

PRESERVING MEMORY: NEWARK AND RUTGERS
IN THE 1960'S AND 1970'S

An Interview with

BARRY KOMISARUK

Conducted by

Gilbert Cohen

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INTERVIEW: Barry Komisaruk

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GILBERT COHEN: Okay. This is Friday, September 27, 1991. This is Gil Cohen. I'm meeting with Professor Barry Komisaruk in his home in Maplewood, New Jersey. And we will be talking about a few things. Okay. I was talking before, and I asked you if you could give us a brief sketch of your academic career before and after coming to Rutgers University.

BARRY KOMISARUK: It started when I was a teenager. There was an academic group before I became aware of it. It was Joe Rosenblatt and Dan Vernon were graduate students together at NYU. So was Ed Bloustein. They were all graduate students at the same time in NYU. And Danny and Joe took as a mentor Theodore Schnitter, T.C. Schnitter, who was an animal behavioral specialist, research specializing in ants. And also Ethel Tobank [sp] was among that group. And the way I got involved with them was that my parents had a summer house in Golden's Bridge, New York, about 50 miles north of New York. And I had a friend, Peter Gold, and we were friends since about seven or eight years old. And when we were teenagers, Pete and Joe Rosenblatt spent the summers in the same summer community with his wife. And Joe had asked Pete to help him out. Joe was working at the American Museum of Natural History in the Department of Animal Behavior. Pete helped him out for several summers. And then Pete got a job one year, and asked me if I would help. So that's how I got.... So I went in my sophomore year in college, I started working at the American Museum of Natural History Department of Animal Behavior. Actually we were helping Ethel Tobank. In my senior year, Ethel told me that I should take a course with Danny Lehrman, who was a visiting professor at City College.

COHEN: Was it your college?

KOMISARUK: Well, I was an undergraduate at City College in New York. So I took Danny's course, on Ethel's recommendation. Then, of course, the structure and function of the organs I learned, too. And I happened to take the course with two other people in the course at the time. One was a Harvey Fader and the other was Alan Siegel. Harvey Fader is now at Rutgers; he's been there for a long time. And Alan Siegel is at the UMDNJ in Newark in their Science Department. The three of us took the course together. There were about 25 seniors in the course. And as far as I was concerned, it was the best course I had ever taken. Danny was the most brilliant lecturer I ever heard. He was absolutely spellbinding. And I felt like—I committed all his lectures to memory. It was the most remarkable experience I ever had. I practically didn't have to take notes. It was so lucid and so forceful that I felt that I understood everything. And he talked at one point about people who were just starting to do a technique of implanting hormones, crystalline hormones, into the brain and stimulating behavior patterns with that. And that intrigued me. And he was starting to tell me about it, and then he asked me if I would like to come to Rutgers in Newark and be his graduate student. It was an offer I couldn't refuse. So I did that. And I started in 1961 as his graduate student.

At the time he had an appointment at Rutgers. He was on the faculty at Rutgers and was a visiting professor at City College. And he had started a laboratory in Rector Street, on the top floor of the building in Rector Street. And he had started that several years before. Then moved to 31 Fulton Street in 1958. I came there in 1961. And I was actually his third student. Phil Brody was there just before me; he was continuing—we were contemporaneous. And before Phil Brody, Marty Higgin was his first graduate student. And my understanding of how the institute got started was that Danny, who had also been working at the Museum of Natural History in New York, brought his bird colony to—[window?] colony—to Rutgers. And he applied for a grant in the late 1950's. And the site visitors, among the visitors was William Young, who recommended to him that it was too cramped a space in Rector Street. So he should apply for a grant to start an institute, which he did. And he got the money from the grant. And that's how it got started.

Officially the institute was started in 1958. And then Joe Rosenblatt, came at Danny's invitation, came to Rutgers around that time, around 1958, '59. And there were several other people: Arlene Tucker and then two people came as postdocs, Colin Beerer and Ernie Hanson. And they stayed on the faculty. And I stayed on, getting my Ph.D. in 1965, four years later. And the way that worked, that there was no graduate program at Newark. But I was taking my courses in New Brunswick in the Zoology Department. I was a biology major at City College. And so I was a zoology major at Rutgers. I took practically all my courses in New Brunswick. And then there was the problem of how I was going to do my—I was doing my dissertation work with Vinnie in Newark. I lived in New Brunswick in married student housing, and I was commuting. And so when it came time for me to take my comprehensives, Danny had to establish some way of granting me the degree. And I think it was because of me as a test case, that he established the Ph.D. program, established getting a Ph.D. from the Institute of Animal Behavior.

COHEN: Which was the first, do you think?

KOMISARUK: I know he had to make some arrangements to get the Ph.D. program by the Institute of Animal Behavior.

COHEN: Yes.

KOMISARUK: And I know he didn't have to do that for anybody before me.

COHEN: Mm-hmm. But he had to do it for you.

KOMISARUK: Probably Joe Rosenblatt would be better—he would know. He was on the faculty, so he would know more about what actually had to be done.

COHEN: Yes.

KOMISARUK: But I know they had to make arrangements. And from that time on, then it was possible to get a Ph.D. from the institute.

COHEN: What were the physical facilities like when you were working in....

KOMISARUK: We were on the top two floors down on Fulton Street, which used to be the old Ronson factory. And the Art Department was below us. There were two long like railroad flats; it was a long, narrow building with many bird rooms holding cages of about maybe a hundred bird pairs per room down a long, narrow corridor. And that was on the top floor. On the fifth floor with a few offices on the other side of the corridor. And right at the end of the corridor it was probably a hundred feet long and maybe ten rooms off to the left and a few offices to the right and some labs and offices down at the end of the corridor. And similar arrangement on the third floor, but with other kinds of laboratories over to the left. And that's where Joe Rosenblatt had his laboratory.

COHEN: How would you compare those facilities with the new facilities in Smith Hall?

KOMISARUK: When we moved into the new building, in Smith Hall, the Institute of Animal Behavior in Smith Hall in 1968, we had a visitor who was an eminent behavioral neuro-endocrinologist, Richard Nigel, and he toured the facilities when they were opened. And he said—he called it “the Taj Mahal of animal behavior.”

COHEN: Is that right!

KOMISARUK: Very fine facilities.

COHEN: In two different situations like that, what can you say about the effects of the facility on productivity?

KOMISARUK: Well, there was an increase in the number of students, as well as an increase in the number of faculty, the work area, so the productivity increased when the facilities increased; because there was just more people doing related work.

COHEN: Yes. What do you remember of the.... As programs on the campus developed, on the new campus, you were teaching courses in—if you could just recount.

KOMISARUK: I was teaching courses in neurophysiology and behavior and then endocrinology and behavior, a lab course.

COHEN: How did the curriculum develop through the years? I mean how was it—what was the curriculum like, if you can explain it to the non-physiologist, in the sixties let's say through the seventies and into the eighties? What was developing within the discipline?

