

Dr. Loring Ingraham
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This is an interview with Dr. Loring Ingraham, full professor of clinical psychology at George Washington University and former member of the Laboratory of Psychology and Psychopathology of the NIMH Intramural Program. The interview is being held in Germantown, Maryland. The interviewer is Dr. Ingrid Farreras of the Office of NIH History.

[Tape recorder did not record the introduction to the oral history]

Farreras: So you mentioned you were just finishing up your dissertation at Catholic University, you had been working with Betsy Parker in the Alcohol Institute, which was in close proximity to the lab, and that's when Seymour Kety, after he retired from Harvard, came down.

Ingraham: Right, he came down to work in the lab, and one of the things we talked about was that it was a natural fit for him because he'd been working with Dave Rosenthal on the Danish studies for a number of years and was very familiar with the lab from the very start. And I was very excited to see him come down because I'd been interested in schizophrenia and had been doing schizophrenia-related clinical work and was excited at the chance to talk about such research.

So I had just come back from my internship up at Harvard and needed to support myself, so I got a job as a research assistant in the Alcohol Institute, and right around the time I graduated, he needed a research assistant, and the person whom he had thought was going to be able to come work with him wasn't able to, so it was a very nice match. And just then they were beginning to switch the data over from doing everything on punch cards and what-not to more contemporary ways of doing things, and that was something I was familiar with, so that was a great match.

In the spring of '85 or so, I started working as a staff fellow with Seymour. I think the first room in the building I was in was 4C205. That's where I was with Betsy Parker. And then I moved down to 4C210, toward the end of that same hallway, and Seymour and I shared a module there carved up, so between the two of us, we actually had a module and a half. It was a strange L-shaped configuration, but it was a nice place to get set up. So that's how we started.

Farreras: Who else was left in the original lab when you arrived in Spring '85?

Ingraham: Well, Gene Tassone was a research assistant, had been a research assistant from pretty much the beginning, but he retired around '90 or early '90s. He was really the institutional memory as much as anyone in the lab at that time, and he had been working with Rosenthal for years and had even been there with Shakow. He lived in an apartment right close by the campus, and he'd seen the institutional memory. He's the one who really would know a lot. Ted Zahn had been there for a long time, and he was there. And that was pretty much it at that time. Herb Weingartner was just leaving; he wasn't really there.

Farreras: I wasn't sure when he had actually left.

Ingraham: He was in and out because he was consulting to the Alcohol Institute some. He was a person who was around some, and I certainly talked with him and whatnot. And then Al was there and Connie was there.

Farreras: Okay, because I've heard that Al brought a whole lot of people from BU down to the lab

Ingraham: Well, that was before my time. Another person was Rich Nakamura.

Farreras: I haven't talked to him yet.

Ingraham: He was one of the people that worked with Al early on, and I don't know how much Al has talked about his own recruitment process and what he was or wasn't promised to come down. But you don't often move your entire career without a fairly attractive reason. So my hunch is that he had been offered inducements to come down, part of which would have been being able to bring people to work with and that kind of thing. And it's not clear to me whether that panned out in a way that he would have expected, so I don't know how many from that first group of people that were working with him ended up staying and doing what they wanted to do. And that first group had been doing more primate-related research and then shifted over to more human electrophysiology and attentional kind of work around the time I came.

Farreras: Do you know what led to that shift in research area?

Ingraham: By the time I got there, that had really already taken place. But my hunch would be that it had to do with what resources were available to do different kinds of things, and all of that kind of work is expensive and difficult. And I don't know whether it was an issue of funding resources or access to various other kinds of things. It might have been. It might have made a difference.

Farreras: Could you say a bit more about what type of research the lab was doing at this time?

Ingraham: Well, individuals had different types of projects that, although of a similar theme, weren't closely integrated, so that it wasn't as though everyone was working on the same area, which is fine. Ted, of course, was working on autonomic responses, which he'd been doing for some time and continued to do, and he really had a good setup and continued to work productively. He probably had the most collaborations with other labs, working with other clinical labs, seeing their patients, that sort of stuff. He would either have patients come to see him in his lab or set up a setup with somebody else's, so he had a fairly broad set of collaborations, which was good. When I first joined him, Al was really gearing up for the event-related-potentials work and had these beautiful rooms that were electrically isolated so that you could measure very subtle EEG type things [unintelligible With so many machines doing so many things all around the NIH, you couldn't do that easily without really shielded rooms, like these really beautiful shielded rooms built to do that. But he was really just gearing up on that, and then was very closely allied to the work that Connie Duncan was doing on P300 looking at attention-related components of event-related potentials. Al was beginning to do his modeling of attention, the notion of there being components of attention. And Seymour and I were very much doing the adoption studies, looking at questions of gene/environment interaction in schizophrenia. And then Al and I worked together on the Israeli studies, which were asking the same kind of question in a little different way, looking at how a very different environment and different rearing conditions affected risk for schizophrenia in patients who had a parent that had schizophrenia. You know, when you do this kind of work and you know all its subtleties you take it for granted. But the big difference between the work I was doing with Al and the work I was doing with Seymour, was a different way of doing risk studies. Seymour and I weren't looking at people – and everyone makes this mistake when they think about it. We weren't looking at schizophrenic parents who had given up their children for adoption, we were looking at people who had been given up for adoption, in most cases before their parent ever had schizophrenia, if they did, and then finding adoptees who had schizophrenia and then going back to see, once the adoptee had schizophrenia, whether their biological family had more schizophrenia than biological families of controls. The advantage of that approach is there's no potential bias for the placement of the kids. Most people thought we should just look at the adopted and the biological families, but the reason you don't do that is you have to qualify to be an adopted family, and so they're very different just from that. So you looked

