Mishkin, Mortimer 2001 A

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Dr. Mortimer Mishkin

This is an interview with Dr. Mortimer Mishkin, chief of the Cognitive Neuroscience Section of the Neuropsychology Laboratory of the NIMH Intramural Research Program held on November 6th, 2001, in Bethesda, Maryland.

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

Farreras: I thought I would ask you first to introduce yourself, e.g., where and when you were born and some of your family background.

Mishkin: My full name is Mortimer Mishkin. I was born in Fitchburg, Massachusetts, on December 13, 1926. I have one brother who's older and one sister who's younger by about the same number of years, five or six. I can tell you all about my family, but I am not sure which part is particularly relevant, except that my parents were always strongly supportive.

I went to grammar school and high school in Fitchburg. I graduated from Fitchburg High School in 1944, but I left a semester before graduation to enter the Navy Officer Training Program, which brought me initially to Middlebury College in Vermont, where I spent one year.

This was a very quick transit time through college because we went three semesters a year, so that's like one and a half years every year, in order to get through as quickly as possible. Initially, I joined to become a pilot in the V5 program but at the end of 1944 they had enough pilots and instead they needed deck engineers and supply corps officers in the V12 program. Because of my interest in what I understood at that time to be social sciences, I decided to go into the Supply Corps, because that would give me the opportunity to take some courses in liberal arts and social sciences. So I was transferred to Dartmouth the second year.

Farreras: So *you* didn't pick Middlebury or Dartmouth.

Mishkin: No, no. They were picked for me by Uncle Sam.

Farreras: Good schools to have picked.

Mishkin: Yes. I completed a bachelor's degree in business administration at Dartmouth, at the Amos Tuck School, because this was what was needed for Supply Corps training. That was my background. I had only taken two courses in psychology and this is significant because this is why I finally ended up where I did, in McGill.

I had one course in mental hygiene. It was given by a wonderful young scholar named Shammai Feldman. And the other course was in international relations. I

say psychology, but that's what it was, international relations. And it was given by someone who would become a very well-known industrial psychologist who had done a lot of work in personality, Ross Stagner. I mention them because they turned out to be instrumental in getting me into graduate school in psychology. It was only with their help that I was able to do it. No one in their right mind would have accepted someone into graduate school in psychology with my psychology background. Anyway, I graduated from Dartmouth.

Farreras: In '46?

Mishkin: Forty-six. I went immediately to Officer Training School for the Supply Corps at the naval base in Bayonne, New Jersey, which lasted a couple of months, and then I was sent over to Japan as part of the occupation forces. So I flew to Japan. I was scared as hell because it was very soon after the war and with all the propaganda that we had heard we were really scared. But we flew into Tokyo and we were dispersed among the various places that needed us. I went on a destroyer tender in Yakosuka Harbor. And my job was to look over the manifests of the ships bringing in oil from the Arabian Sea, which was then emptied into the big tanks that they had in the harbor. That was my major job as a member of the Supply Corps. I was also a savings bond officer aboard the ship. But because I was an officer I also had a jeep and was able to get onto shore often and that was very good because I actually mixed with Japanese in Kamakura, a little place a few miles north of Yakosuka Harbor, where they had a beach house in which there was dancing every weekend. They played American music and I joined in because I had played the saxophone and clarinet in school and was able to play with the band. So that was a lot of fun. But I was the only American, all the rest were Japanese. And so I met a lot of Japanese and became fast friends with some of them. That's how I spent my nine months or so in Japan, in my Navy Supply Corps uniform, dancing and playing music.

When I returned from Japan in the spring of '47 I spent the summer near my home in a little town called Gardiner, where they had a state mental institution. I think it was called an insane asylum at the time. I did that because of my interest in psychology, despite the fact that I didn't have any background in it. I was interested in the area of psychology because in high school I had actually steeped myself in Freud. I found it so fascinating. So I knew I wanted to go into psychology. And during that summer I also went up to Dartmouth to visit my professors, Shammai Feldman and Ross Stagner, and they agreed to write letters of recommendation for me. Despite their letters of recommendation, there were very few places that would accept me because I was sort of ignorant, and yet I applied to all the Ivy League schools.

Farreras: In what program?

Mishkin: Psychology.

Farreras: But which track? Didn't they have specialties at the time?

Mishkin: Oh, I see. I knew I wanted to go into social psychology because that was the way to save the world. It was either politics – I was very left-leaning, obviously – or economics, and I chose psychology, thinking that politics and economics were a question of psychology.

