Dr. Mortimer Mishkin

This is a third interview with Dr. Mortimer Mishkin,

Chief of the Cognitive Neuroscience Section of the

Laboratory of Neuropsychology of the NIMH Intramural Program,

held in Bethesda, MD on May 28, 2002.

The interviewer is Dr. Ingrid Farreras of the NIH History Office.

Farreras: Did Josephine Semmes arrive in the early '60s? How did this come about?

Mishkin: Yes, she arrived in the early '60s. I had known her from Luke Teuber's lab, and so we started discussing things when she moved here to Bethesda. In fact, I even have something that was never published but that we spent an awful lot of time talking about. It was my work, but she had a lot of input to it. It wasn't work, it was thoughts about inhibition and neural mechanisms of inhibition. We had a long discussion about that. We had lots of discussions – when she was not employed – about the meaning of the logic behind double dissociations of deficits after interventions of one kind or another, mainly lesions. And out of this grew the idea that she would come into the section and so I am sure I was partly responsible for getting her involved again in research. When she came into the section, she wanted to pursue the work that she had done with __________ at Orange Park on the somatosensory system. I was very happy to help her because, whereas she had the ideas that she wanted to pursue, she was not skilled as an experimental neurosurgeon, which, of course, I was, having been trained – as you know – by Karl Pribram. And I, in turn, have trained a lot of other people. But this was not her forte. Josephine was a really bright, brilliant woman, but she was not skilled in neurosurgery. So I did the surgery. We talked together about the research design. We had tactile apparati built for the monkey, interesting ones, actually. We came up with a design in which the animal was not in the dark. The animal put its hand through a plumber's elbow to reach an object that was put in place behind the elbow, and when the animal pulled this lever – that's what it amounted to – a drawer opened either containing a piece of food if it was a positive object or an empty drawer if it was negative. So we trained monkeys on all kinds of tactile discriminations this way. There were two such elbows, one above the other and therefore two places to put the tactile stimuli.

So we did many, many experiments together, which were published. This collaboration ultimately stopped, not because she left the section – because we continued working together after she left the section – but there was a horrible problem for her because she felt as though she wasn't being given the proper status. And that's correct; she wasn't. So we had to go together to the administration to get her independence from all, and she therefore took up space in that part of the U [of Building 9] that was closed in, which had been Ron's [last name?] initially but was later enclosed, allowing us to enlarge the section, although we lost it, in a way, to her. I continued collaborating with her for another four or five years afterwards, until she left the laboratory – somewhat discouraged – to go to the Extramural Program.

Farreras: Ben Carlson mentioned that she was switched from the Animal Behavior Section to his Perception Section, even though she was still working in Building 9.

Mishkin: Yes, that's correct.

Farreras: Okay. He couldn't remember whether it was over problems with Rosvold or with anybody else in the section. He didn't remember what the dynamics behind it were but he did remember that she was in his section, even though he didn't have anything to do with her research.

Mishkin: That's correct. There was some question about whether she should be an independent investigator within the Section of the Chief, but who was the chief at that time?

Farreras: Rosenthal was the new Lab chief in '66…

Mishkin: Yes, and that's why I think it was decided that that was not a good idea. One of the people who helped make this decision, I think, was Bob Cohen; where she would be and how this would be arranged. I know that he and maybe Eberhart appointed a little committee that consisted of…now my memory fading is going to show. Who was basic research in Cellular Pharmacology?

Farreras: Giulio Cantoni?

Mishkin: Yes, Cantoni. Thank you. Giulio Cantoni. He was a member of two or three – I don't know who else was in that little committee that was appointed to adjudicate. I remember Josephine and I went to the committee and made our complaints. I guess they spoke with Rosvold, too. And we somehow were able, Hal and I, to get over it. And that was true at a later time as well, because this came up, too, with Pat Goldman when that problem arose, because at that time he was backing her for giving her some resources that I thought were going to essentially be taken away from me or divided in a way that I thought was unfair, and so I became quite angry. But in the end, we got over that, too. So we were able to continue collaborating until the very end, when he had to leave because of his illness, although I did collaborate with him less and less as I became much, much more independent myself.