at adoptees overall, some of whom had schizophrenia and some who didn't, and then you go back and see what their families looked like. It's a purer way of saying what the factors are. The high-risk approach, which is what I was doing with Al in the Israeli studies, involved finding people who have schizophrenia and had kids, and then looking at the kids as they develop to see how early on do we see differences between the children and people with schizophrenia and those who don't, and how early do the first symptoms appear? And so it's a very powerful technique for seeing early on what's going on. But there's potential bias because every once in a while, so-and-so had a mother or father who had schizophrenia. So that satisfied a complementary kind of work to be able to do, to have the high-risk studies studying how early and how soon you begin to see differences, how early the symptoms associated with schizophrenia begin to manifest themselves, and then have the adoption studies as a pure way of separating out genetic risk from environmental risk.

So that was all going on. But around that time, when I had first come, around '85 or '86, he convened a conference of high-risk researchers in the Stone House [Building 16] that was probably the most thematic pulling together the lab while I was there. Basically everyone in the lab presented or talked about their work and how it related to this high-risk concept of how early these people had signs of psychopathology and what those might be, and so Ted would talk about his electrophysiology with children of patients with affective disorders and schizophrenia, and what did you see in these children. And Al would talk about attention. And we talked about what we saw in our Israeli study, and Seymour talked about the work he was doing. And we had Elaine Walker, who is at Emory doing studies of children of parents, families with schizophrenia, and looking just at very young kids. She's done some interesting studies looking at old home movies where you see a healthy family where no one had risk of schizophrenia and the child later on developed schizophrenia, go back to their first birthday, second birthday, third birthday and see what the behavior was like, what the interaction was like of this child, and then compare them to their siblings at their first birthday and second birthday. So she has a nice way of doing that. Joe Zubin also came and talked about his work.

Farreras: Wasn't he here? I've got some photographs with Al and Seymour and...

Ingraham: They might be from the conference. It's possible that they might have taken some pictures. I think there's a picture where we're all sitting in the front row. That was at this conference.

Farreras: Okay, I see.

Ingraham: So that was a really neat thing. I mean, it was fun, and in a way that was the most integrated connection, lab-wide and to other labs around the world or around the U.S. doing that kind of work. So that was a way of people fitting together as a lab in what we were doing. But then, after this meeting, people continued to pursue their own directions.

We had a lot of collaborations. It was tough. You know, patients were a resource and patients were real expensive. Because it was the Clinical Center, everyone there had to be treated like a full-fledged medical patient, even if what you were doing was interviewing them. Admitting patients meant everyone needed a complete physical, just worked up as though they were coming in for a medical study, and that was demanding. And because they were a scarce resource, other labs weren't particularly eager to say, "Here, now help yourself to our patients if you want to interview them." And they tended to bring in other labs that would be more likely to bring in their own post-docs if they wanted someone to do a neuropsychological evaluation rather than saying, "since this is what you do as a lab, why don't we have you come help us with this." They'd be more likely to say, "well, it's a lot easier just to hire a post-doc for a couple of years and have them do our neuropsych for them." That was one of the wonderful things about working with Seymour, that because he was such a senior person, everybody came in to talk to him, and so, by sharing an office with him, I heard everybody's complaints. And when I first started there, I thought, everyone is out to get the psychologists, you know, it's just terrible. I could just see so much evidence of unfairness and lack of resources and whatnot. But then the biological psychiatry group came in and they were being robbed of their just resources.

Farreras: By whom? Because they at least were mostly M.D.s, not Ph.D.s.

Ingraham: Everyone was after everyone else. Where the big tension outside – and I mean big, and it still is probably one of the driving tensions there – is the tension between clinical research and basic research, and so if you're Danny Weinberger doing clinical work with schizophrenia patients, you justifiably feel this is wrong, and what you say is, "This isn't the National Institute of Science, it's the National Institute of Health. And although the basic research is essential to our mission, we're supposed to be thinking about patients." And if you were in, say, Carl Marilyn [sp.], who's a fine person doing basic biochemical genetics, you would say, "We're wasting our time until we're able to really figure out the basic science." It's a very appropriate role to take. But the clinical labs would be outraged at how expensive the machines were that the basic scientists were buying, and the basic scientists would be outraged at the hundreds of thousands of dollars it took to talk to a few patients. And I should say, it's not just within the NIMH. The time I was there, the chemists were feeling attacked. They

said that there was a sense that chemistry is an old science, and we're going to do molecular biology, and that chemistry has really done what it has to do. And so I could talk to people who were chemists in the Cancer Institute or Heart, Lung and Blood who would be saying, "They're out to get the chemists." So I was fortunate to have that experience of seeing people from a number of branches in other institutes come to talk with Seymour about how afflicted it was and how tight resources were.