KOMISARUK: Well, Danny had the idea of bringing people into the institute from different specialty areas, and gave each person free hand in designing a course that this person would teach in the area of his or her specialty. And so because there was a diversity of the specialties, there was a diversity of courses, and that became the core of the courses that the students took to get a degree in—the degree was still in zoology, a PhD in zoology, and a PhD in psychology, one department or the other. And it's been like that ever since, until very recently when—it's still

not official—but there will be a degree in psychobiology or actually in behavioral neurosciences. But that's not official yet. But even now the students still have to get a degree in zoology or psychology. But we were able to provide them practically all the courses that they needed to fulfill the requirements for the Ph.D. And in different areas. Areas of—Ernie Hanson specializes in social behavior in primates, and Colin Beard specialized in ethology, Joe Rosenblatt specialized in behavioral development and maternal behavior, parental behavior. Danny Lehrman specialized in hormones and behavior. And my courses were in nervous system and behavior and the endocrine system and the liver system and behavior, neuroendocrinology. And then Harvey Fader I recruited since Harvey and I were undergraduates together. And then I went to work with Danny Lehrman and Harvey went to work with William C. Young in Kansas, Ph.D.'s. When we got our doctorates, we kept in touch, and we saw each other periodically. I went to work—did my postdoc with Ken Sawyer—Charles Sawyer—at UCLA in the Byrnes [sp] Institute. Harvey did his postdoc with Jeffrey Harris at Oxford. And I met him while we were postdocs at a conference. He was looking for a job, and I said, “Why don't you come and interview with the institute?” And I told Danny about him. And so he came, and they he offered him a position so that he came back. And for a number of years we taught a course in neuroendocrinology and behavior together.

COHEN: Were you still teaching undergraduates at that time?

KOMISARUK: I taught one undergraduate course once. I really stayed almost exclusively in the graduate school teaching some graduate students.

COHEN: What contacts— You mentioned before something about the students or the SDS students were there, that you....

KOMISARUK: Yes. When I was... I think about a year after I'd gotten to the campus, probably around 1967, they asked me to be their faculty representative.

COHEN: Undergraduate students?

KOMISARUK: Undergraduate students.

COHEN: Undergraduate students.

KOMISARUK: Yes. Hacklin. What was his name? His last name was Hacklin—Steve? I can't remember his first name. And they were—I was very sympathetic to them. And I asked them what my role would be. [Break in recording]

COHEN: And we are back. [Laughs]

KOMISARUK: He said that I would be more or less a chaperone. They would do what they wanted to do, but they needed some faculty approval. And I said, “What if I don't approve of what you want to do?” And he said that there's really nothing that I could do about it. So I said, “It sounds like I would be responsible for anything that went wrong, but I wouldn't have any influence as to what they did.” And he said, “That's more or less true.” And that made me

nervous. And even though I was very sympathetic to the cause and I liked them and I thought what they were doing was good, I felt that it would be out of my control, and I wouldn't have any say as to what they would be doing. But I would be liable for anything that went wrong. So I—it was a difficult decision, and I was flattered to be asked. But I turned them down.

COHEN: You weren't signing that blank check.

KOMISARUK: Yes.

COHEN: What year was that?

KOMISARUK: I think it was around 1967 or '68.

COHEN: Since we were on the subject of the SDS, what was the impact of—since they were involved in the anti-Vietnam War movement? What was the effect of the movement among the students on the campus, on the teaching, if any?

KOMISARUK: The teaching was relatively unaffected, at least the teaching in the institute and the work in the institute. Although there was a lot of discussion, it never really had an impact on the nature of the teaching. But there was an interaction between the university and myself in that regard because I had read an article about a war resistance that was, that entailed refusing to pay the federal excise tax on telephone bills. It was a symbolic gesture. But the reason for that was that...this was organized by the War Resistance League. And the reason that the telephone excise tax was initiated at the time of the Second World War...specifically as a way to raise taxes for the war effort. It was reaffirmed at the time of the Vietnam War by Congress, that the excise tax that was on the telephone bill would be used to fund the Vietnam War. So the War Resistance League recommended that individuals refuse to pay the telephone tax. So I thought that was a good idea. And I started withholding the tax, which came to maybe two or three dollars a month. It was a symbolic gesture on the telephone bill. But it was a line item. It has excise tax, specified. The federal excise specified on the telephone bill. And each month I wrote a statement, I typed up a statement, and photocopied it. And with each bill I submitted the statement saying that I'm refusing to pay the excise tax because...and I'd get a quote from the Congressional Record saying that it was specifically designed to fund the war.

And so I deducted that amount from the telephone bill with a copy of the letter. And I was doing that for maybe three years, every month for about three years. And after that three years, I got a letter from the government, from the IRS, saying that they were going to put a lien on my salary to collect the tax. They finally caught up with me. And they said that—well, they had to get permission from the university to do that. So they must have contacted the university. And then I remember getting a copy of a letter from Bloustein to the IRS saying that he would refuse to comply.

COHEN: Oh!

KOMISARUK: And that was the last I heard of it. [Laughter] They never attached my salary. They never collected the money. But they did start auditing me. You know they audited me one

year, and the director of the IRS, when this issue came.... They were just auditing me for other—ostensibly for other purposes. But I’m sure it was because of this issue, but they called me in for an audit. And I remember the strategy of, this disarming strategy where the auditor said, “I really agree with what you’ve done. I think what you’re doing is great. But—” And I said to him, “Well, why are you auditing me now?” And he said, “Well, I’m just doing my job.” [Laughter] But, you know, he kept insisting that he agreed with my position and thought I was very courageous to do it. But he’s just doing his job.

COHEN: Yea, right... [laughter].

KOMISARUK: Yes. So....

COHEN: How did it affect your relations with other faculty, with faculty or colleagues?

KOMISARUK: Oh, it had no effect. I mean I think people were generally sympathetic. I think I let people know about it. But it was generally, I would say, people felt...the faculty and students were opposed to the Vietnam War in general.

COHEN: Were you on the faculty...? Let me get this right. Were you on the faculty of the College of Arts and Sciences?

KOMISARUK: Yes.

COHEN: Okay.

KOMISARUK: I was a member of the Zoology Department. That was my appointment, in Zoology.

COHEN: So you were on the faculty of the College of Arts and Sciences. So you were involved with everything else. Well, for instance in 1970 when...after the invasion of Cambodia and the killing of the Kent State students there was a moratorium ordered... Do you recall that time? In May, I believe, it was called off. There were teach-ins.

KOMISARUK: What was the...?

COHEN: A moratorium on classes in the spring of 1970. And it’s still there when you talk to people about the moratorium generally ... whatever was going on.

KOMISARUK: I can’t recall specifically, but in general I was sympathetic to any actions that were done to protest the Vietnam War.

COHEN: What did you think were the appropriate roles of the faculty members in the debate? Here on the campus or speaking in a forum against it.