Farreras: Let me back up for a minute, when Semmes came here, was it to work with you?

Mishkin: When Josephine came, we were collaborating. That's correct. But she wasn't brought in to work with me, although that's how it worked out. She was, after all, senior to me, by quite a few years.

Farreras: I wondered, when you said that she wasn't considered independent enough, whether that meant that there was too much overlap between her work and Hal Rosvold's work or…

Mishkin: No. I'll tell you what the issue is. The issue has been and always will be with us.

Farreras: "Us", meaning…

Mishkin: Investigators. Investigators in laboratories. Resources are never enough. There are never too many resources. There's always too little. It doesn't matter how many because you always fill up the space and you always make use of whatever resources you have, and you always need more. It's not just space, it's resources of another kind: people, help, what is called at NIH "other-objects money". I don't know if it's still called that. Other-objects money is all the money that one uses, other than for salaries, to support one's research. Hal was the one in our lab who would always approach the administration to continue getting as much as we were getting for the section and the lab and always asking for more. That's the way it worked.

Farreras: But by that time, the Section Chief – not the Lab Chief – delegated the resources, no?

Mishkin: Right. And there really was nothing formal. So there was precedent, in the sense, that once some cages were made available to me – which meant that I therefore could fill these cages with monkeys and use them for the research that I wanted to do – that those cages would never return to the initial owner. So precedent would determine to a large extent how the resources – which in our case was a way to house money – were divided. It was often a bone of contention among the investigators in the lab. That's what it was always about, because our research with monkeys depended upon how many monkeys we had.

Farreras: I see, yes. And this was when you were all in Building 9?

Mishkin: Right. And I'm quite sure it was built initially to house rodents and other small animals, rabbits maybe. And in 1955 it was renovated, in part for us, in part for a number of other laboratories, although I think we were one of the first to move in. I even remember seeing it being renovated, walking through old Building 9. The other labs that were there were people from NINDB. They had one wing; we had one wing, the middle section. Then there were two wings on each side of the middle section. One was initially, I think, given over to NINDB, and Maitland Baldwin, who was a neurosurgeon that came with Penfield in Montreal, had chimpanzees there and did research on chimpanzees, maybe monkeys, but mainly chimpanzees, I think. We had chimpanzees. __________ Crawford was one of our animal-care staff, and we had about a dozen chimpanzees in our lab at the time, as well as monkeys. Remember I told you that we had started at Yale and had done an experiment on chimpanzees, and that work was continued here with __________. We did lobotomies on chimps, and that would not be permitted any longer, but we did. We did frontal lobotomies and published a paper on them. Tried to find out what it did. So on the other wing opposite the one occupied by NINDB was a neuroanatomy wing, which I guess also belonged to NINDB, and there was Windell and Rasmussen and a neuropathologist who outlasted both of them and continued for a long time, but I'm afraid I've forgotten his name for the moment. And then there were some younger people, our age. These were the senior people at the time __________. And they are still in NINDS as far as I can remember. Now, who the hell are they? Let's just see if I can. Bob Burke was never in Building 9, but Bob Burke goes all the way back to the same time as we __________. Tom Reese was, for a short time, in Building 9. I don't think any of these other people were. Hal Gainer. Wayne Albers may have been. I'm not sure. But these people go way back, too, not as long as Bob Burke and I. __________ perhaps most of the others. I'm sure that Al will remember because he was actually a friend of both of us.

Farreras: Marsan?

Mishkin: No. Al was close to Cosimo because they did collaborative research using EEG. But, no, this was a neuroanatomist who ultimately did molecular studies, electron microscopy and stuff. I can't remember. Anyway, you might ask Al. I'm sure he'll remember.

Farreras: Okay.

Mishkin: So, anyway, that was a little bit about Building 9.

And Josephine is where we started. I don't remember the year that she finally left, but it might have been about '75.