Farreras: So you applied to every Ivy League school.

Mishkin: And I think I actually did get accepted to one or two places as well as McGill but I understood that McGill had a fine school so I went there. And I initially went to the chairman, Robert MacLeod, who was a phenomenologist but also a social psychologist well versed in the work of people like Kurt Lewin and the social psychology and the gestaltists of that time. He later became chairman of the department at Cornell. So he was chairman and I went to him because he was the one whom I would have to convince to let me study social psychology. And he said, "What are you doing here?"

Farreras: After they had accepted you?!

Mishkin: Yes! "How come we accepted you?" I think that by then he had had a peek at the record rather than the letters. And he said, "What I suggest is that you do a qualifying year, and during that qualifying year, I suggest that you go down the hall and meet with this new instructor we have who has just came up from Orange Park, Florida, so that you can study experimental psychology with him. When you become a good experimental psychologist, you come back to me."

Farreras: Get the basics first.

Mishkin: Yes. And so I went down the hall and spoke to this new instructor, who happened to be D[onald] O[lding] Hebb, who had just come from Orange Park, Florida, where he had worked with [Karl] Lashley. So Hebb was newly there, as was another even younger person who'd just gotten his Ph.D., [Haldor Enger] Rosvold, who was teaching a course in personality. I became Rosvold's teaching assistant. In any event, I did my master's thesis with Hebb, and my master's thesis was probably the most well-known thing that I've ever done – it was well known then, it's not well known now. But it sort of started a lot of people off on tachistoscopic recognition experiments, looking at the difference between left and right visual-field perception. The whole idea was Hebb's, not mine; I did it under his direction with a colleague student, Don Forgays, and it was one of my first publications. And I was very proud of it because it was included in Hebb's book *The Organization of Behavior*.

Hebb was perhaps my most influential mentor ever, not because I had a lot of direct interaction with him, but because I was a student in his lab, where I learned how to make equipment, including the tachistoscope used in a pilot study for this research. It was nothing but two slabs of wood, hinged at the bottom and attached at the top by strings so that when the first one fell it would bring the other one after it, but it would open just enough so that something on the second panel could be seen for a fraction of a second. This was the kind of equipment we had at the time. We made our own. Incredible. Hebb had done a lot of work on animals at Orange Park. So we made our own mazes, our own rat cages. It was really quite remarkable. But apart from the lab work, including the anatomical study of the rat brain, of greatest importance was his book, because he had just been writing all this and it was still in manuscript form. And we had seminars on it chapter by chapter. And as unschooled as we were, because after all we were all youngsters in this seminar, I think everyone recognized how influential the book was. And, of course, it was the most significant work in years in psychology because it showed for the first time how it was possible to think in neural connection terms, how the brain might work and how thoughts might occur. Incredible. Until then, what we had to go on was either the black box, as we called it, which was championed by most of the behaviorists, or physiological psychology in which the models were field theory, and that was true of almost every model at the level of higher brain function, starting with [Ivan] Pavlov's, including the Gestaltists like [Wolfgang] Köhler, and the foremost physiological psychologist of all, Lashley. They were all field theories. They paid lip service, in a sense, to neurons. Hebb was the first to have a theory of how the brain worked that involved connections among neurons. Incredible.

So among the graduate students in Hebb's seminar, Herb Lansdell – who later left McGill to join the National Institute of Neurological Diseases and Blindness –, Peter Milner, Brenda Milner, Sam Rabinovitch, and Lila Ghent, who later became Lila Ghent-Braine. There must have been about 12 of us. I mention them because so many of them really went on to do important things. And this was just a first group, because he had many afterward and many who also became key figures in neuropsychology. So I got my Master's degree in 1949, with Hebb, on the tachistoscopic experiment of left versus right visual field recognition of words.

> And it had some physiological implications for brain function but it really wasn't looking at brain function, it was trying to infer brain functions from perceptual accuracy. What we had found was that there was better recognition in the right visual field than in the left for English and we got a result that was in the opposite direction in Yiddish, suggesting that there was something about the reading experience of going from one end of the line to the other that was actually training the retina differentially.
>
> And it wasn't just training the retina, it was training the retina and the hemisphere to which the hemi-retina was projecting, so that with the successive presentation of stimuli in either the left or the right visual field, you could "see better" with your right hemisphere if it was English and with your left hemisphere if it was Yiddish. That issue became so complicated…it's unbelievable but there were lots and lots of experiments that were done after that in this field to try to understand what was going on. I'm not sure all of the factors that go into this have ever been disentangled. But as I said, I wasn't actually doing any brain research for that study, and I wanted to do brain research.