KOMISARUK: I felt that the war was morally wrong. And I was actually—I first heard about the Vietnam situation, first read about it in I think around—probably 1960, ’61. In fact I recall

the very first information I had about Vietnam, the Vietnam situation, was in a publication, a fascinating publication by M.S. Arnoni, called *The Minority of One*. And he had a section, two columns, in his publication. It was a newsletter something like *I.F. Stone's Weekly*, and it was larger. And it was written almost entirely by him, *I.F. Stone's Weekly*. And he had a section in there called "Things of Which I Am Ashamed, and Things of Which I Am Proud," separate columns. And things of which I am ashamed, he said, the genocide war in Vietnam. And I said what is he talking about? This was about 1960, '61. And this was before anything was in the newspapers about Vietnam. I would ask people, and nobody knew what this was all about, the genocidal policies of the American government in Vietnam. So I was sensitized to it, and I was reading about it, and I was following it all along. And I was always opposed to our government's position. And so I can't remember details about the moratorium, but I'm quite sure that I was in favor of the faculty and the students taking a position that was morally opposed to the war on moral grounds.

COHEN: Do you remember any of the demonstrations on campus, confrontations of, I believe, one confrontation of these groups with police? Do you have any recollections of such scenes on the campus?

KOMISARUK: No. I was a beginning assistant professor and deeply involved in my research. And I pretty much had my nose to the grindstone and wasn't involved in a lot of political activities. You know I was sympathetic.

COHEN: What do you recall, again, about events dealing with activism? What do you recall about the—now going back a little bit—the riots in Newark in 1967?

KOMISARUK: Well, I was in....

COHEN: July of '67.

KOMISARUK: I was not here. I was in...that summer I was away doing work with James Oldes[sp] at the University of Michigan.

COHEN: Oh.

KOMISARUK: And so I wasn't here during the riots.

COHEN: Were you on campus in February of '69 when Conklin Hall was occupied by the Black Organization of Students, members of the Black Organization of Students. Do you recall that?

KOMISARUK: Yes. Very dimly. I wasn't really involved in that.

COHEN: Do you have any recollection of what the grievance was that led up to that?

KOMISARUK: Not really.

COHEN: Any recollection of the—

KOMISARUK: Can you refresh my memory on that?

COHEN: Well, do you recall a number of students taking the building on February 23rd? Chained the doors and took over Conklin Hall, wouldn't let people into the building. There were demands that had been presented previously to the administration ...was admissions. Do you have any recollection of discussions, debates, and so on about admissions and admitting—

KOMISARUK: Open admissions.

COHEN: The term that was used by, if I recollect...it was for really much more open, much more open, greater access. Do you have any recollection of the discussion?

KOMISARUK: I have a recollection of discussing admissions with some of my colleagues, Helen Strasser and some of the people in the Zoology Department. And I was in favor of more open admissions. As I recall, the counter debate was—or the counter argument—was that it would lower the standards of the university. But I felt that it was more important to make the university more accessible to more people. And that was my position.

COHEN: Yes. Okay. How did the people who were supporting greater access for minority students, how did they square that position with, on the other hand, the position of maintaining university standards? How did they address that perceived conflict?

KOMISARUK: The way that.... The constructive approach, I think, was to make the university more accessible and— [Break in recording]

COHEN: Okay. We're back. And we were talking about the open admissions debate after the takeover of Conklin Hall.

KOMISARUK: I think a constructive solution is to make the university more accessible to more people. And provide additional training for them to bring them up to a competitive level in college; acknowledging that some disadvantaged students might not have had the adequate background of other students. And the basic intelligence is there, but the training is not. And so it just requires some additional work. And I think the role of Academic Foundations shows that this kind of approach can work and the students can succeed.

COHEN: What's your perception of the role of Academic Foundations, of its effect on the university in Newark?

KOMISARUK: I don't have any data on which to base it, but from the descriptions of it, it sounds like it does a good job. And I don't have that much contact with the undergraduates here except the Minority Biomedical Research Support program.

COHEN: What is that?

KOMISARUK: This is a program that is funded by the National Institutes of Health through the Division of the National Institutes of General Medical Sciences. And it's a large grant. I'm the program director of the grant. We're now in our eighth year, and it has 12 faculty and about 20 students in the program, about 12 undergraduate and eight graduate students. And the principle of the grant is to give the minority students the opportunity to work with a faculty member on a research project and become—learn how to do research and present at a conference as a coauthor and to get these students hooked on doing biomedical research. They get a salary, a substantial salary, and supply money, travel money to go to conferences. And overall the grant is over \$400,000 a year in direct costs. And we've had a lot of success in graduating.... Undergraduates get bachelor's, go on to graduate school, medical school. We have students getting Ph.D.'s and masters.

COHEN: How do students qualify for that program?

KOMISARUK: They need a B average and letters of recommendation. And they have to be able—willing—to put in 15 to 20 hours a week doing research.

COHEN: And with an ethnic or racial background of some kind?

KOMISARUK: It's primarily black and Hispanic.

COHEN: Mm-hmm. But how does the program define the ethnic or racial background?

KOMISARUK: The students who qualify according to the definitions of the National Institutes of Health are black, Hispanic, Native American, and Asian/Pacific Islanders.

COHEN: Maybe I asked this. How long has this program been going on now?

KOMISARUK: Our program is now in its eighth year. But the program was started 19 years ago at the NIH.

COHEN: At the NIH, I see. Well, how many students, roughly, do you think have passed through this program, just came through it aside from there.

KOMISARUK: At Rutgers—

COHEN: Rutgers in Newark?

KOMISARUK: Rutgers in Newark, yes.

COHEN: Yes.

KOMISARUK: This Minority Biomedical Research Support program, MBRS program, there are about a hundred programs like it in the country. We on the Newark campus are the only one in New Jersey. We have passed through approximately 70 students since we started. And at the national level, I think there are something like between 1500 and 2,000 students in the program

currently. I think that's about it—something like that. I don't know what the totals are overall, but I think there's something like 1500 students who have gotten doctoral degrees nationwide.

COHEN: Nationwide. What can you say about the students who've been in this program? What is the setup, the level of preparation that these students bring to the college when they come in?

KOMISARUK: It varies. But the students are bright, and they're motivated. And they do good work, and they get out to good schools. We've recently had students have going to Yale and Rockefeller and Penn State Medical School. They do quite well.

COHEN: Do these students have any kind of contact with the Academic Foundations Department? Former contact.

KOMISARUK: They may have had former contact. We don't have any—there's no formal contact between the MBRS program and the Academic Foundations, although we're currently working on a large project to establish a pipeline from the high schools through to graduate school, to have an integrated program where the different high schools in Newark and Essex County College and Rutgers to go on with the program of research to get the students involved in research early in their careers. Even in high school get them hooked on research and science, and give them an opportunity to work in laboratories and see what it's like to really be a scientist, and to hire students, graduate students, helping undergraduates, undergraduates helping high school students, and bring them along. Because one of the major problems in education of minorities currently is the tremendous attrition rate. It ends up as a trickle of students going into graduate school. And what we're trying to do is have a close-knit, personal interaction to encourage the students at each step of the way so they don't fail out at transition points.