Farreras: Okay. I think the Perception Section existed until about 1979, but Carlson had mentioned '75…

Mishkin: Josephine left before, certainly before '79. She was probably there, actually, even a little earlier, in '70.

Farreras: Did the shift to the Perception Section help much? Was the funding getting reassigned there?

Mishkin: Oh, no. I think that I continued collaborating with Josephine largely through Ruth Nadel, who was Josephine's post-doc. Ruth was a pediatric neurologist who came in to study multis, and she was a post-doc with Josephine, and the three of us continued collaborating. But at some point shortly after that, I no longer collaborated with Josephine, and I think she found it very difficult. This is when she left to take the job in the Extramural Research Program.

Farreras: So before she moved to the Perception Section the only senior scientists in the Neuropsychology Section were you, Hal, and Josephine? And when she moved to Perception it was just you and Hal?

Mishkin: Yes.

Farreras: OK, I wanted to make sure I wasn't missing anybody.

Mishkin: No, just the three of us. There was someone who came in – in fact, we've just been talking about him today – Brian Robinson. He came in from Paul McLean's Section [Limbic Integration and Behavior, in Wade Marshall's Neurophysiology Lab] and we did stimulation studies together of the kind that he had done with

Paul McLean on the squirrel monkey. We worked together. He also worked a little bit with Hal, but I think mainly we worked together. And then he left to go to Emory.

Farreras: On his own accord?

Mishkin: Yes.

Farreras: Okay. I ask because I was recently reading some old memos and letters that discussed the shift from the neurophysiology Lab to your section, and Hal had written that if Robinson was going to prove "difficult" in his section as well that he would be moved out of it.

Mishkin: Really? That's interesting.

Farreras: I seem to recall some friction with Paul McLean and that's why he wanted him out of his section.

Mishkin: Right. But there weren't any difficulties. That isn't what happened. What happened was that he was really interested in using remote stimulation, remote electrical stimulation.

Farreras: Like Delgado's work?

Mishkin: Yes, but with a much more sophisticated design, which was devised in collaboration with Brian by someone from GE, I think, who came to work with him and Hal. And that program that Brian and this engineer put together moved down with the two of them, to Emory, Yerkes' Lab, I guess, and they did some experimental work with that system there. We didn't really have the kind of facilities – unless we made use of Poolesville __________ and there was no way he was going to get there, so he was interested in a place where he could do this sort of work in a more ethologically valid setting. I believe that was part of the motivation.

Now, I mentioned Brian because he was senior. I don't remember what position he had in the lab, but it was one in which he did not have independent resources. So we worked together and did the stimulation experiments together, and he worked with Hal on this remote stimulation design apparatus.

I was looking [at his list of publications on the computer] to see how long he was here. I wouldn't know from what I'm looking at, but you can get a rough idea from the dates of our publications. Oh, God, that's early, '62, '66, '68…

Farreras: Around the time that Josephine was here then.

Mishkin: Some of the stuff might have come out after he left. So I don't know when he left. It could have been about that time. I can't say he was a senior scientist, but he was certainly past something like a post-doc. And if you're past something like a post-doc, there's no way that you can come in unless you have tenure, unless they had a different arrangement with him, which I wasn't aware of. But it might have been agreed that it was provisional, depending on how things worked out, but they went on for quite a while. He must have been with us for five, six, seven years before he left, maybe '60 to '67, something like that. I'm just guessing.

We did have some visiting scientists during that period. We did have post-doctoral fellows. By visiting scientists I mean people who were beyond post-doc but weren't here for a long time. I remember, in connection with Brian, Rob Plitchick, who ended up at this university on Long Island, and who was with us for maybe a year or so. Rob Plitchick is pretty well known in psychology circles for having written on a model of motivation and emotion in which he devised something like a motivation or emotion wheel that was modeled after the color wheel. So he was involved, for a little while at least, in working on emotion in monkey in connection with behaviors evoked by stimulation.

And when did Al leave? Around '61?