Anyway, so there certainly were turf battles and inter-professional tensions and just personality conflicts between people running different labs, but it was partly in the context of resource limitations.

I don't know if you're familiar with Colin Turnbull old ethnographic study of people called "The Mountain People." It was this African tribe that he'd gone to study as a graduate student, and they were the most peace-loving community on earth, and everyone treated each other with respect. It was this idyllic Eden. And then, because of political considerations, this tribe was moved from a part of the country where it had always been to a different terrain. It was very different in terms of having to hunt differently and there were fewer food resources. So he goes back to visit this tribe years later, and they're now the worst place on earth. The people are laughing at each other's misfortune, they're stealing food from their parents, the children were abused. It was this completely dystopian society. And so I used to think about that and make my way through the jungles of the NIH. As long as there are enough resources, it was splendid and people really worked together very well. And once resources were very scarce, people became much more competitive and difficult. And so part of that disintegration of collaboration was when resources were so tight. There certainly were intellectual battles also about what's the right way to answer these questions, but that was...

Farreras: You mean within the lab or outside of the lab?

Ingraham: It was in the lab as a whole and within the institute as a whole. You would do to grand rounds and I would argue that our way of approaching _____ interaction was a good way to do that, and the people argued that no, another way was good. That was fine. But the more passionate arguments were more often around resources than they were around ideas, and it's probably still true to some extent.

Farreras: I'd also heard, although it was a little earlier than when you arrived, that when Herb Weingartner took over as acting chair, after Dave Rosenthal, that he was so wrapped up in his own research that he wasn't really focused on or looking as much toward the future or success of the lab itself, and that once Al Mirsky came in, that he's such a nice, conciliatory

person that he didn't have the competitive greed that would have pushed the lab to obtain a greater share of resources.

Ingraham: Yeah, this was before I was there, but my sense from talking with Gene Tassone, who really was there through this, was that as Rosenthal became more and more incapacitated there was a long period in which it was difficult to collaborate with the lab. The sense I've had talking with some other lab chiefs from before and from hearing people's experiences with it, as Rosenthal became more and more incapacitated other labs became more wary of collaborating because they couldn't count on things happening that were supposed to happen, and partly as a result of that had begun to look elsewhere for their collaborations. And so I don't know how true that is in terms of, if I were a competitive lab chief wanting to get out of collaboration, I might say, "Well, the current chief or the LPP is a little disorganized and let's find someone else to do it," as opposed to how much that was a real problem. But, as a consequence, when that period ended, a lot of labs who previously had had fairly strong collaborations with our lab no longer did and it fell on the next people, whether it was Herb or Al, to try and reestablish connections that had been lost. So it wasn't that there were these strong collaborations going along and they just ended but that they dwindled away over a period of several or many years.

Farreras: What were the main labs during this Rosenthal period that collaborated the most?

Ingraham: Adult Psychiatry was the main place. People were looking at schizophrenia and doing all kinds of testing and evaluation. And I guess at one point we were sort of part of the lab, so when it migrated to being its own lab, it's where all those connections had already been established, and so it made sense to continue that. And then as time went on, it sort of dwindled.

Probably another factor in the background going on at that time was Fred Goodwin's desire to reunite the NIH with the NIMH, wanting to make the NIMH look like a natural fit to the NIH was certainly much more oriented towards biological psychiatry than clinical psychology. So that is partly part of the theme of where support was coming from, which labs were supported, and what kind of work you wanted to encourage, and that certainly had an influence.

When the ADAMHA was created and they pulled these institutes into it, a lot of the psychology went to the Extramural Program. I don't know if you are talking to people up there, like Stan Schneider... Who was the director of Behavioral Sciences? Llewelyn Bigolo [sp.]? I don't know. And there's someone else. Not him, a director of the sort of behavioral sciences up in the Extramural Program. But a lot of people had migrated up there because the Extramural Program was much more behavioral. It has laboratories that are sections devoted to personality and personality

development, child development...and so a lot of people ended up there. And there's a big tension, and there always has been, between Extramural and Intramural staffs.

I don't know if you've heard the stories about when ADAMHA was founded in Seymour's [Kety] time. When it was first founded – I forget who the first director of ADAMHA was – he was a very community-active, social-science type person, and they had a retreat where they brought all the labs down to study what was going to be the direction of mental health research, and they basically said it was going to be all community psychiatry and that's what ADAMHA did. It supported community health centers, and very appropriately, _____ this was its mission, but that was so different from the mission of the Intramural Program. It really went very far in the other direction, saying we're not here to do community mental health, we're here to do basic research, and so there was a real split between the two.

Anyway, the person I'm thinking of was director of Behavioral Sciences in NIMH for a while, and then he got sent to be in charge of the Office of Scientific Integrity or something. It will come to me.