COHEN: Why are the attrition rates so high?

KOMISARUK: Well, I asked this question—I was on a site visit to the University of New Mexico a couple of months ago. And I asked the students we were meeting, an affiliate MBRS program, in Washington; we met with about 25 students. And I asked them why—how many of you are planning to go on to careers in science? These are all students in the MBRS program, and nobody raised their hand. I said, “Why not?” These were students from all over the country.

COHEN: These are students at the University of New Mexico.

KOMISARUK: Yes. They're in the MBRS program.

COHEN: Yes. Okay.

KOMISARUK: And nobody raised their hand.

COHEN: Yes.

KOMISARUK: And I said, “How come nobody wants to go into biomedical research? Is it too much work? Who thinks it's too much work?” Nobody raised their hand about it being too much

work. So then is it that the salary's too low compared to other careers? And nobody raised their hand. I said, "What is it then?" And one of the students said that they hear that it's impossible to get grants. And you need grants to do your work. And I said, "How many of you feel that way?" And half the students raised their hands. So somehow they got the word that the grant situation is so terrible that it's hopeless to try to get support for doing research. So they don't want to go into it because it's too frustrating. They're students, and they already got the word that it's too hard to get—it's too hard a career. There's not enough support for them.

COHEN: And what year was this meeting with these students in New Mexico?

KOMISARUK: This was a few months ago.

COHEN: Just a few months ago. Okay. So the program you say is eight years old. It started out at the beginning of the eighties.

KOMISARUK: There's that brochure.

COHEN: Uh-huh. This is a brochure. Minority Biomedical Research Support. "Salaries for biomedical research training for minority undergraduate and graduate students. Rutgers, the university, is funded by the National Institutes of Health." May I have a copy of this? I'd like one very much. Do you have any perceptions at all of—aside from the students participating—the level of preparation of the students, in the university in Newark generally?

KOMISARUK: Well, it's really hard to say. The students in the Institute of Animal Behavior are bright. And I think that their level of training is not that good. I don't think they're that good as undergraduates. When we see them, they're graduate students. And I don't think they get that good an undergraduate education, by and large. Some do. I think there's not enough.... One of the biggest problems that I see is that they have not read enough. Now another big problem is that they have not been trained how to think scientifically. And they want to know what the facts are. But they have difficulty recognizing how to know a fact when it hits them in the face. And this is a big problem with graduate students. I found this going on, that there's a big transition, big change that has to occur, between a student mentality and a scientist mentality. And that is that the student, a good student, is a student who learns the material and can represent the material that is taught. But that doesn't provide training for being an investigator. And the remarkable thing that I find is that when a student makes a discovery, they don't appreciate it. They don't know what a discovery looks like when it happens. They haven't had any background. I don't think they've had adequate background in interpreting data, interpreting the results of experiments, comparing controls with experimentals. So they have a lot of difficulty in recognizing a phenomenon when it occurs. And they have to be, they really have to be told that this is something important because they don't have the—they haven't been given the tools—I don't know what it would take. But they haven't been given the experience to know what it takes to recognize a discovery when they've made it.

COHEN: You're describing the current situation?

KOMISARUK: I'm describing a chronic situation.

COHEN: Chronic situation.

KOMISARUK: That I've experienced over the years.

COHEN: Yes. Okay.

KOMISARUK: That it's kind of humorous to see. I find myself being in the position of having to convince the students that they've made an important discovery. And because it's almost like they're in the position of emphasizing all the exceptions. But this one didn't do that, and this point—this individual didn't go along with that. And they don't, even when they do statistical analysis and they come up with a significant difference between groups, they tend to emphasize differences, individual exceptions, and have a lot of difficulty accepting the fact that they have made a discovery. Somehow they have a feeling about a discovery coming in some kind of foreign way in a published paper with a headline on it. But what really happens in your hands, in front of their eyes, they don't know what it looks like.

COHEN: Now. Alright.

KOMISARUK: I feel it's my job.

COHEN: Your job.

KOMISARUK: My job is to try to show students what a discovery looks like.

COHEN: Right. You say this is a chronic situation. Is this a chronic situation in Rutgers in Newark?

KOMISARUK: No, I think it's a chronic situation among graduate students, period.

COHEN: Period.

KOMISARUK: Nationwide. I think it's—that's what I mean. It's a transition from a student mentality to an investigator mentality. Because an investigator goes—I don't think the students are trained how to investigate. They're trained how to learn and rehash and give back the data. But they're not trained in how to convince themselves that they have found something. And I feel that's my job. And I run into problems with students who feel that—they're not being taught what they think they should be taught. And I feel that—I think they should learn certain things. So sometimes we run into some misunderstandings in the teaching process.

COHEN: What do they think they should be taught?

KOMISARUK: Facts.

COHEN: Facts.

KOMISARUK: They think they should be taught the material, what they have to know and the way things are.

COHEN: And you are teaching them?

KOMISARUK: I'm teaching them that, but I'm also teaching them how to recognize a fact without telling them it's a fact. And it sounds peculiar. But it's the question of how do you know a fact is a fact? Is it because somebody important says so? Well, if nobody—what if there's nobody there to say that it's an important fact, how do you convince yourself that it's an important fact if nobody tells you it's an important fact? And the students don't have any training for that. They don't know how to do that. It sounds peculiar, but that's what I mean by an investigative mentality. They don't have—they haven't learned what to do to come to a conclusion themselves that a fact, that they may have observed a fact, that a fact is there.

COHEN: Yet you learned this at one time and continue.

KOMISARUK: Yes.

COHEN: Why did you learn it and these students don't?

KOMISARUK: Well, I think eventually they do. But it's a very interesting process to watch them not recognize it at first, and then eventually start to recognize. Eventually start to believe their own data and to accept their own data, that they have made a discovery.

COHEN: Now would you say that this is more of a problem, let's say, in the seventies and into the eighties with the students that you're dealing with, than it was let's say 25 or 30 years ago? Is it possible that...?

KOMISARUK: I think that one thing that is a problem is that I think in general the level of literateness is declining. Unfortunately. I think that the students—well, just the basics of grammar and spelling and sentence construction and syntax is worse now than it was 20 years ago.

COHEN: How does that, the literateness, relate to the ability to what you were talking about before: recognize the fact as a fact. Is there any link between—

KOMISARUK: I don't see any link there. I just think that it's a different phenomenon. I think there's too much television. I think we're seeing in the students now the effect of television, that there's not enough reading, and too much— They get too much verbal communication and not enough written communication in their background. And I think we're seeing the effects of that. Graduate students have trouble spelling. Many can't construct sentences properly.

COHEN: Mm-hmm.

KOMISARUK: But that's a different aspect, the mechanics of it.

COHEN: So what effect does that have on research and productivity?