Farreras: Sixty.

Mishkin: Okay, so that's probably about the time that Brian came in. So there's Josephine Semmes and Brian, and they were both there much the same time. Brian probably left about the time that Josephine separated from the section to have her own unit within Ben Carlson's section.

Farreras: Did that alleviate any of the resource problems in the Neuropsychology Section?

Mishkin: No, they just took them. In other words, she was given the resources that she wanted, and the way she got them was by taking them away from Hal. So she had a self-contained research unit in that space, and it consisted of a surgery room, where histology was also carried out. John Sulu was her research assistant, so he helped set up surgery and assisted in surgery and he helped do his __________ take care of the animals. She had a couple of post-docs. There was a room, maybe two, in which animals were housed, and then there were a couple of testing rooms and an office. So it was a small unit, but it was self-contained, and she had everything in it she needed to do the research on which we continued to collaborate. When we expanded, for example, those two monkey-housing rooms were part of our section. It wasn't a lab yet. And everything which we gained, we jealously guarded. But those had to be given up by Hal when the decision was made to grant her autonomy.

Farreras: Did she have to stay within the Psychology Laboratory? Was there any similar research being done in the other laboratories where she might have been able to move to?

Mishkin: I'm trying to think if there were ever any suggestions that we might have made about possible arrangements. There might have been, but I can't remember.

Farreras: That was a difficult position for you to be in, siding with her over your mentor and chief…

Mishkin: Yes, yes. But, fortunately, it worked out. I mean, we were able to get over it. And shortly after that Brian left and Pat Goldman came.

Farreras: Yes, I want to ask you about her in a minute, but I was hoping you could confirm something for me first. When I interviewed Ben Carlson he seemed to recall that when the Section Chiefs were asked – by Cohen or by Cohen and other administrators – to suggest someone to take over Shakow's position as Lab Chief, that at least he and Rosenthal voted very strongly in favor of Rosvold.

Mishkin: Yes, that might well be.

Farreras: But that didn't happen; Rosvold wasn't made Chief of the Psychology Lab.

Mishkin: That didn't happen. Why it didn't happen…who would have been Scientific Director at that time?

Farreras: John Eberhart [for Basic] and Bob Cohen [for Clinical].

Mishkin: I think that they didn't want Hal Rosvold because he was in basic science.

Farreras: I see. Morrie Parloff was one of the other Section Chiefs at the time, and I asked him whether he remembers that the Section Chiefs were asked to recommend someone and he doesn't. But he did volunteer some reasons why it may not have been Rosvold. Cohen was very clinically oriented, and as you just said, it might have been that Rosvold was too basic and Cohen wanted someone who was more clinical. Morrie also suggested that perhaps Rosvold's research approach might have fallen into question in some powerful sectors of the scientific community and that perhaps the animal rights groups had also started to…

Mishkin: The animal-rights thing is not correct. As far as I'm aware of that would not have been an important issue at that time.

Farreras: And when I asked Cohen he also didn't think that was a factor.

Mishkin: But I think that it is indeed possible that there were people, even within mental health, particularly in the basic sciences, who derided the kind of work that we were doing, brain lesions, that this is no way to study brain function. And people who would have that opinion would be very likely to be neurochemists and biochemists, maybe even neurophysiologists because according to this view, you're destroying what you're trying to study. Or studying the damaged instead of the intact brain. So, yes, lesion studies have always had antagonists in the scientific community.

Farreras: But these other Lab Chiefs would not have had a say as to who should…

Mishkin: No, no, no, but they might be giving advice. In other words, they might be asking for advice. So I think that what Morrie said is not clearly out of the question, it's possible. But I think that maybe the notion about __________

TAPE 1, SIDE B

__________.

Farreras: Shakow retired in ’66.

Mishkin: I think that the problem with the Silver Spring monkeys, for example, which did impact on NIH because they were brought here…

Farreras: When was that?