The other person who knows the history of all this stuff who's still around is Ellen Stover [sp.], and she runs the AIDS program now at NIMH. That was an interesting story. I mean, that was when the AIDS became _____ that as a chunk of my time there, too. When AIDS came along, because it was a new source of funds, every institute said, "We should be the lead institute and we should be in charge of this money and how it's spent," and so there was a big fight, of course, between Allergy and Infectious Disease, and "This is our disorder," and Mental Health saying, "This is a behavioral disease; it relates to behavior and that's why it should be our disorder." And Ellen was fairly successful in getting a lot of resources for the NIMH and I think that her office is now called something like the Office of AIDS and Behavior or something up at the Extramural Program, like the Office of AIDS and Behavioral Studies. But she very adroitly saw that this was going to be a long-term problem and a long-term resource and made sure Mental Health got a piece of that. And her former boss is the person I'm thinking of. It will come to me. But that was a big event in NIMH.

Farreras: Tell me a little bit more..you were talking about Goodwin wanting to bring NIMH back into NIH from ADAMHA.

Ingraham: Right. It had been an issue for a while. It first was an issue when it split off, and then it was an issue to bring it back. My understanding was the major sticking point in that _____. Basically, it seemed one of the major sticking points in bringing them together was that they had developed completely separate study sections and grant-review teams, and they really had separate reviewing procedures. Bringing them back together was supported partly by an economy-of-scale kind of thing: "why

do we need separate review processes if we really are doing the same kind of science?” But the personnel issues would be, “how do we protect these many, many employees, and ADAMHA, if we’re going to be merged back into it?” So that was, I think, part of the process in trying to figure out how to do that in a way that would be acceptable.

When ADAMHA was formed, it was very much in the context of services, and the reunification with the NIH was in the context of getting mental health out of services and we’ll have SAMHSA, which will be interested in services but will be a completely separate organization. So that would be part of it.

I think the other big part of the picture is thinking about the history of psychiatry and its own struggles to decide what kind of a science it wanted to be. Early on, psychiatry was very much involved in behavioral kinds of questions and then very strongly embraced using biological models and tools to address mental health questions, which was fine. There were wonderful new tools being developed and ways to do that, and that was fine. But then it was a fight for what is psychiatry as a profession, and what kind of training models do we want to use to train our clinicians, what kind of research do we want to do, how are we the same or different from the Neurology Institute? And there was a fair amount of thinking about what kind of research fits into mental health that doesn’t fit into neurology, and how do they go together? And I think Irv Kopin was in Mental Health for a while and then was in Neurology. So there was a recognition that there was a lot of overlap between those two. But part of the reunification was saying let’s align mental health as a basic science along with the other basic sciences, as opposed to services. And, again, that’s a clinician-basic scientist battle going on. Is psychiatry primarily a basic science discipline or is it a clinical discipline? That’s still a debate. People still fight about that.

Farreras: Do you think that that shift from the behavioral approach to the biological approach might have contributed to...

Ingraham: Well, certainly there were... I don’t know if Al _____ talking about subsequent intramural directors. Fred Goodwin, for whatever directions he took things in, recognized the importance of behavioral science, sometimes more, sometimes less, but in his work on bipolar illness, for example, he’s very clear that this is not solely a biochemical disorder, that at least the treatment requires the integration of clinical work with biological treatments. And I remember working on some policy statement with him at a point where he was _____ be clear that it was – he felt it was unclear whether behavioral factors caused mental illness, but it was clear that behavioral factors were very influential in the course of the illness. And so he was uncomfortable saying – I think appropriately – that you could cause schizophrenia or cause bipolar illness solely by psychological factors. It’s more likely we’ll find biological factors that are very important in the onset of those illnesses. But then once someone has

an illness, how their course goes, how they respond to treatment, whether they are a good outcome or a poor outcome is very profoundly affected by behavioral factors. So, as anyone in that position, there's a world of limited resources, you're wanting to support your own students and post-docs and whatnot, and that's what people do. Bringing the NIMH back into the NIH may have not been helpful for behavioral science, but it's hard to tell.

The later director, the one who was probably the harshest on behavioral science, was **Mike Brownstein [sp.]**, and he would say things like, "Why do you want to waste your time studying things like that?" or "If you feel like you should waste time and money on something like that," "If you want to waste your resources that way, okay, but I don't understand it," but a very much more, "Why even bother to do this? This is so beyond what modern science should be doing that we don't really want to have any of this." So that certainly influenced the lab. But I think all the labs were feeling _____ stress.

Farreras: Okay.

Now let me see. You moved from staff fellow to senior staff fellow in '88, from '88 to '93. How are these categories defined?

Ingraham: One of the things at the NIH is you could only do certain positions for so long, and so after I ran out of being a staff fellow, I had to be called something else. But they're all really pretty much the same. I mean, I was doing the same work. They were just different titles.

NIH at that time was going through a reorganization of how it structures its fellowships and the timing. Back in the time when there were lots of positions, it didn't have to focus so carefully on these things. And then it realized it was going to have to do that.

