couldn't have a better control situation for comparing left and right than that. And then he had his special methods so that you could always use touch. What you

feel with your left hand, with vision excluded, is only going to go to the right hemisphere, and so on. You could devise these tasks.

But what is so disconcerting at first, until you get used to it, is to realize that – you see this person looks perfectly normal just like you or me, that's sitting in front of you. And you start talking to them, and then you have to realize that you're only having a conversation with the left cerebral hemisphere because it's only the left hemisphere—I'm saying in the typical right-handed person, and it is the case in this series of patients that the left hemisphere was the dominant hemisphere for speech—and so, essentially it's only the left hemisphere of this seemingly intact individual, who really has a split brain situation, that is able to talk to you. And the right hemisphere is really imprisoned—is mute—in that sense.

And what is really interesting then is all the methods. We did some studies on memory, and Sperry did lots and lots of studies on different things, by devices. You can train the right hemisphere to put the hand under a screen and palpate something, and then after an interval to retrieve it. They can understand that. But the individual sitting there is unable to tell you what it is that the left hand, and therefore the right hemisphere, is grasping. The person can only guess.

This divided consciousness in one brain really captured the popular imagination. Sperry was a magnificent experimental psychologist, but a lot of us feel that some of his most brilliant work was work which he did much earlier in his life, on frogs and so on. But what really captured people's minds was the idea of these two consciousnesses in one brain, so to speak.

And it was a great privilege to be able to work with these patients. And in some ways also, to compare what we were finding with those patients with what I was finding with HM, my amnesic patient, who had intact commissures; who was not a split brain patient but who was lacking these critical structures for autobiographical memory. I was able to show that even the mute right hemisphere without any words could perform better on some tactile memory tasks than HM could. You see?

Which had some theoretical importance, because it showed that you didn't need to have verbal labels. There were some psychologists who said you can't remember things unless you can apply verbal labels, and these amnesic patients are not doing that. I was quite sure this was nonsense. But a very nice way of showing that it was nonsense was to show that the right hemisphere, although it had no words, was doing better than HM was doing on such a task. So, it was very, very exciting. I found all the work with the split brain patients very exciting.

I'm also very glad that you asked me about it because you say people are interested—or I'm certainly interested—in the history of my field. One of the

things is how fashions come and go in a field. And memory was unfashionable for a long time, and now is of course very, very fashionable. And the frontal lobes are extremely fashionable. That's another area I've worked on.

But the interesting thing is that all this excitement about the split brain patients—the commissurectomy patients and Sperry's Nobel Prize, and so on—there were all sorts of rather wild things that you read in the newspapers. Like you must start to educate the right hemisphere; which is nonsense, because when you've got a person with intact commissures you're only educating the whole person. You can't educate a hemisphere. You know?

But you read all this sort of stuff. The popular press went overboard. And then suddenly now nobody talks about it. Nobody has asked me anything about the split brains for ages—I teach the medical students about this—but nobody ever in an interview. You're the first person, I think, who has asked me about that. So, I was very delighted with your question.

GC: I read an older book by Michael Gazzaniga. Was he a graduate student of Sperry's?

BM: He was a student of Sperry's, yes.

GC: It was called *The Mind's Past*. It's actually one of the books that got me interested in neuroscience. He was talking about how the left hemisphere in these patients would sort of make up stories to explain what the right hemisphere had done. Did you experience that?

BM: That's right. They have this need to rationalize. I think we all have a need to rationalize, to make sense of things that are happening. And yes, they would do that. And Sperry himself used to like to tell this story in one of his introductory lectures about these patients. They can have this set up where they can flash images briefly into one hemisphere for such a short time that you can't move your eyes, and so they can be sure that the image has gone into one hemisphere. And he would flash slightly suggestive, sexy pictures into the right hemisphere.

This young woman—NG—she blushed a little. Of course, emotion can get transmitted by lower centers that are not separated, you see. So, this patient probably felt a bit uncomfortable, or whatever. And the left hemisphere said, 'Oh, Dr. Sperry, what a strange machine you have there,' or made some comment about the machine, or what Sperry was doing. Because she obviously had no idea of what actually had happened—did not have access—but had access to the emotion and was rationalizing, trying to understand the way she was feeling, I suppose.

There's also a very annoying tendency of the left hemisphere to sort of interfere with your experiments. As I say, we used to test them in their homes. We would drive out to the home. We tested actually more patients than Sperry had tested by then. We got more of his group active and he worked with them. But until we went out he had only worked really with a couple of these patients; the star performers, so to speak. There was this NG, and then there was LB, who was really a sort of smart-alecky young man; quite bright. I think he was supposed to have said when he was asked how he felt after the surgery that he had a splitting headache.

Anyway, this patient was very verbal. I mean his left hemisphere was very active, let's put it that way. And it could be quite annoying. If we would do an experiment, say, one of the experiments that we were doing on touch where the patient is working with the right hemisphere and the left hand under a screen—the sort of experiment I was describing to you earlier where there were pieces of wire, a coat hanger bent into different nonsense shapes—he would have to palpate this and then be quiet for a minute or two, and then put his hand in again and find the shape.

So, this was all addressed to the right hemisphere. But then in the middle—and this would happen—the patient gets up and says, 'I want a coke,' and rushes to the fridge and gets a coke. You see, it's in his kitchen. And so the left hemisphere is very much the boss in this way, and working with the right hemisphere is quite tricky because it's really very underprivileged without words. And they were very, very fascinating patients.