Mishkin: It happened later. I don’t know exactly when. [mid 1980s] But the animal-rights activists held candlelight vigils at the house of the director of NIH here on campus. Who was the director of NIH at that time? [Jim Wyngaarden] I have anomia. I can see him but I can’t remember his name. And I think that the animal-rights movement really took off at the time of this Ed Taub affair. That went on for a while before the monkeys were brought here, I think. I’m not sure.

Farreras: What is the “Ed Taub affair”? Is that too much of a tangent for us to go or could you talk some about the impact it had on animal research?

Mishkin: It had an impact all over the world. But you have to read Clemson Thompson’s stuff. And Adrian Morrison would be an excellent source. I got involved by helping to convince Bill Raub – who was the Scientific Director of NIH, I guess, under the Director whose name we’re looking for – to allow us, a group of investigators that I had brought together when I was president of the Society of Neuroscience, to conduct an experiment that had been decided essentially by NIH in consultation with Congress not to allow any further work on these monkeys, who were deafferented. That was what this was all about. They had deafferented arms, which was accomplished by cutting the afferent nerves that contain the spinal cord, in order to study the effects of damage to the nerves. And Alex Pacheco – who is one of the leaders of PETA, *People for the Ethical Treatment of Animals* – was a youngster at the time, and infiltrated Ed Taub's lab, which was in Silver Spring. That's why they call it Silver Spring monkeys. In fact, it was at a private facility that was put together largely, I think, by Joe Brady and some of his colleagues to continue, under private auspices, the work that they had been doing at Walter Reed, pushing Skinnerian behavioral research. It was called the Institute of Behavioral Research, the IBR. Pacheco entered as a volunteer into Ed Taub's lab, and at a time when Ed Taub was away on annual leave for a holiday or something, arranged somehow for a couple of the caretakers to take leave as well, and the place became a total disaster. And that's when he called in the press. And this led to years and years of unbelievable, unbelievable disaster for Ed Taub. So that's a long story, including indictments that he had to defend against, and losing a lot of money. I mean, he spent a lot of money. Anyway, that was the start of PETA and the start of lots of other animal-rights organizations, which are really raking in the money from the public all over. It is a big story. This was a cause célèbre, and Adrian Morrison was one of the two veterinarians who were brought in by the American Physiological Society, I think, which at that time I guess was part of the Federation for Experimental Research in Biology. APS was one of the organizations within that federation, and they called in Adrian Morrison and the other veterinarian, who were both basic researchers as well, to help defend Ed Taub, and Adrian has been involved from that time until now in __________. So, what I was telling you was that I did help arrange in the end to do some experiments on these animals, who were otherwise just going to die a natural death. They were being cared for at the time when they moved them out of NIH under cloak of darkness.

Farreras: But they were brought here after this break-in?

Mishkin: Yes. There was a period when they were in some animal facility somewhere else, and they were brought to NIH. They were here for several years. That caused misery for NIH because the animal activists would be storming up and down Rockville Pike, blocking traffic, having these vigils at night at the NIH director's office. Finally they were sent down to the Delta Primate Research Center in Louisiana.

It was during that period that I got involved trying to get this research done, and it was based on the work that we had done in our lab. Tim Pons, who had received his Ph.D. with John Tas at Vanderbilt, came to be a post-doc with me, and we did work on plasticity, which grew out of the research that we were doing by chance. If you want me to tell you about it, I can tell you about it fairly quickly.

Farreras: Sure, please.