Just in the way the NIMH Intramural Program was structured, the budgets went to lab chiefs, who then allocated them across the lab, and so the lab chief had a fair amount of discretion about how they used the resources. What happened was the staff fellows – not just in mental health but across the entire institution –, as the academic job market became more difficult, started having more and more difficulty getting jobs after their training at NIH, because universities were saying, "We want to see someone who's had a grant or had funding and has potential to do that." And since all the funding at the NIH was through the lab chief, if you were applying for a university position, you couldn't say, "Well, this is a grant that I applied for and won and work that I found." So they now have revised it so that staff fellows have the equivalent of a budget. It still may be assigned by the lab chief, but you can track what your research funds are how you're doing the research. So it's a little clearer, if you're applying someplace else, what your specific projects were and how they went.

And likewise, the whole tenure system was that when Seymour set this program up, they thought to have the Intramural Program exempted from traditional civil

service regulations, which was, if you worked someplace else in the government for a few years and made tenure _____, the sense was you didn't want to hire a bunch of people and have them be there for life. You want to keep bringing in new people and train them. The mission of the Intramural Program when it started was to train people who would then go out and help start other departments in other locations, which is a good thing. So it was set up not to keep people around very long, and so there's a lot of the structures there which are designed not to keep people around very long. So I was fortunate to be able, after I'd finished my staff fellowship time, to have interesting enough projects to stay around and _____.

Farreras: But you eventually left in '97, right around the time when Leslie Ungerleider was made permanent Lab Chief, and the Lab was renamed the Laboratory of Brain and Cognition...

Ingraham: Yeah. So how did I decide to do that and what made that interesting?
SIDE B

Well, I mean, _____ a couple questions. Not having been there in a while, _____ does Leslie have an office in the lab space? Certainly for the first few years, her lab had been in another building, and she then _____ for at least as long as I was there, she didn't have an office. She had a space that was assigned to her in Building 10, but she didn't use it.

Farreras: Her office is on the fourth floor of Building 10 now.

Ingraham: Okay. So she's now moved over. At the beginning, she never appeared in the lab; her office was in Building 49, I think.

Farreras: Right, where the Neuropsychology Lab is.

Ingraham: She was in the Neuropsych Lab there. And her work didn't really change when she did this _____, and she continued to do pretty much the same work she'd been doing before from the same office she'd been in before. So certainly, from a perspective of did a new person come into the lab and say, "I'm going to set a new direction and this is how we're going to do things," it wasn't like that at all. It's more that she continued to do the work that she'd been doing, and she now had some additional resources. And in addition to the resources she had already, she now had some more positions and space and could think about how to use that. It was less, "I'm coming in to take this lab in a new direction and do new things."

And I think part of that, too, is that it wasn't as though she had a research program that she was bringing newly to the NIMH, in the sense that when Al came it was "this is a direction I'm going in and work I want to continue and I think is important." It was more that that work was already part of the Intramural Program. Our lab wasn't doing it, but the Neuropsych Lab was doing it, and it was more a continuation of work that was already going on. So, it was probably _____ of the Neuropsych Lab was expanding its

resources and the Laboratory of Psychology was reducing its resources. And in a sense you could conceptualize it as, since the Neuropsych Lab had split off from the Laboratory of Psychology, maybe this is a way of pulling pieces back together. And as resources became tighter and things shrunk, this is almost a rejoining of these two labs back into one. So you could think of that as a theme. But I think that what was very clear and what really did change was that there no longer was any focus on psychopathology, which had really been the core of the lab since the start. Leslie was very clear that she wasn't interested in psychopathology, which is fine. Her research interests lay elsewhere. But I think if there were a theme behind holding the lab together, why people did this was that we were interested in understanding... You know, schizophrenia was really the core of that work _____ an understanding the severe mental illness, and we might try to understand it through ERPs or through psychophysiology or through genetic studies or longitudinal studies or whatnot, and that the methods changed and the people changed, but the question of the centrality of severe psychopathology was no longer part of the lab, and that was a dramatic change. That's a real difference.

And, thinking broadly within the field, I think in addition to working with patients, we were working with basic science. Part of it, the debate about psychopathology is best understood as a variation of normality, and so we really want to understand normal brain function and then see how that, in its extremes, might look like psychopathology. And I think that's a very psychological way of thinking, that we have normal distribution, we have extremes, but it's all part of the same thing. And so if we understand normal signal processing in the brain, like Leslie was studying, visual perception and how the pathways transfer information, if we wanted to take that information to study psychopathology, it would be the variation of that, versus a more discrete model of psychopathology, of saying severe bipolar illness or schizophrenia is something different in the same way that Down's syndrome is different from not-Down's syndrome. We can't be confident that studying normality will let us understand this particular flavor of abnormality. So that was a real change. And it's sort of a question, or a theme across psychology, too: Is the business of psychology studying normality and its variation around the mean, or is it studying psychopathology, which is something different?

So in terms of how the lab changed, that was a dramatic change, that henceforth, the lab would not be interested in psychopathology as a sort of organizing principle.

Farreras: Was that a decision that people higher up would have had to have made?