At the time, Rosvold, who was this new assistant professor at McGill, had just received his Ph.D. from Stanford, where he had worked with Calvin Stone. I think Stone had been one of the early editors of the Journal of Comparative and Physiological Psychology, which has since changed its name. When he died, in fact, Hal Rosvold received from his wife his collection of reprints, so that sort of started off our reprint collection.

Farreras: You showed me, yes.

Mishkin: All of his files. There was a time when that was how we collected the literature, really collected it. And, of course, I still do. But Hal Rosvold got me to help him do some research at McGill that did involve brain function. This was my first paper with Hal, in fact, and it looked at the effects of frontal lobotomy in war veterans who had become schizophrenic and were later lobotomized. At that time that was one of the main "treatments". They were living incarcerated at St. Anne's Hospital in Montréal and because they were war veterans and had been tested at induction, we were able to compare their IQs then with their IQs before. This had never been possible before. We were able to show, we thought, that the frontal lobotomy was actually producing cognitive loss, IQ loss, and that, therefore, it had a deleterious effect. Of course it did, but we couldn't pick up the

deleterious effect with IQ very well, although we had a couple of other measures, such as a form of delayed response or alternation, which was somewhat more revealing of impairment – except we didn't have really good controls. The study was published in 1950, I think, and it was important to me because this is what got Hal Rosvold involved in something in which I joined him.

In 1949, after I had finished my Master's degree, Hal Rosvold was invited to go to Yale to join what became the Yale Lobotomy Project – supported by the Veterans Administration – to look at two issues: the effect of lobotomy on patients with schizophrenia and to study frontal-lobe function in animals. And this was done at Yale where the first reported work had been done on apes, by the psychologist [Carlyle] Jacobson working with the physiologist John Fulton. They had studied Becky and Lucy, two chimpanzees, from whom they had removed the frontal lobes in a study of delayed response. And this is what led, not just to an understanding of the importance of the frontal lobes for delayed response function, which was the point of the study, but rather, it led to the Nobel Prize being given to Egas Moniz. He had been searching for what to do with hundreds of psychotic individuals in his hospital in Portugal and when he learned that one of the Jacobson and Fulton's chimpanzees, who had had temper tantrums when he failed the delayed response test prior to surgery, no longer had temper tantrums, he had this "ah hah" experience. That is what led ultimately to frontal lobotomy, although I guess it wasn't frontal lobotomy but rather leukotomy. So historically, the work which Jacobson and Fulton did was very important but it was deemed so initially for the wrong reason. It was, however, a critical step in trying to understand frontal lobe function from a cognitive standpoint.

So Rosvold was invited down to Yale to join a large group of investigators who were converging on Yale at the time.

Farreras: Was Donald Marquis there?

Mishkin: No, he had been there but had already left for Michigan. I do not think he was associated with the physiology department. What Yale was most known for, for me, was [Clark L.] Hull and the school of behaviorism that he created there. Another central figure in psychology was Frank Beach. He had studied with Lashley and was doing a lot of research on sexual behavior in dogs in the psychology department. Neal Miller was also at Yale; I was fortunate enough to be able to take a course with each of them (Miller and Beach) when I was there, having joined Rosvold. But let me go back to the story because the Veterans Administration was providing support for two programs now, one based on John Fulton's physiology department, and the other based in the psychiatry department headed Fritz Redlich.

Farreras: So the study of the effects of the lobotomy on the schizophrenic patients was done in the psychiatry department and the physiology department was in charge of the basic research on the frontal lobe functions in animals?

Mishkin: Yes. I think the plan was to study about 12 well-selected patients along with 12 control subjects.

Farreras: You mean schizophrenics who hadn't gone through a lobotomy?

Mishkin: Yes, as far as I can remember. I am not sure about the controls because it may not have been deemed ethical not to provide this kind of relief from their illness. But it was recognized at the time that there were problems with the treatment so people were no longer going into it blindly.

Farreras: So it wasn't to test the efficacy of lobotomy?