Mishkin: If you remove the post-central gyrus, which is the primary projection area for touch, the question was…oh boy, this is a very complicated story after all, because it's very closely related to the work that Josephine and I were doing. What we were trying to do was see if we could figure out the sensory processing pathway in touch as I had done earlier in __________. So we had done some anatomical experiments in which we were trying to trace an anatomical pathway that would take us from the primary projection area in the post-central gyrus. We did some anatomical experiments with David Friedman, who was a post-doc in our lab. So we removed the post-central gyrus with the question, what would happen to the next station in the pathway, which was area S2? Let's call the first one S1 – it's not really but we'll call it that for now – to see what would happen to S2. And the way we would study it would be to record neurons in S2 to stimulation of the somatosensory system, in the absence of S1, to see if there was a serial pathway, the idea being that if we took out S1 and S2 depended upon S1, then when we stimulated, let's say, the hand, we would find no neurons in S2 that would respond, even though normally, of course, they do. But the question was, was it serially dependent? And the answer was yes. When we took out the hand area of S1, stimulated the hand, and recorded neurons in what we knew was the hand area of S2, there was no activity for the first month. And then, in the second month, it began to appear that the neurons did respond to stimulation, but not of the hand; stimulation of the foot, which they had never done before. So, this was strange. So what we had found was that when we deprive the system of an early part of the pathway, the later part of the pathway, which depends on the early part, undergoes some kind of plastic reorganization, which was somewhat __________ because there was such a lot of reorganization. It wasn't just a little; it was a lot, in the sense that the foot and leg area took over the entire area that had been hand, was completely filled in in S2. It was at that point that I asked Tim whether he thought we could use the same approach in the Silver Spring monkeys, and he said, "Yes, let's do it." So that's how we got a whole bunch of neuroscientists from around the country together who were specialists – not in the area specifically but plasticity, although John Koz was one and __________. But we had Ted Jones and John Koz and Bob Burke and Peter Strick and someone who had worked with Ed Taub. Anyway, there must have been about eight or 10 of us, and I invited them to NIH for a meeting. This was tricky because we didn't want to use government facilities. This was very touchy because we were concerned about what the repercussions would be if the animal-rights activists got wind of it and went to Congress, who had essentially dictated that nothing more was going to be done. That was by a __________. It wasn't anything like a legislation, but it was an understanding between the director of NIH and Congress that no one would touch them. But it was important, I thought, for scientists to show that these monkeys had not suffered in vain, and that's what would have happened if they had just been allowed to die; whereas if we made use of them in a scientifically legitimate way, it was a big chance because that's science. You don't know what's going to happen. It's easy to look back and say, well, of course, but we didn't know at the time what we were going to find. We might have found nothing, nothing at all. We didn't know. We don't know that our techniques are adequate. We don't know, when we have adequate techniques, what the results will actually show. But at least there was a legitimate hypothesis to test. But that was the argument. It was the moral argument that we had to make use of these monkeys. So we had to go into very delicate negotiations. But we finally convinced Raub that it was a good thing to do. And he had to fight for it, too, I guess. He wasn't the strongest of allies, but he helped. And with the help of the people down at the Delta Primate Research Center, Tim Collins and a colleague who had also studied with John Koz went down to the Delta Center and studied several of the monkeys. And we had to follow certain guidelines, which we didn't like because it didn't permit us to do all of the research which we wanted to do. We wanted to look not just at sensory organization but at motor reorganization. That would have required a much longer period of time. We were given something like four hours per animal. They had to do everything in four hours under anesthesia, and then the animal had to be killed. Well, that was the arrangement. It would not wake up. And it worked. It worked because when they studied S1 neurons – not S2, S1 neurons- the entire arm area had been taken over by the face. So that put neuroplasticity in adults. We knew about neuroplasticity in infants but that put neuroplasticity in adults back in the big-time. And there's been a lot of research on it now following upon that study.

Farreras: Is it common to follow up? You said earlier that there was no neuronal response to stimulation for the first month, that you didn't see any reorganization until the second month.

Mishkin: Right.

Farreras: Is it common to follow up for that long – especially when you first began to work on this, not knowing whether there would even be such plastic reorganization – just to see whether there would be any change in neural response?

Mishkin: Yes. I shouldn't say no. I suppose that it had been done initially with small perturbations, like removal of a digit or part of a digit, or cutting the input to one of the nerves from the hand. And that's what John Collins had done with Mike Merzinick. That work came first. And that has to do with what we did in the monkeys in our lab, looking for S1 to S2 plasticity in the adult. So there was a little precursor in the work of John Koz and Mike Merzinick. Then we did this work in the monkeys here, and that led, in turn, to the work in the monkeys down at Delta, and not just looking at reorganization as a result of damage but reorganization as a result of experience. And now we know that that's happening in all of us all the time. Now we know it happens in every system.