Ingraham: Yes. But it's hard to know how or what different things influenced that. There certainly would be plenty of people who might speculate on that. And the transition wasn't as though Al decided to retire and a search was mounted. It was a much different process. It was, "We're going to

reorganize things.” And I think it was under **Brownstein** that that happened. I think Goodwin had left and it was when **Brownstein** was chief that that happened. What Brownstein did was mount this – I don’t know if you’ve been introduced to the topic of lab reviews and how they work...

Farreras: Oh, yes, the Board of Scientific Counselors.

Ingraham: Okay. **Brownstein** basically mounted a series of very hostile and critical lab reviews of labs whose resources or allocations he wanted to change. Now, to what extent it was his own initiative and to what extent it came from other sources, it’s hard to know. If I’m not mistaken, Fred Goodwin ascended to directorship of ADAMHA or whatnot with the ability to name a successor, which I guess was Steve Hall, a sort of unusual arrangement, but that worked out. And Steve Hall was a very good, in many ways, intramural director. And then when he left, Brownstein came, and Brownstein was probably the most hostile person.

And so on his watch, you had these series of lab reviews, which were extraordinarily hostile, and which were basically to try and come up with some justification for things that you _____ wanted to do anyway. So I don’t know to what extent his motivations were reallocating resources to his own lab – I mean, certainly as intramural director, he managed to bring quite a bit of resources to his own lab, and other labs didn’t fare as well – and to what extent he had actively supported that, I don’t know.

It’s hard to know, in terms of the direction the lab took for its last five or six years, how much of that was resource-limitation based and how much was just the direction the lab was taking, because – in the context of limited resources and limited access to patients- it made a lot of kinds of work harder, so being a lab chief under those conditions limits the kind of work you can do. So it’s hard to know. Is the lab not doing certain kinds of work because it’s not interested, or is it because it doesn’t have the resources? And certainly resources became pretty tight.

Now, in the last lab review that I was involved with, there was moderate criticism of the work that Seymour and I were doing because we weren’t doing as much basic genotyping of the patients that we’d seen, and so this was a concern: was this old-fashioned, to be studying schizophrenia without doing genotyping? At that time there was a sense that we were just around the corner from identifying a gene for schizophrenia, and if only we had blood from these patients, we would solve the problem. So our corner of the lab could have been criticized for not doing more basic genetics. On the other hand, our corner of the lab barely had enough money to persuade our collaborators to send us transcripts. So the resource issue was part of it.

I think that that was at the peak of the time when there was a sense that we would stop part of the work that I was working on – that is still worked on – on the notion that, are we going to come up with different diagnostic schemes for psychopathology that, rather than being based on phenomenology and

symptoms and a clinical picture, will be purely biological in nature? And the reason for that is that, unlike almost any other disease you study, you don't have a definitive lab test for something like schizophrenia or bipolar illness. There's a clinical picture. From the biological side is the sense, why don't we define illnesses _____ who has an elevated amount of this neurotransmitter or a very biological definition of the disorder, and that's not really worked so well because there's a great deal of variance in that. There are some schizophrenia patients who have somewhat slightly enlarged ventricles, but you couldn't make a diagnosis based on that, and likewise for all the biochemistry and whatnot.

So there was a sense that if we went to a biochemical model of psychopathology, we might then say this person doesn't have schizophrenia but they have excess dopamine. But that hasn't been as successful as I think people thought it might be. It may turn out at some point to be able to do that a little better, but it hasn't yet. So in some ways, I think the biochemical labs are saying, "Let's move in that direction. Let's have biochemical definitions of psychopathology," and that's why any work looking more at clinical phenomena is going to be less useful. And I think one could think about that and say, if you're studying attention, as AI was, that attention is the final manifestation of a complex chain of biochemical events, and if you're studying them back here, where they first begin to start in the brain, you're studying a more basic phenomenon. AI would probably argue that you're not studying the patients, you're not studying what's going on clinically. Anyway, so I could see someone looking in that direction. Certainly the access-to-patient issues is an enormous _____. Because if you're arguing, as we are, that the important thing is the clinical phenomena of the disease, then you really need patients. If you're saying the important thing is saliva cortisol, then you don't need as much access as you would otherwise.

Farreras: And this was happening around the time when you left, then?

Ingraham: Yeah, when it was very clear when Leslie became lab chief, Seymour retired, and we'd been working very well together. Basically, we had Mike Brownstein come in and do a lab review and basically say all of our work wasn't that good and we shouldn't be doing it. Seymour decided that it was time for him to retire for real. And Leslie came in as lab chief and really said, "I'm not interested in psychopathology," and I'd say not in a hostile way at all. In talking with her when she first became lab chief, it was, you know, "You're doing interesting work, I hope you continue to do it, it's worthwhile and important, but I'm not going to be doing that and that's not going to be the focus of the work my lab does." So that was very clear that that wasn't going to be what was going on there. So that made me – not wanting to change the focus of my work in schizophrenia – want to continue this, but not there. So that was really what led me to say we're going to continue to do work. I was very fortunate that around that

time GW [George Washington University] was setting up this new program and they needed someone who could do brain and behavior work for them, so that was very good. I was very fortunate that that worked out. That's really how that came about.