KOMISARUK: I don't think it—it slows them down a little bit. But basically I think the ability to do research is about the same, it's been about the same. I don't think that's deteriorated. I think one problem that I do see is that the students I find are becoming somewhat more intimidated against speculating. This is troubling to me. I think there's an attitude of competitiveness in general, and a few— There's less of a spirit of playfulness now than there was 20 years ago. It's more... The science seems to be... The students are picking up a message of science being a business, more now than it was 20 years ago. And they feel that they can't really play with experiments or with ideas. And they have to be rigorous and scientific. And I think that creativity is being compromised by it, dismayed by that trend.

COHEN: Why is this happening, do you think?

KOMISARUK: Because there's a premium on productivity, more so now than there was; that it's important to produce papers, publications, small projects. Grants now—premium on a grant is to propose a few experiments to nail down an issue, rather than to propose an area of investigation, a series of experiments on an area of investigation. It's much more short range now, much more focused on defining an objective. There's much less playfulness now. And I think there's a whole attitude in business of going for short-range projects and not going for long-range planning. I think science has caught the same virus of let's get short-term results rather than long-term bigger questions or just.... There's not enough—there's an attitude that there's no time to speculate, be more playful in your thinking. We can't afford that. We've got to get down to business. And I think that science suffers from that because there's got to be an attitude of playfulness, of not really knowing what you're looking for. And in some way something gels and you make some connections, something happens. And you can never really predict it. But when something like that happens, very often it's an insight that solves problems that you could never have predicted before. But it's hard to justify that when money is so tight, and everybody's expecting results.

COHEN: When did this situation begin to develop, this attitude toward science? What years?

KOMISARUK: I think it's been developing gradually. As science becomes bigger and more businesslike, I think it's just been—it's a development that's gradually occurring. And I think students are becoming a more businesslike attitude, more serious. There's nothing wrong with being serious, but it's a fear—there's a fear that they have to do something and show something. And it's high pressure, but it's not necessarily greater creative productivity.

COHEN: How has this situation affected faculty in the tenure stream, who have not yet achieved tenure?

KOMISARUK: It affects them directly because it's the same. It's even worse for the faculty. They have to show productivity to get tenure. They have to publish papers and get grants. And there's a lot of pressure on young faculty to produce papers and come up with facts. And what often happens is that the facts are relatively trivial facts. They're facts, but it's under great duress. And sometimes, you know, in some kinds of areas, that's okay. But in other kinds of

areas, it kills creativity and kills bigger ideas. It's unfortunate, but that's.... I find that the.... It seems to me that the students are—by osmosis they get the message, that they should not speculate, and they should not, that they shouldn't have wild ideas and far-out ideas. But in reality I find far-out ideas very interesting. And I think it's a way of focusing actual experience.

COHEN: How has this affected actual actions on the campus in promotions? How does this affect decisions and recommendations for promotion to tenure on the campus? That on the one hand you recognize that these expectations can cramp creativity, and on the other hand it's through the demands of the university that bibliographies grow long and longer. How does that enter into deliberations about promotion to tenure?

KOMISARUK: Well, productivity is a major factor in promotion, and productivity—it's hard to...it's easy to count the number of papers. It's hard to define the significance of somebody's findings.

COHEN: How is it that—is it an attempt made to do that? This is to weigh one paper against another paper or this paper is worth ten other papers or something like that? Anything like that happens? I've never understood that.

KOMISARUK: It's a very tricky business, very capricious. The evaluation is very capricious. If people on the committee think that the work is important, then—or if people who are the outside referees think that the work is important—then that could count more than the number of papers. But it also depends on opinions of people, and that's very capricious. Another standard is the number of citations that are made to a person's work.

COHEN: Ah....

KOMISARUK: In other words, to assess the impact—that's taken as a measure of the impact of somebody's work on the field. But even that is capricious because sometimes an important finding doesn't get the publicity, and it's not cited. But it's an important finding nevertheless. I mean it takes many years before the rest of the world comes around to recognize it as important. This whole question of recognizing things that are important is very, very tricky, very capricious.

COHEN: To what extent has the institute suffered in the tenure wars in the seventies and into the eighties?

KOMISARUK: We've had a small faculty. There are only seven faculty members, and everybody's tenured. So everybody got tenure and that hasn't been a big problem. We've had—it hasn't been difficult in general for the faculty in the institute to get tenure. [End of Tape #1]

COHEN: We're back. Now we were talking about the—we were discussing and talking about the Minority Biomedical Research Support program, within the context of affirmative action. And you were saying some of the problems that you perceive down the....in administering it?

KOMISARUK: Yes. I believe fairly strongly in the goals of the program, which is based on a reality that the minorities are severely underrepresented in the biomedical sciences. On the basis

of the proportion of minorities in the population, the proportion of minorities in biomedical research is much lower than their proportional representation in the population at large. So there is a problem, a serious problem, there. There's also a problem in that the minority population in the country is growing; however, the proportion of minority people who are going into biomedical research is not. So there's going to be fewer and fewer people going into biomedical research. And we need people in biomedical research. So the objectives of the minority program—and this is, the budget is, on a nationwide basis—is about \$40 million a year to encourage, to pay students' salaries to go into research and get them interested in going into these careers in biomedical research. Those objectives are laudable, and I think they're excellent objectives. The crunch is that the—as resources, financial resources, in general become restricted, the non-minority students feel that the minority students are getting an advantage. And this problem is, in a microcosm, this is a problem that the country is facing on a much larger scale. The need for affirmative action give incentives in certain areas where there's underrepresentation. But then simultaneously the reaction of the people who are not entitled to those incentives feel that they're losing out. And I don't know what's fair. I don't know what's just in that kind of situation. Incentives are simultaneously designed to achieve an objective to compensate for a problem, and simultaneously they are perceived by those who are not eligible for them as an unfair advantage. So how do you—you know, what's fair? What's the right perspective on the whole issue? It's inherently unfair, but it's also inherently a way to compensate for a problem.

COHEN: How does this impact on the way the program is run in Newark.

KOMISARUK: There are strong, excellent incentives for the minority students to come into the program. Undergraduates get \$6,000 a year as a salary to work in the laboratory. And the graduate students get a regular graduate student salary. And plus there are incentives such as supply money and travel money and publication money. So there are feelings that...I've heard people, non-minority students, express—grumbling—that they're not entitled to the same kind of inducements and lucrative opportunities.

COHEN: And there's no mechanism—any mechanisms set up to deal with these kinds of things?

KOMISARUK: Well, there are other opportunities. But there's no question about it that the minority program is a program designed as an incentive for minority students. So it depends on the range of your view. And I feel comfortable taking the view that the country needs more biomedical researchers, and I'm helping to focus on a certain group that's targeted for encouragement to come into this field.

COHEN: Yes. Again, the area of affirmative action. Back in the seventies, there was a class action which was entered into by the women faculty on campus, claiming discrimination in promotion. And I was wondering what your perception of their claims?

KOMISARUK: I think that they had a legitimate claim. The statistics on salaries for women of comparable background and training, they received lower salaries on a statistical basis than men of the same, of comparable background: number of years as faculty members, age, as

comparable objective standards as possible, and there really are differences. And I was just reading an article in the paper last week that the situation is a little bit better, but it's still in the same direction. And there's across-the-board for comparable years of training and comparable background and comparable—all things being equal, white men earn more than white women earn more than black men earn more than black women.