Farreras: When was the Delta experiment done?

Mishkin: Let me look it up. It wasn't that long ago. It was published in *Science* in '91 so it must have been done in '90 or '89.

Farreras: Oh, that recent.

Mishkin: It couldn't have been more than that, I think. And I must say that Ed Taub has made it back since that time. He didn't have anything to do with the experiment, but he was an author on __________ in the __________ because the monkeys were ones that he had done the surgery on. And this goes way back to the time when Joe Broyman was at Yale and Ed Taub was his student, and together they did the afferentations. And, of course, that's what Ed Taub continued studying. The idea was a very important one. It was simply to determine whether the theory was correct that motor function required sensory feedback. That was a very important theory about how the thing worked. And the answer was, it didn't, because he was able to show that you could get motor responses out of the monkey __________ monkey could make motor responses in the absence of feedback. That has turned out to be an important, totally separate issue besides all the work that Ed Taub and many others have now done on this issue of reorganization.

By the way, I should say that the reorganization that takes place after massive injury is not adaptive. It turns out to be maladaptive. This is still a theory, but quite strongly supported, that it is probably responsible for what we know as phantom limb pain, the reorganization. It is possibly responsible for tinnitus, reorganization after damage to the auditory system. And so there is reason to think that one wants to prevent that reorganization from occurring rather than helping it along.

Ed Taub had been involved in a lot of that kind of research, but a lot of people are now looking at the issue of plasticity in adults just due to normal experience, and so there are all of these studies on specialists like violinists and what happens to their motor system using functional imaging; what happens to those who are blind who use their visual cortices to help them read Braille. If they are blinded early enough in life, they can still do this. If they are blinded – and this is something that Leonardo Cohen at NINDS discovered – after puberty, it is not so. The reorganization doesn't take place. So some of this reorganization that takes place early in life is adaptive. Some of the reorganization that takes place later in life is maladaptive.

Ultimately, we're going to understand how to make use of it when we know how to turn cells on and off and when we know how to use stem cells and so on. Ultimately. But right now it's in the early stages.

But remember I was telling you about training the retina? This was the first idea I did with Ed. That training is real. Long practice in sensory skills trains the brain, and we can see it in functional imaging. That is, there's an expansion. Sometimes it's accompanied by expansion, sometimes it's accompanied by contraction, but things change as a result of experience and training. So the brain is much more plastic than we ever imagined it to be.

Anyway, I'm sorry I got off the story, but this is something that we did have a hand in that helped essentially resurrect Ed Taub.

Farreras: Oh, no, this is very interesting!

Mishkin: Another person who was a very strong backer is Neil Miller, who just died. Neil did a lot of things for Ed. One of the things that they had worked on together and separately was biofeedback. And, as I say, __________ Morrison got people involved in this, and he would be a good resource for the history.

Farreras: Yes, I'd like to talk with him. Ben Carlson also mentioned that Neil Miller was being courted to come head the Perception Section. Apparently he asked for all of these resources but I guess Yale matched them and he ended up not coming.

Mishkin: Neil? It's possible. Neil went to Rockefeller from Yale before he went back to Yale. Carl Fastman, a very famous psychologist who was at Rockefeller, recruited Neil. I was there. I was a candidate. I lost out to Neil Miller. But I was a candidate for Rockefeller and I was there. I don't know if you remember or heard about the blackout in New York City that ultimately resulted in an explosion of births nine months later. Did you know about the blackout?

Farreras: I've heard of the blackout; I didn't hear about the explosion of births.

Mishkin: Yes, I was there being interviewed by people at Rockefeller because Carl Fastman had invited me, and I was actually in his apartment at the time of the blackout and he had to leave to go to Rockefeller to see about the hospital that they had some association with. He had to see what was happening with the patients because the whole city was blacked out. But as they say, I lost out to Neil Miller.