Mishkin: It was in part, yes, as well as to try to understand what it was doing. But I had very little to do with that part of the research because I really started feeling uncomfortable with it, as I thought it was not possible to study frontal lobe function properly with that kind of surgery where one doesn't really know what one is doing. I was a hard-nosed experimental psychologist at the time. I thought you had to have the control you have with animals.

So Rosvold invited me to go with him to Yale and Hebb okayed it. I was still getting my degree from McGill, but Hebb let me go with Rosvold so that I could study brain function in non-human primates. And that is what I did for my thesis, I studied monkeys and baboons. And I was really fortunate because at Yale, also coming from Orange Park, Florida, was Karl Pribram. Karl Pribram had a different background from everybody else who studied with Lashley. Karl Pribram was a neurosurgeon. He had been trained in neurosurgery at Chicago by Paul Bucy and he had studied, in addition, with people that Bucy worked with: [Gerhardt] von Bonin, [Percival] Bailey, [Warren] McCulloch, and Heinrich Klüver. Klüver and Bucy were the ones who studied the effects of temporal lobectomy in monkeys and Pribram was aware of it, was probably there while some of it was going on. Because of his knowledge of surgery, anatomy, physiology, strychnine neuronography – the technique that was used at the time by McCullough, von Bonin, and Bailey to work out the connections of the brain in the rhesus monkey and the chimp – and because of his interest in behavior, Karl went to Florida specifically to do his residency in Jacksonville, so he could be close to Orange Park and work with Lashley. And there he met all of the other really outstanding people in the field at the time and who became even more so later: Hebb, Roger Sperry, Frank Beach, [Henry] Nissen, and a number of younger people. I say younger but they were probably the same age as Karl, who had just begun his internship. Kao-Liang Chow, Josephine Semmes, Ed Evarts, Robert Blum. Lashley had an amazing group. An interesting sidelight is that Hebb later asked Lashley to co-author his book with him and Lashley refused because it was contrary to his views on how the brain works. Hebb believed that Lashley held physiological psychology and cognitive neuroscience back by a quarter of a century because of his concepts of mass action and equipotentiality. It allowed the black-box psychologists to argue that the brain could be viewed as a bowl of porridge, no part different from any other part. That, of course, is not

what Lashley meant. Lashley was brilliant and I suspect that, in time, people will go back to Lashley and understand that there's a constant dynamic interplay among huge ensembles that has connections among neurons as its basis. We will never understand brain function without understanding both points of view, and that will happen in time. Localization theory is absolutely essential to understand the basics. But there are other basics in a dynamically active brain that also have to be understood and this concerns the short- and long-distance interactions that are constantly occurring among all the parts of the brain. And although we need to know the underpinnings and the way the circuitry is arranged, knowing the circuitry is not enough, as there are going to be principles of dynamic interaction that we will still have to understand. So we will probably come back to Lashley but in a different way. And in that sense Hebb himself was leaning in that direction. The reason that Hebb is so central to neuroscience, as distinguished from psychology, is because of his one postulate in his book, that led to the notion of the rules underlying long-term potentiation and long-term depression; that is, how synapses are modified. That was the postulate. Synapses are modified as a result of one neuron being successful in depolarizing and firing a second neuron. The more it does so, the more effective it becomes because of a metabolic change in one or the other. That is what made him famous among neuroscientists. But he formulated that postulate in order to understand cell assemblies, which he saw as the basis of perception, and phase sequences, the term he gave to a still higher-order organization, which he postulated underpinned the thought process. But phase sequences is getting awfully close to field theory, so in the end there's going to be some kind of marriage.

Where did I get to in my story at Yale?

Farreras: Karl Pribram has just arrived.

Mishkin: Right. Karl Pribram came up from Orange Park and taught Hal Rosvold, Al Mirsky, and me anatomy and neurosurgery and some principles of behavioral testing in animals that he had garnered from his stay at Orange Park, Florida, with Lashley and others. And Fulton had put together a great group of people. I was telling you about the group that was down at Orange Park which I was not a part of but benefited from by interacting with so many of the people who came from there. But Fulton was also successful in bringing together many, many talented people: not just Karl Pribram but also Hal Rosvold, Paul MacLean, Robert Livingston, [James] Stevenson (a physiologist from Canada), Pat Wall (a well known pain researcher), BK Anand and [John] Brobeck (two pioneers in the field of hypothalamic feeding and satiety function), José Delgado (who at least was momentarily famous in his work on stimulation of the caudate nucleus; he demonstrated that he could stop a bull in mid-charge, which was a neat trick), Lawrence Krüger (who did a lot of important work in anatomy) and Joseph

Berman, who later moved from Yale to New York where, with his student, Ed Taub, he studied the effects of sensory deafferentation on motor function. That work has a long history. Maybe we'll have a chance to talk about it.