Farreras: And you've been there since then.

Ingraham: Yeah, I've been there since. That's worked out really well.

Farreras: And what the clinical neuroscience branch?

Ingraham: Oh, when I left, I'd been working with Ed Gent's [sp.] lab. He's now up at the Birdneck [sp.] Neuropsychiatric Institute, up in Massachusetts. He's been doing work on bipolar illness. I don't know if you're familiar with the Amish studies and this attempt to look at the genetics and the _____ bipolar illness. He'd been working with Janice Eglund [sp.], who does the Amish studies, and he'd been doing the basic biochemistry and genetics and had really been doing fascinating work. And he had two things that he needed to do that I could help him with. One was, he needed to set up some fancy statistical stuff to help manage his data, and I knew that kind of stuff. And he wanted to think about how you think about gene-environment interaction in a sophisticated way in clinical populations, and _____ wondering what are the kinds of stuff that Seymour and I worked on in Denmark with something that might be relevant to the Amish and the bipolar illness. And it was a natural connection for me because I was interested in schizophrenia, there was increasing evidence that there's some overlap in trying to understand psychosis, and it may turn out that the genetics of the disorders are that there are separate genetic liabilities for psychosis as opposed to schizophrenia, sort of schizotypal symptoms and manic symptoms, and just regulated sort of time and sleep-wake cycles in bipolar patients. And so I said I really owe it to myself to know a little bit more about bipolar illness. I've really been a schizophrenia person, but I should understand more about the boundaries.

So he was looking for someone to help him out and _____ interested in me, and so I said I would come out, and _____ as much from them at GW as in his lab, _____ worked together on a couple of papers. Then when he moved up to Massachusetts, our collaboration decreased because we didn't work as close.

Farreras: So this all happened around the same time that you'd left for GW...

Ingraham: Right, like '97 through '98 or '99. And sometimes Judy Rappaport will sometimes have an interesting childhood schizophrenia case in, and she had a staff fellow, Rob Nicholson, there for a while, and he and I have collaborated on a paper. So there are still contacts there, especially in the schizophrenia world, people that do schizophrenia stuff that I connect up with. But I'm no longer officially affiliated with the NIMH.

Farreras: I see.

When Al came back to head the lab he brought BU people with him; did Leslie bring any

new people in as well?

Ingraham: She took over administratively more, and then she brought in a couple of post-doc fellows at Fogarty. _____ people are starting to do is bring in more Fogarty fellows because you could still get those, and you couldn't get the regular staff fellows. So she certainly brought in several people to work with her in that area.

There's an LPP alumni web site; you can dig up from there who all those people were over time, where they are now and what not.

Farreras: Yes, I've looked at it.

Ingraham: The person who is the British guy who's gone back to England, Peter -- not Grossenbacher [sp.], another one. You can actually look at the list. But there are a couple people whom she brought in as either Fogarty fellows or staff fellows who worked a little bit and probably did the human neuropsych stuff. Alex Martin became _____ more of that.

Farreras: I think there was a hope that he and Al Mirsky would collaborate more than they did.

Ingraham: Yeah, I think they had different orientations. I know that at one lab review, they brought in Alex Martin to evaluate Al, and I think Al was a little miffed. At that time a very, very junior person had been brought in to evaluate him, and I think it created some awkwardness in their relationship that they couldn't have otherwise _____. I think that was difficult. But Alex got a lot of work, and the AIDS money came along, so we got stuff started with that. That was interesting. But, yeah, they were really doing very good _____, so it didn't lead to a natural collaboration _____.

I think a lot of the frustration and difficulty at that time was, just as the big imaging things were getting started, they were very closely guarded resources. And in a sense, academically and career-wise, there was a time there, for a few years, where basically anything you did on a scanner was an interesting publication, because it was new, and so you didn't really need to have a particularly elaborate model of the world to say, "I'm going to scan for laughter," "I'm going to scan for bilingual," or "I'm going to do scans for verbs versus nouns," or I'm going to do scans for whatever, and you did the scan, you wrote a paper, isn't this exciting, and this whole field started evolving, with some people having more of a model of what that was going to be about, _____ less, and I think everyone, in some ways, like anything else in science, brought their model of the world to imaging and said, "I can use imaging to show that my model of understanding of the world is a useful one." And to some extent, that still continues. You say, "I can look at this imaging." But imaging slots were very contested, and there were bloodthirsty battles over who would have a slot and who wouldn't, and then everyone had to pretend that they used their slot, because a lot of times the slots _____. You had to be secretive, very secretive about it, so you would do things. You'd grab people off the floor

and say, “Let’s get someone down so we can show that we’re using the slot, and it doesn’t really matter what we’re doing.” So there was a hyper-competitiveness around the slots.

And also at the same time, there was the impending implosion from St. Elizabeth’s, where Danny Weinberger had his imaging group, so there was just a very difficult atmosphere around imaging.

In thinking about what was modern about it, the two modern things going on were the development of molecular genetics and the imaging stuff, and the molecular genetics was a little more open. People could do molecular genetics as a cottage industry. The imaging, you couldn’t. It was big money. And so that was very hard.