COHEN: Mm-hmm.

KOMISARUK: There is that gradient that exists now. This was in the *Times* just about a week ago.

COHEN: In the area of race relations on the campus, any perceptions of how things have changed, progressed, through the seventies and into the eighties?

KOMISARUK: I think that a major change that everybody perceives is that there are fewer native-American graduate students and far more foreign-born graduate students. That the professional future in academic areas, of native-born Americans is going down. And foreign students from China, India, other Asian countries, from the Eastern European countries, these students are filling the brain gap. And I think it's a dangerous trend in which this country—I feel that this country is becoming anti-intellectual. Has become anti-intellectual, and we're seeing the consequence of it. I'm not opposed to foreign students getting into intellectual activities and professional activities. I am seriously dismayed by the fact that the native-born American students are dropping out of that endeavor.

And the reason for it is that there's no premium placed on it, no respect for it in this country, in this society. That I think—In fact I think it may be a result of the—this is just a speculation—but I think it may be a result of the Vietnam War, where I think this was the first time that the government was burned by the universities; it received severe criticism from the students and the faculty in the universities. And I think it generated an attitude of hostility toward the universities on the part of the government. And it's been reflected in the reduction in the student loans and subsidies, making it increasingly costly for students to go to college and university. And it's as if the government has taken revenge on the students for criticizing it during the Vietnam War. And the result of it is that we're losing the university population of native-American students.

So the professions—So it's turned into more of a service-oriented nation and not an innovation nation. I think in the long run, we're going to suffer because the native-American population is not going to be competitive in the world intellectual community with innovations in science and technology. They're just not going to have the brain power. The students who are coming from foreign countries will very likely tend to go back to their countries and take the advantage of an American education with them and develop their own countries. And we're going to be left with a service-oriented society, and we're going to be—we're going to be trumped in the coming decades. There's not going to be an intellectual—there's not going to be a brain bank in this country. The government is not supporting basic research. It's supporting fast-buck investments and short-term investments. There's no long-term planning, and there's no long-term investment in this. It's all part of the lack of exploration and investigation and

playfulness, this whole thing. It's too much of a businesslike, fast profit, immediate goal, and we're going to get screwed in the long run. That's my opinion.

COHEN: And yet you were instrumental in getting the Center for—

KOMISARUK: Center for Molecular and Behavioral Neuroscience. [Laughter] And I fought hard to have the word “behavioral” in there against opposition from faculty, everybody, but I finally got it in there.

COHEN: Okay. Do you think you can tell us about the—going back to the beginning, the genesis of the idea which you were telling me about over coffee and cake before, which I found fascinating.

KOMISARUK: This was around 1982 when I saw in the *Star-Ledger* an article stating New Jersey was developing—wanted to develop—six new centers of excellence and had already set up five, committed itself to five, which they have yet to specify the sixth. And the five included a few technology and research and waste management research and ceramics and several others—computer I think was one of them. And I thought that it would be.... We could develop a—we had a nucleus of people interested in the neurosciences, and the neurosciences was becoming a major force in American science, which I've actually realized this decade because Congress has called the 1990's the Decade of the Brain. So with that in mind, and since we have.... Let me go back to when I first came here with Danny Lehrman offered me a job in 1965 before I went on my postdoc, I said, he wanted me to come back to the institute. And I said, “I'm really interested in brain mechanisms, and nobody here is interested in brain mechanisms. And, you know, if I come back here, I'm afraid I'm not going to have anybody to talk to.” And he said to me, “Well, if you come back here, then you can bring other people here.” And I remember that conversation very vividly. And so while I've been working here, I've brought people in from UMDNJ, doctor and got them appointed associates in the institute because a few people in neurosciences, Alan Siegel and Ben Navenson [sp] and Barry Levin...and I set up, with the help of the provost, the Mexican Exchange program with the Center for Advanced Studies in Mexico and the Center for Reproduction in Animals at several different universities: Upsala and National Congress University in Mexico and the Center for Advanced Studies at the University of Vera Cruz. Bringing people interested in brain research and neurosciences together. And we have people coming here.

And there was a growing interest in neurosciences. So on that basis, I suggested to Harvey Fader, who was then the chair of the provost's taskforce, that maybe we should work through the thinking of what we should be doing. And I suggested that we try to work for a neuroscience center to get a critical mass of neuroscientists here on campus. And we discussed it among a number of people on the taskforce, people from the Institute of Animal Behavior and chemistry and biology, and we thought that we'd give it a try. And the way that—we had several meetings about it. And I said I would bring it up to President Bloustein, and this was in 1984 when I got the board of trustees research award. And this would be an opportunity to see Bloustein. So as he was handing me the award, I said, “We have a bunch of—we have a group of neuroscientists on the campus, and we're thinking of maybe trying to develop a new center. Would it be possible?” He said, “Let's talk about it.” So we rode back together, we got in there,

we drew up some plans and spoke to the provost and to the dean. And I remember we had several meetings to try to decide on what the name of this center should be. And it started as the Center for Molecular Neuroscience. And I fought hard to have the word “behavior” in there; people thought it was too diverse, too opposite poles, molecular and behavioral. But I said, “That’s what we’re trying to do. We’re trying to integrate as many different areas as possible, and this would include everything.” So we finally decided to keep that. And then it just gained momentum, and we wrote several proposals. And then the administration asked us to—said that they would go ahead with the proposal if we would start recruiting an outside director. And so that was—it just kept gathering momentum. And we had various stages along the way. We kept getting various kinds of approvals for the different stages until it became a reality. And the Crease [sp] and Tamara were interesting to us because it was a husband and wife, both professionals, both well-respected in the field, and they—Crease was on the molecular side and Tamara was on the behavioral side in cognitive neuroscience. And they were both doing interesting work. So we decided that they would be the most appropriate as the outside directors.

COHEN: How were the Aidekmans brought into this?

KOMISARUK: I’m not sure exactly. I know that they want.... The work that Crease is doing is on dopamine receptors and those involved in Parkinson’s disease. And Mr. Aidekman was a victim of Parkinson’s disease. And I don’t know the details about it, you’d probably have to ask Crease about it, but somehow Aidekman made an offer to donate I believe two million dollars to the center if he could have his name attached to it. And that was worked out.

COHEN: From the genesis of the idea goes back to the....

KOMISARUK: Nineteen eighty-two, I would say.

COHEN: The original idea was dropped in about 1982, you think?

KOMISARUK: That’s what I—yes. That’s when I—I remember a specific conversation, and I remember where the conversation took place. I said to Harvey, “I think it would be nice to have a—why don’t we try to get a—I saw this article in the paper, and are you interested in building the sixth center? Who don’t we try to make it the Neuroscience Center?”