Farreras: Sure.

Mishkin: So there was a large group of people that got together to work on frontal lobe function. That was the theme, but of course it didn't stay focused entirely on frontal lobe function, it was brain function. I remained there for two years. I did my thesis under the direction of both Hal Rosvold, with whom I had gone to Yale, and Karl Pribram, who was my really important tutor in that he taught me primate anatomy, neuroanatomy, neurosurgery, and how to work with monkeys, behaviorally. Although I did frontal lobe work at Yale in collaboration with these people, I got my Ph.D. from McGill for my work at Yale on the temporal lobe, a follow-up of some studies that Pribram had done at Orange Park with Chow and Josephine Semmes.

Farreras: Josephine Semmes, who then came to NIH?

Mishkin: Yes, as did Evarts. He came first and she, his wife, came later and joined our Animal Behavior Section. He had his own Lab of Neurophysiology although before that he was part of Kety's Laboratory of Clinical Sciences. After Yale I went with Pribram to the Institute of Living at Hartford, Connecticut.

Farreras: Does it not have a different name today, the Hartford Retreat or something?

Mishkin: It may.

It was famous because it was a private institution for wealthy patients with mental disorders who could afford to pay. And the Institute of Living then decided to set up a research laboratory. It was called the Burlingame Research Laboratory; Burlingame after the psychiatrist who began the Institute, although I am not certain. I believe that research laboratory was, in part, a tax haven for the institute. It's hard to believe how, otherwise, a private hospital of that kind could have set up a research laboratory that involved basic studies on brain function in animals.

Farreras: So Pribram was not at Yale for that long, either, then.

Mishkin: No. He was there for two years, too. He might have arrived a few months before I did but not much before. So I went with Karl to the Institute of Living in Hartford where we set up a primate facility. And we were joined by a lot of young people. I was also young but I already had my Ph.D. and there were some Ph.D. students who joined us, some of whom have become very important. Larry Weiskrantz was one of the students who got his Ph.D. from Harvard for the work he did at Hartford, much like I had gotten mine from McGill for the work I did at Yale. The thesis that Larry Weistkrantz did with Karl was on amygdalectomy and its effect on avoidance conditioning, which led to the recognition of the amygdala as key in mediating fear reaction in monkeys. But

there were many others who also became well known figures: George Ettlinger, John Stamm, and William and Martha Wilson. I stayed there for four years, and it was at that time, in '54, that Hal was recruited by David Shakow to join the NIH in the NIMH's Laboratory of Psychology.

Farreras: So it *was* Shakow by then? Al Mirsky told me his letter was signed by Dick Bell. Was he Acting Chief before Shakow arrived?

Mishkin: Yes. And both Hal and Al came a few months before I did. So Hal recruited Al and me, his students from Yale essentially. Al had gotten his Ph.D. with Karl and Hal as well. So we both joined Hal here in the Laboratory of Psychology, in the Section on Animal Behavior.

Farreras: Do you know why Shakow chose Hal to head that Section?

Mishkin: I suspect by writing around and asking who was good, but I'm not sure. I really have no good idea. It's not anything that was ever discussed with me.

Farreras: And that first year, too, according to the Scientific Directory, the Lab was called the Laboratory of Clinical, Experimental and Developmental Psychology.

Mishkin: That may have been.

Farreras: Who came up with that title, and why did they change it to the Lab of Psychology the following year?

Mishkin: I don't know.

Farreras: Alright, I had been curious about that.

Mishkin: It could have been Bell if he was the Acting Lab Chief.

Farreras: But someone needed to have appointed him and I am wondering why him in particular.

Mishkin: I'm not sure of the answer to that either. I could make some guess that he was thought to be an upcoming star, scientifically rigorous…but I don't know.

Farreras: OK.

Mishkin: There's one person whom you may not have on your list to talk to but who might be able to tell you a little bit about what went on in one of the sections early on. Donald Blough. Don is at Brown, he's been at Brown ever since he left NIH.

Farreras: Yes, I've written to him already.

Mishkin: Okay. He is wonderful. He's a really fine experimentalist. He's worked with pigeons all his life, as far as I know.