I never particularly tried to grab any imaging slots because it wasn’t what I was doing, but I’m sure Al could tell you more about that, what was possible and what wasn’t, and his interest in being able to do that and what the limitations were.

One reflection of that was how challenging the environment was, because for years NIMH tried to bring someone to run their imaging programs and didn’t have much success in persuading anyone to come. _____ were aware of how challenging that environment was. So we had seminar after seminar, people coming to talk about imaging, and then _____ end up there. So that was a different environment.

Farreras: Very interesting. I’ll follow up on this with Al as well.

Well, those were all the questions I had at the moment. Is there anything you wanted to talk about that you feel is important that I haven’t picked up on?

Ingraham: Let’s see, I want to give you this article I saw this week in the [American Psychological Association’s] *Monitor*. What’s interesting is that he talks here about psychology being fragmented and split into cognitive neuroscience and whatnot, and now there are some efforts to pull things back together.

Let me just turn this off for one second. [Recorder turned off and on again.]

Part of the competition of the NIMH going back to the NIH was to say, the critics would say, “Well, why should this very weak science and very unadvanced science without a good paradigm come back to be part of the real world of science?” and there was a real push to prove that and for NIMH to say, “We’re doing science just like everyone else does.” And I think within psychology, if you were psychology within NIH, the kind of criticism one would get was, “Why aren’t you doing the basic science the way other people do basic science?” so that pushed it to do that kind of work, rather than saying, “How do you integrate this with the broader picture?” So that was very much the case there.

Farreras: When I talked to some people from the other sections, like Morrie Parloff in Personality, and asked them about their various sections and whether they could come up with a list of “classic papers” or “discoveries” that they thought represented the research the sections did, his response was,

“You know, we did a lot of research. I can tell you all about the research. But there weren’t any kernels of truth that we discovered out there the way neuropsych people can say, “Well, Mort Mishkin discovered this and that.” There were no earth-shattering discoveries that were made because we didn’t have those kinds of hard data that maybe the Perception and Learning or Neuropsych people had.” But that seems to be, by definition, what you’re going to get, given what you’re studying. Some areas don’t lend themselves to such hard data...

Ingraham: Right. That’s true. That’s interesting. Within the genetics of schizophrenia part, that was _____ to say what were the things that the lab did that were real paradigm shifts? That was a real paradigm shift. And up through the ‘60s, people seriously talked about genetics not being a part of schizophrenia and...

Farreras: Just schizophrenogenic mothers.

Ingraham: There were schizophrenogenic mothers and there were double binds and there were all these things suggesting that genetics played a role. It’s dehumanizing and reductionistic and it couldn’t possibly be relevant.

And the Danish studies were a real gamble. You didn’t know _____ what they were, there was strong inference. You didn’t know how they were going to come out, but once they were done, you really had an answer and you no longer... Once in a while you’ll see papers written. There’s one that came out a year or so ago saying, “The Danish study didn’t show what they were supposed to show,” or something, but generally, today, if you want to argue that genetics aren’t a major factor in schizophrenia, you have to be pretty persuasive. People accept that. You say, okay, genetics is a big part of the picture. We don’t know what part, and I think that Seymour and I always stressed that saying that genetics play a role doesn’t say they play the only role or that they play a role in the brain or in the immune system. You don’t know that answer yet, but we know part of the answer, and that’s a big difference. And that’s a complete shift in the field. And at the time, there was a sense of, “This will be the end of...” Seymour told me once that one of his colleagues said, “Seymour, don’t do these studies. It’ll be the final nail in the coffin of psychiatry. If it turns out that it’s true that genetics plays a role in schizophrenia, you won’t have a science anymore.” That’s not true. I mean, we’re now saying, “Okay, we’ve learned something and we can go on.” I think that’s _____ and it’s funny. Once you get a shift like that, then you don’t have to do it again. It’s as if you’ve done an experiment and you don’t have to do it over and over and over again. I’m just thinking about criticizing the lab for not doing new things, because you could say, “All right, the lab did its job. It found out this about schizophrenia, and now it’s time to move on,” which we tried to do, but it already was a change.

The other _____ thing about that, Joe Zubin [sp.], who had been working in the lab and did schizophrenia work and was a colleague, was

once asked, “How do you tell if you’ve had a major impact on a field?” – because he had come up with the whole idea of gene-environment interaction, he called it stress diathesis hypothesis, that people have a genetic underlying structure that may or may not develop into schizophrenia, but with the right triggers, it will – and he said that when he came up with that, it was very radical, and that then it became accepted knowledge, and we have books about it now. _____ how _____ good theory of schizophrenia. He says, “You know you’ve had a major impact when _____ anymore.” Your work has just become common knowledge. Well, of course; that’s the way it is. So in some ways, some of the things I think the lab did early on became common knowledge, and so people would say, “Oh, yeah, we always knew that.” But we didn’t, and that was a change.

So, anyway, that’s all.

Farreras: Thank you so much for your time. I really enjoyed speaking with you.

Ingraham: You’re welcome.

End of Interview