(Music)

GC: When you were working with patients like HM, he couldn't remember long enough to even come up with an explanation of what was going on for him. Or could he?

BM: Oh, yes, HM knew he had a very bad memory. He was never confabulating, and he was very logical about things. He lived with his elderly mother. And she said she didn't like to go out and leave him at home because—and this I think had actually happened—somebody could come to the door, and HM would open the door, would not recognize the person, but would think, 'Well I have a bad memory and maybe it's just somebody that my mother knows,' and would ask this chap in. And the mother would come back from her grocery shopping and would find this stranger sitting in the living room. Because HM quite logically had thought, 'Well, I don't remember. I have a bad memory, and maybe this person is somebody my mother knows.'

This is true of all the patient here of Dr. Penfield's that I studied with the same memory disorder. There's a complete awareness of the fact that they have this forgetting problem and they are disturbed by it, but they never make up stories to confabulate or anything. It's not like somebody with a Korsakoff's psychosis from alcoholism, where there's much more involved than this system in the brain, really, and where you do get people making up stories and saying that they've been downtown playing bridge when they've never left the hospital. HM wouldn't do anything like that.

GC: Well, before we close is there anything else that you would like to talk about? I guess I've taken up a lot of your time.

BM: Oh, no, it's a pleasure. It's nice. Especially when you asked me about the split brain patients, because that was a little treat for me. I didn't expect it. I was sure this was going to be all about HM and things leading out of HM, or else the general questions that you did raise about such things as the multidisciplinary aspects.

We're so indebted to technology. I suppose I do like to emphasize that, because when I started here all those years ago we would have to trust so much what we could find out clinically and behaviorally. We got plain films of the skull, we got the shape of the ventricles of the brain, and that was about it. And even the beginnings of electroencephalography, the EEG—which is so important when you're studying epilepsy, because it's abnormal electrical activity, after all, that you're looking for—we had a person who became a real world expert in EEG, Dr. Herbert Jasper.

But even with all Dr. Jasper's skills and so on, EEG was so primitive in those days compared with what it was to become. And of course the imagery wasn't even dreamed of in those days; the imagery that we have now. So, the technology is making it possible for us to ask questions with more refinement; questions that it would have been stupid to try to answer years ago because we didn't have the tools. So, I think we're greatly indebted to the technology.

GC: Yes, I observed when I was talking to my guests—we were talking actually about the MRI—it's in a way similar to the impact of the microscope. There are questions you can't ask until you have certain instruments. And I guess that's why the physicists are so excited about the Large Hadron Collider.

BM: Yes.

GC: But that doesn't take away from the importance of the creativity and curiosity of scientists like you, who are the ones coming up with the questions.

BM: Well, this is right. I mean this is where it's teamwork. It's this interdisciplinary teamwork. It's really exciting. It's an exciting atmosphere to work in. You know? As I say, I feel very privileged to be part of this kind of environment.

And we've been living in such exciting times across the last 20 years or so. We had a visitor here from Italy in neuroradiology yesterday, and we were all chatting along these lines last night about just this; that things have moved so quickly over the last 15 years or so. The pace of this has been incredible compared with previous periods, I think.

GC: That's why in doing my podcast I don't have to worry about running out of anything to talk about.

BM: Definitely not.

GC: And there are so many great people to talk to. I really appreciate you coming on the **Brain Science Podcast**. Thanks so much.

BM: Well, thank you very much. I've enjoyed it. Bye bye.

GC: Bye.

(Music)

GC: I want to thank Dr. Milner again for coming on the **Brain Science Podcast**. She was a very gracious guest. And don't forget if you haven't heard her interview on *Futures in Biotech*, that is Episode 33 and there will be a link to that in the Show Notes at brainsciencepodcast.com.

I would love to hear your feedback about this episode and anything else about the **Brain Science Podcast**. The best place to share is at our Discussion Forum at brainscienceforum.com. You can also send me email at docartemis@gmail.com.

I also want to mention that I just posted a new episode of my other podcast, **Books and Ideas**. It's Episode 23 and it is an interview with Nobel Prize winning physicist Frank Wilczek, the author of *The Lightness of Being*. We talk about the Large Hadron Collider, and if you're like me and are kind of wondering what a hadron is and what is the Large Hadron Collider for, you'll want to hear that episode. You'll find **Books and Ideas** in iTunes or at booksandideas.com.

I want to thank those of you who have made contributions to help support the **Brain Science Podcast**. Don't forget that you can now send direct donations and you can find the address at the website, brainsciencepodcast.com.

If you happen to be a fan of Leo Laporte's work you can also find me in the TWiT Army Cantine. Anywhere you look for me I will usually be using the name Doc Artemis. I just set up a Neuroscience News Network on the new website, socialmedian.com, so if you want to check that out it's in open beta. And I need help in improving the sources for that network so that it can become really useful.

Also, I keep forgetting to tell you that if you see a great article on the Web and you'd like to share it with others in a really quick way, just give it the Delicious tag 'brainsciencepodcast,' and it will automatically show up in the **Brain Science Podcast** Friend Feed Room.

Usually after I post an episode I realize that there's something I meant to announce that I forgot, and I'm sure there will be this time, too. I'm getting ready to head off to the Society for Neuroscience meeting in Washington, D.C., so if you're going to be at that meeting please do drop me a line and let me know so that we can get together. If you are going to be at the meeting and have any ideas about content that I can create for the next episode I would love to hear from you.

Thanks again for listening. I look forward to talking to you again real soon.

(Music)