Farreras: He was in the Perception and Learning Section.

Mishkin: Right.

Farreras: But I have that he was only here between 1954-1958, working with Virgil (Ben) Carlson and Jack Calhoun.

Mishkin: Carlson and Calhoun, right. I wonder if Carlson is still around.

Farreras: Al Mirsky mentioned he might be living in Fells Point, in Baltimore.

Mishkin: Maybe. So I interacted with the people in the Perception and Learning section more than with the people in the other sections, because they were not

experimental, or at least not in the same way.

Farreras: Plus there was that division between the basic and the clinical sections, too.

Mishkin: Right. So I arrived in '55. And I think Al and Hal in '54.

Farreras: Right. Hal in August and Al in October of '54.

Mishkin: Okay. So I came in '55. I don't remember if it was April or June. Initially we were over in the Clinical Center, Building 10, and we had some space in the animal facility, which was in Building 13, and that is where we did our initial research, before we were able to move into our laboratory space in Building 9.

Farreras: When did that happen?

Mishkin: That must have been the next year, '56.

Farreras: Were the other sections in T6?

Mishkin: Most of the sections were in the Clinical Center. There may have been a section in T-6, but I'm not sure. I think some of the administration was in T-6. So that's the beginning of the work at NIMH in 1954-55.

Farreras: Was there any sort of collaboration between the psychology sections and the other NIMH labs?

Mishkin: I shouldn't say that there was none, but I think that, at least at that time, we did not have any ongoing collaborations.

Farreras: What about relationships between the Psychology Lab and the three that had been established before it: Neurophysiology, Neurochemistry, and Socio-Environmental Studies? Were there any?

Mishkin: No. We [Animal Behavior Section] did not have very much interaction with the Social-Environmental Studies Lab. I think Al might have had a little because he was doing social interaction studies in monkeys. We did have a little interaction early on with people in Neurophysiology, mainly with Ed Evarts. And I'm not sure how early that actually was. It may not have been until the late '50s, when I started interacting with Ed Evarts to learn about some recording techniques in sleep studies on cats.

Which reminds me that in the neighborhood of the NIH, besides the incredible number of fine people here, were the people over at the Walter Reed Army Medical Institute who had been brought together by Ted Rioch. He had brought Walle Nauta, a renowned neuroanatomist, who later joined Hans-Lukas Teuber at MIT. Teuber recruited him into the psychology department, which was something unheard of at the time: an anatomist in a psychology department? Robert Galambos was there at Walter Reed and a lot of well known behaviorists were there, Joseph Brady, Murray Sidman, Charles Ferster...

Also, Ronald Myers, who studied with Roger Sperry. But the most important of

all was David Hubel, from the point of view of what happened later. So I had

done some electrophysiological experiments with Ed Evarts. They never came to fruition but I learned a bit about recording in cats. And this is what David Hubel was doing at the same time at Walter Reed, looking at the neurophysiology of sleep in cats.

Farreras: Were Evarts and Hubel working together or were they independent…?

Mishkin: Independent, but they visited each other. I remember Ed Evarts and I visiting David Hubel's lab on a couple of occasions. One thing I forgot to mention: when I was at Hartford, I didn't totally give up my research on humans. Karl arranged for me to spend two days every couple of weeks at NYU-Bellevue Medical Center. And this is where Teuber was working with [Morris] Bender.

They had both been in the U.S. Army Medical Corps together during the war and after the war they remained together studying brain-injured war veterans. When I was at Hartford I would commute biweekly to New York by car or train. I did it for three or four years. Luke Teuber had a lot of young people with him who later became well known, including Josephine Semmes, who was there because Ed Evarts was doing a residency, probably in neurology at Cornell, I'm not sure. Lila Ghent-Braine, who had been at McGill with me. Sid Weinstein. Stan Battersby. There are a couple of monographs on visual and somatosensory defects co-authored by Teuber and several of these people. Luke not only had the group at NYU, but he later moved to MIT and brought in a number of excellent people to help train the next generation of neuropsychologists. This was pivotal to turning neuropsychology into behavioral and cognitive neuroscience. He was also a walking encyclopedia…he had an encyclopedic mind. There was nothing he did not know; and it was not superficial. He was not a great experimentalist, he was not a great theoretician like Hebb, but he was a fantastic teacher.

Farreras: What were you working on there?