COHEN: Let’s see. I wanted to get into the areas of the— your perceptions on various administrations. Let’s start from the top down, the administrations that you have worked under. Mason Gross. Let’s start off with Mason Gross, president of the university to 1972, I guess. Any perceptions of his—

KOMISARUK: Benevolent. I was really very much involved in my research, and I kept my nose to the grindstone. I didn’t have time for anything having to do with the administration, and I was insulated from that. Dan Vernon was affected. He was a brilliant administrator, and he loved doing it. So he would just provide resources and provide a free-thinking environment for doing science. And having people come in there, and it was just a very vigorous and intellectually stimulating scientific environment. And so we were insulated from the rest of the Rutgers-

Newark. We were a world unto ourselves. And I really didn't have anything to do with the president or the dean or the provost. They were just remote figures.

COHEN: How did—if you could give us any more detail and more concrete, how did Daniel Lehrman generate this environment? You mentioned before his classroom technique when you first encountered him at City College. But what were his methods, if you can give an example, of how he did all this? He's a legendary figure.

KOMISARUK: He had a way of creating suspense and mystery and having the audience come to the conclusions. He could set up the experiments, and he just had a brilliant way of putting a spell on the audience. He had a spellbinding manner of presenting research as a mystery that the audience could solve. So everybody had the experience of being a genius, listening to him. [Laughter] He had an incredible talent for making people feel that they're geniuses.

COHEN: Yes, yes.

KOMISARUK: You know to be able to figure out and come to the conclusion. And he also—The experiments that he did—He loved animal behavior, and he loved to tell stories about experiments and about adaptations of animals and, you know, just—He had an encyclopedic mind. He knew incredible amounts of things. And he could always recount fascinating stories. And the adaptations of animals to their environment, the infinite numbers of adaptations. And these stories of how they adapt are fascinating. And he just told them as these fascinating tales. Just thinking of random species of—one species of bird has cone-shaped eggs. Why are they cone-shaped as opposed to a chicken egg which is oval? And this is a species of bird that lives on narrow cliffs, narrow ledges on cliffs. So they lay their eggs on the ledge that's just a few inches wide. So if an egg goes out of the nest, if it's cone-shaped, it can't—it just rolls around in a semicircle. It can't roll off the cliff. But if it was an oval-shaped egg, it would roll off the cliff.

COHEN: What species is that?

KOMISARUK: I can't remember the species. [Laughter] I can check on it. Cliff—a ledge-dwelling bird. And just that kind of thing. But he just had a million of those. [Laughter] And it was just fascinating to listen to him talk about the adaptations and mechanisms. And he was a spellbinding speaker.

COHEN: Do you have any recollections of at all or any experiences at all of how he used these in the faculty wars? Namely on the admissions....

KOMISARUK: Well, it was a question of....

COHEN: That emerged after the building takeover of Conklin Hall?

KOMISARUK: I don't know about admissions. I recall.... I don't think he had anything much to do with that, admissions. I don't recall any...

COHEN: Other faculty wars? Promotions...?

KOMISARUK: Oh, sure.

COHEN: The Vietnam War?

KOMISARUK: Well, I think that he took a very courageous stand against the House Un-American Activities Committee in that era when he refused to testify to them.

COHEN: What year was that? Fifties, sixties?

KOMISARUK: I think it was the fifties.

COHEN: Uh-huh. Okay.

KOMISARUK: But I wasn't here at the time. I just knew that he had done that, which at the time took a lot of guts. And I know that he made speeches opposing the Vietnam War at various times on the campus. And he was a brilliant orator, and he was also it was a delight to listen to him work the phones with the administration. It was fascinating to listen to him cajole and humor and intimidate and just generally get his way with the administration. [Laughter] It was great.

COHEN: The first—well, Malcolm Talbott was appointed vice president of the Newark campus in 1965. Oh, Let's talk Malcom, 1965. Do you have any recollections or things to say about his administration?

KOMISARUK: Nothing at all. I really don't know anything about his administration. My first dealings with the administration I would say came later with Nathanson as dean and Samuels as provost. My first dealings with Samuels was to ask him to support an exchange program with Mexican institutions. And he was very supportive of that and he continues to support.

COHEN: After Malcolm Talbott—well, he was acting dean. Any recollections of the deanship of William Gilliland?

KOMISARUK: Gilliland.

COHEN: Yes.

KOMISARUK: No. Nothing at all. I remember there was some scandal, but I never really....
[Break in recording]

COHEN: Ok, We're back. You're aware of some problem with the Gilliland administration.

KOMISARUK: Yes. I don't know what the problem was. I just knew that there was a problem.

COHEN: And Malcolm Talbott served as the acting dean while he was also vice president. Henry Blumenthal's administration in '69 to '71. Any feel for that, what was happening then?

KOMISARUK: No. The only thing I remember about Blumenthal was that when Danny died in 1972 and we had a memorial service, Dean Blumenthal came over to me and said that we should be sure to do something quickly to get the university to remember Danny because memories fade quickly. And the scholarship was set up in his memory.

COHEN: Oh!

KOMISARUK: We give that. And then when he died, I got a phone call from Jonas Salk because Danny was an associate of Salk Institute. They had him out there very frequently because they were very interested in adaptation and immunological evolution. And Danny was an expert on evolution and adaptive mechanisms. And he was very well liked. And he was an associate there, along with Francis Crick—

COHEN: Oh!

KOMISARUK: —and Jacques Bineau [sp], Nobel Prize winners. And so he rang one day, and it was Jonas Salk, and he said that he wanted to convey his condolences. He was very fond of Danny, and it was a terrible personal loss to him, and he wanted to tell somebody.

COHEN: Do you have any recollection of or association with Jacob Bronowski and his book, *The Ascent of Man*, and the TV series.

KOMISARUK: I know he had a lot of conversations with Bronowski. And Bronowski came to visit them a number of times.

COHEN: On the campus?

KOMISARUK: Yes, they were friends. But I was never involved in the discussions. That was something between them.

COHEN: Yes. Was there any thought ever given to name the Institute of Animal Behavior after him?

KOMISARUK: There was. There was. I would still like to do it. I think it would be appropriate. And we've never done anything about that really. It's a good idea, though.

COHEN: Because there are named buildings throughout the campus, you know, all around the campus now. Blumenthal, Stonsby, Talbott, and Woodward and....

KOMISARUK: Smith Hall.

COHEN: Smith. Oh, yes. That's so cool.

KOMISARUK: Ned Smith. They named the whole Smith Hall, yes.

COHEN: Smith, Conklin, Boyden, Ackerson.

KOMISARUK: Engelhard.

COHEN: Engelhard.

KOMISARUK: Dana Library.

COHEN: Dana.

KOMISARUK: Robeson.

COHEN: Every single building is...Olson for chemistry. And yet that's....

KOMISARUK: Aidekman Research Center.

COHEN: The Research Center. I realize it's got a home in Smith Hall. While we're on the subject of buildings and Smith Hall, [laughs] got around to it, what can you say about the whole question of the ... in the whole chronology of... on charges of pollution? There were two reports. What can you say about that?