Farreras: That's competition!

Mishkin: Right!

Farreras: So what happened to all the Lab's sections?

Mishkin: We just fell one by one; all the Indians fell.

Farreras: Well, the neuropsychology one didn't, that became it's own Lab, but all the non-physiologically based ones did… Morrie Parloff seems to think that by the time he left for his sabbatical in '64 he'd already started to see a decline in the Lab from when it had the largest number of people, 84. The theme I keep hearing is that by '72 NIMH adopted a much more physiological approach to mental health and illness, that all of the early psychoanalysts who rejected psychotropic medications and any type of biochemical treatment were being completely supplanted by this new orientation.

Mishkin: They were, to begin with, psychiatrists, but they were psychiatrists making use of pharmacology without a very good understanding __________. But we didn't have any good understanding at the time of the neurobiology, and they have to go hand in hand, and that's why that group ultimately was supplanted. Fred Goodwin's in the… But __. I didn't have the feeling that Bob Cohen was ever dogmatic about anything. That was at least my impression. He was never a dogmatist. There is another side to that. He was never, on one hand, I thought, very strongly committed to anything either. So I'm not sure that what Morrie Parloff said __________, although he would have had far closer interactions with Bob Cohen than I, and therefore, I might not have been aware of some of the positions that a Bob Cohen might have.

Farreras: Was that psychoanalytic orientation at all noticeable in your section, given that you didn't deal with clinical issues and therefore wouldn't be exposed to such a tradition? You or the Perception and Learning Section, for that matter.

Mishkin: No, they were not psychoanalytically oriented.

Farreras: Right, that's what I assumed. So the shift from a psychoanalytical to a biochemical perspective would probably not have been as patent within your and the Perception and Learning Section…?

Mishkin: No. There was no such orientation to begin with.

Farreras: Did no similar theoretical shifts within your own section take place, whether within the Neuropsychology Section of the Psychology Lab or after you became your own Laboratory of Neuropsychology? I don't want to call it paradigmatic change because it's too strong a word, but…

Mishkin: No. Remember what our antecedents were. They were ___________. And, of course, if psychoanalysts were honest, they would know that was Freud's hope, too. It just never happened, but it was what he wanted. He just didn't think it was possible at that time, not that it wasn't desirable. Later, it became not even an issue. Psychoanalysis was very much like black-box psychology. They went hand in hand, even though the operationalists among the black-box psychologists couldn't stand the metaphors. They weren't theories, according to them; they were metaphors, hypothetical constructs and intervening variables. That was not for them. But they nevertheless shared the notion that what went on inside the brain was locked from view and therefore best not to worry about it. Input and output somehow would be enough. Even Skinner would say something like that centuries from now maybe. So the foolhardy among us pushed __________. It's really interesting because there are still some hardcore cognitive scientists who don't want to have anything to do with the brain. But little by little, that, too, has shifted. And more and more cognitive scientists are making bridges. It's doable now. It's possible to look, in a way, inside the brain. __________ except with EEG or ERP, and that was really very difficult because it was so unlocalizable, what the source of these waves were. But that's changing too, because little by little there's going to be a marriage between electrophysiological recording and blood-flow imaging and metabolic imaging. There's a lot of __________ because there are virtues that I've indicated. There are so many mistakes that one makes by looking at just one technique. So we need the temporal resolution that's possible with electrophysiological recording and the spatial resolution that's possible with __________ imaging, and they have to be brought together, and they will be. It is really the case that technology really runs the show, but that sounds like it gets short shrift to ideas, and, of course, they also run the show.

Farreras: Well, it doesn't seem to shortchange the questions that that technology can answer but rather that it ignores the questions that that technology can't answer, what it's leaving out…

Mishkin: Would you like some coffee?

Farreras: Actually, it's five. I should let you go. We can meet again sometime when you get back from London. Thanks again for all of your time.