Mishkin: I was looking at the effects of frontal lobe lesions caused by penetrating brain wounds in war veterans on some rather strange phenomenon having to do with vision, posture, and equilibrium. The subject sat in this tilting chair and we looked at the effects of tilt on perception of the vertical in the light and in the dark. The chair was a mammoth piece of furniture.

Farreras: Your comment about turning neuropsychology into neuroscience made me think of your Section in the Psychology Lab. Why was it called Animal Behavior and not Neuropsychology from the start?

Mishkin: I'm sure there was a reason; it couldn't have been random selection. And it couldn't have been because no one else was working on animals. We just mentioned Don Blough, Jack Calhoun… Blough did experiments on pigeons and Calhoun was looking at rats or mice. Those three sections – Aging, Perception and Learning, and Animal Behavior – were sort of in a cabal because we always felt as though we weren't being treated properly by the clinical psychologists. God knows why. We were trying to protect our turf and so we would mutter a lot to each other.

Farreras: Didn't the Aging Section already exist prior to the Lab being formed?

Mishkin: That may well be. I think Nathan Shock may have started it.

Farreras: I thought James Birren headed the Aging Section…?

Mishkin: Yes, I think Birren was Shock's student or associate. You might want to check if that's correct.

Farreras: I will. Do you know why it was taken in by the Psychology Lab?

Mishkin: I suppose it was necessary to find a home for groups like that. I don't know if there were any others already in existence.

Farreras: I hear Kety really pushed for it but what was the rationale for establishing this Psychology Lab in the first place? It *was* post-World War II and there *were* a lot of veterans returning…but the emphasis in the intramural program here wasn't so much on mental health treatment…

Mishkin: It was in the clinical area. I shouldn't say that in a blanket way but Dave Shakow's interest was in treatment. He was a true clinician. But he was also interested in making it as experimental as possible and therefore he did experiments, including having interviews videotaped.

Farreras: Yes, he taped an entire psychoanalysis.

Mishkin: Yes, and examined it to see what was going on.

Farreras: Yes, a very complicated and expensive process only to have it thrown away later.

Mishkin: I don't know.

Farreras: Now that you've pointed it out, was the division between basic and clinical there from the start of the Lab?

Mishkin: What do you mean by "division"?

Farreras: There was an organizational division where Basic Research consisted of laboratories like Neurochemistry, that only had basic sections, and Clinical Investigations consisted of branches like the Adult Psychiatry Branch that only had clinical sections. The Psychology Lab had sections in both: Aging, Animal Behavior, and Perception and Learning belonging to the Basic Research Division and Personality, Child Development and the Section of the Chief belonging to the Clinical Division.

Mishkin: Yes.

Farreras: Could you tell me a little bit about whether there were boundaries that clearly demarcated them back then?

Mishkin: It's necessary to understand that, at this time, when we were just starting out, there were questions about whether, because we were a federal institution, Congress was going to dictate what we did. That was the main issue for us. Were we going to be told, as scientists, what to do? And because the National Institutes of Health were organized around diseases – because this is how the public provides its support based upon its knowledge of disease entities – we needed to mount all this research effort to fight disease.

Scientists needed to make clear that direct attacks were rarely successful, and that ultimate success would depend on conducting a lot of basic research.

Farreras: So the basic scientists would not be made to feel as if they had to conduct clinically-relevant research…?

Mishkin: That was probably at the root of the concern and of any tension that there may have been – and there was some – between clinical and basic. The clinical people probably felt secure; they were doing, essentially, the nation's work, or at least that was, I am sure, what many of them felt. And we, the basic scientists, had a

somewhat different view of things: that there was no way to do the nation's work at this stage of our knowledge – at least in our institute. We had to understand brain and behavior first. There was no way we were going to understand these things based on direct clinical interventions into mental disorders. And I think that we were proved right.

Farreras: Would you say that the scientists in the basic sections were allowed to pursue their own research interests?

Mishkin: Yes, absolutely.

Farreras: So they weren't told, "there's a pressing need to know more about X so this is what you need to research right now."

Mishkin: There was no time that I can recall when any scientist I am familiar with was told what to do. These were all investigator-initiated research projects. It was fantastic.

Farreras: Was there any research that was done because there was money appropriated for it?

Mishkin: Yes, that could happen.

Farreras: Can you think of any examples? Al Mirsky had mentioned to me that Bob Felix was given something like $6 million to do work on mental retardation and they had to brainstorm experiments they could do to spend that money.

Mishkin: Yes, that could happen. That's a bribe, though; it's not the same as dictating.

Farreras: In the sense that at least there's funding to do certain research?

Mishkin: But no matter how much money is put into a particular project, it is taking it away from other pockets because there's only so much money. So, it really is not a good idea from a scientific point of view. And yet you can't say that too strongly because sometimes the research that gets started for the wrong reason comes out with significant answers that would not have been found otherwise. That's science. One likes to think that the research monies should all go to people who have good ideas, the goodness of them being judged in the only way that we know how, by peer review. It's not good. It's just that there's no better way that we know of. But peer review is awful. It is awful.

Farreras: At the hiring level, would you say it was scientific excellence or the promise of social relevance that was being used as the criterion to hire people who became part of the Psychology Lab?

Mishkin: There was always a little bit of both. It's hard to say. Within a particular lab, one might say, as we usually did, that we weren't looking for any particular kind of scientist, we were looking for the best person available. But the labs that got set up were set up presumably because of the felt need to do research in that area, so it had to be both. There's no way to say that it could be one or the other. Sometimes one is more important, sometimes it's the other, but in the end there is always a compromise between both needs: for something relevant in the sense of

some felt need to have a theme pursued and for the best person, independent of relevance. It's only a question of how narrowly or broadly one determines the category in which the best persons fall. If it's very narrow, that means that there is a big effect of the felt need for research in a particular area. And if it's very broad, then, one assumes that scientific excellence is paramount. The change that has taken place over the last decade at NIH is a measure of how important the felt need to pursue a theme can be, because molecular biology blossomed in this period, and the need was to bring in people who were good at it. It couldn't be just anybody. It had to be people who made use of molecular biology. And this has, of course, happened all over the country and all over the world, but it's just one example of the way in which felt need can be critical in the selection of scientists.

Farreras: Would the funding that was available in the Psychology Lab go toward hiring new people who were considered good for the Lab, or would it go to established scientists already in the Lab who had ongoing research programs?

Mishkin: In the early development of the Laboratory of Psychology and the parts of it that I know, people were brought in in the mid '50s so that we filled up all potential slots. That, of course, created a problem because everybody who was brought in was not necessarily great. Some left who were really good. Others who were not so good stayed, and it caused a problem in the later years because there was no place to bring in new people.

Farreras: Who was in charge of the hiring? Was it always Shakow or was it someone above Shakow?

Mishkin: The hiring within sections? No, not Shakow, the Section Chiefs. Dave Shakow never interfered. As far as I remember the Section Chiefs determined who would be in their sections. Shakow may have had to okay it but I bet that he felt that he would have to depend upon the judgment of the Section Chief. Why else have a Section Chief?

There's one interesting story about Shakow I want to tell you before closing. One thing that Shakow did, which was really great for the Lab, was hold a wonderful picnic supper at his home in Bethesda every year to which he invited all of the Lab scientists. He and his wife, Sophie, were wonderful hosts. And I only mention that as an introduction to what I wanted to say next, which is that in the late '50s, in '58, we were visited for the first time by Jerzy Konorski from the Nencki Institute of Experimental Biology in Warsaw. He knew about our work but we, or at least I, had never heard of him.

Farreras: You mean your work within the Animal Behavior Section.

Mishkin: Yes. He was doing frontal lobe studies in dogs and he knew all about the Western literature but we didn't know anything about the Eastern European literature. So it was tremendously exciting to know that there were people there doing this kind of research. I sort of gravitated to Konorski and he invited me to come to Poland, which I did in early 1959 for three or four months. And since that time I've had very close interactions with the people from the Nencki Institute.

But the story is that Dave Shakow and Sophie, before I left, had my wife and me,

Hal and Mary Rosvold, Bob Livingston – who at that time was the Scientific Director of NIMH – and maybe a couple of other people to his home for dinner to wish me a good trip. That was the main purpose of the dinner. Dave Shakow was about the same size as Jerzy Konorski, which is short, and I was leaving in the dead of winter for Poland. And I don't know how it came up in conversation at the dinner table, but I didn't have a winter coat. So Dave Shakow gave me his winter coat.

Farreras: To take to Poland with you?!

Mishkin: Yes! And I took it, and it was wonderful. I couldn't have survived the winter without it. This was just a couple of nights before I left and there was no time to get a tailor. I had my arms sticking way out and my legs sticking out… But still, it was very helpful!

I'll tell you a little bit more later.

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