KOMISARUK: Well, those really it was a scare. There were different issues. One was an issue of whether— There were two main issues. One was—there were two charges. One charge was that there was an inordinate number of illnesses in Smith Hall, period. The other charge was that there was contamination of the building by estrogen. And there's a Health Department study, analysis, made of the illnesses in the building compared to other buildings. And the conclusion was that there was no significant difference between the illnesses, the incidence of illnesses, in Smith Hall compared to other buildings on the campus. So nothing...it's a small circle, so it's always, it's very difficult to come to any statistical conclusions. But the conclusion of the Health Department was that there was not any greater incidence of illnesses in Smith Hall than other buildings. The question of contamination by estrogen: There were the National Institute of Occupational Safety and Health Group did surveys on the building. And they stated that in some areas there were high concentrations of estrogens. But they made a lot of objective errors in their measurements, which were readily in their calculations. And so there were a lot of problems with their report. And I recall...to deal with these two—I showed them as two separate issues, and I drew up a list of illnesses that are caused by estrogens and a list of illnesses that were recorded in the building to see if there was any match between the two. And there was no match. In other words the kinds of illnesses that were reported in the building were not the kinds of illnesses that are caused by estrogen. And the kinds of illnesses that are caused by estrogen were not the kinds of illnesses that were found in the building. And that was one report that I gave to [?]. And then I called up Harvey Fader and got up a list of some of the methods of analysis that we used by my Nayash[sp], and got comments from other people in different universities, basically criticizing the Nayash [sp] Study. So by and large, I think that there is no substantial evidence of a hazard due to estrogen causing illnesses in the building. So basically it was...while there were illnesses and there were some places where there was estrogen, there was no connection and no connection was ever really established.

COHEN: How did the people on the faculty in the interim deal with the public part of this whole thing, the newspaper, the *Observer*, discussion. How was that handled with public relations if you can recall?

KOMISARUK: Well, there was a lot of press coverage, television coverage, and it tended to create more hysteria but not based on a balanced picture. You know the television crew would come in and take a picture of dirty cages, dirty animal cages. And you know that kind of, it doesn't—it looks bad, but it doesn't really mean anything.

COHEN: Television crew. Where were the pictures shown?

KOMISARUK: On Newark news programs.

COHEN: Is that right?

KOMISARUK: Yes. All the major stations at one point or another came in and did stories on the institute. So there was a lot of innuendo, but the data never really substantiated any significant incidence or health problems or any substantial health hazard.

COHEN: In this report...you said you issued—did a report comparing, trying to match the data with estrogen, okay. How was that handled by the university Public Information Office?

KOMISARUK: I never heard anything coming out of the Public Information Office. It was just a very—I handed it to Al Pine.

COHEN: Just Al Pine?

KOMISARUK: Yes. He was interested in it. I don't know what he did with it. I know that there were arrangements...there were many lawsuits that were evidently settled one way or another. But there was never any real information forthcoming publicly about how that was all dealt with.

COHEN: How did you feel being in the center of this?

KOMISARUK: Well, until we had the information, it was very stressful because we didn't know if there was in fact...we didn't know whether or not there was in fact a health hazard. You know some person had lymphoma, and there were various.... One person had breast cancer. And one never knows what to make of it. You know what is a significant incidence? We had a few people getting various kinds of illnesses, different types of illnesses, you know. We had a lot of discussions as to what the proper population to compare it with: Is it the population of Newark? Is it the population of Rutgers? People in the building? There were all kinds of...we spent a tremendous amount of time with, you know, cooperating with the Health Department, with NIAS [sp], developing questionnaires, developing analyses. Everybody was in it because we were all scientists, and they were cooperating with us, we were cooperating with them, trying to figure out what the appropriate controls are; and, you know, should it be in terms of how long people were in the building, how many years they spent in the building or how many hours per day? What if I teach one class in the building? Millions of problems. And, you know, who to

interview, who to examine? What kinds of tests to do on people? There was a tremendous amount of discussion about all this stuff. And, you know, in the midst of it, you don't know if you're at risk or not. So it was very stressful to everybody. And some people saying, don't worry because there's nothing to worry about. And other people saying, there's definitely a health hazard.

COHEN: So what were your final conclusions?

KOMISARUK: My personal conclusions?

COHEN: Yes, yes.

KOMISARUK: My personal conclusion was that there were definitely people with illness in the building, but there didn't seem to be any kind of concentration of any particular kind of illness that was greater than any other place on the campus. And there was no specific hazard ever identified that could have caused those illnesses. So I came to the conclusion personally that there was no issue. It dissolved in my mind. One never knows. But, you know, it's a small sampling; you can't really do an experiment on it. But that was my personal feeling about it.
[Break in recording]

COHEN: We're back. After talking about Smith Hall, we got diverted from my questions about the administration. And I think I asked about your perception of Henry Blumenthal, and I think we touched on that, did we? Okay. After Henry Blumenthal, Gil Panson was acting dean for a year. Any recollections of his deanship?

KOMISARUK: Not really. Just that he was supportive of some experimental work I wanted to do. But I really didn't have any dealings with him directly even then.

COHEN: Do you have any recollections of his important role in establishing the graduate school?

KOMISARUK: No.

COHEN: Have any feeling for the significance of the establishment of the graduate school for graduate study on the campus?

KOMISARUK: I think it was a good thing, but I had no involvement in the issues, the pros and cons. As I said, we were pretty much insulated from it in the institute.

COHEN: The next appointment to dean was Richard Robey. Any perceptions or recollections? It was a troubled administration.

KOMISARUK: Yes.

COHEN: Any recollections about the troubles or what the issues were?

KOMISARUK: No.

COHEN: Conflicts?

KOMISARUK: I didn't have anything to do with him.

COHEN: And then 1976, I think, Norman Samuels took over as dean of the college in 1976 to '82, and then he became provost. Any feel for his deanship?

KOMISARUK: Didn't have anything to do with him as dean. I was a hardworking scientist who—I was making what I thought were very exciting discoveries and surprising. And I was just trying to work on those and get those worked up and written up and, you know, further work done on them.

COHEN: Could you summarize what these discoveries were?

KOMISARUK: Well, I made two main discoveries. One of them was that there's a brain rhythm, a beta rhythm, that is synchronized with behavior on a one-to-one basis, particularly the respiratory rhythm in rats, will synchronize with the beta rhythm, seven per second each. And when the rats started chewing and drinking, they performed the chewing movements and drinking movements at the same rate. So I had this idea that there's a sort of pacemaker in the brain for rhythmical behavior, and that different motor patterns just get—like a transmission, they get hooked into it, and they're all going at the same rate. One or the other gets hooked into this pacemaker. And when any one is manifested, it is driven by the same pacemaker. And I spent a number of years working on that. And while I was working on that, I made another discovery that was—it started conflicting for my time. The other discovery was that vaginal stimulation in rats produced what seemed to be a powerful blockage of pain. And I then had two discoveries that I was trying to work on. And I realized it was impossible to work on both. And I had to make a decision as to which one to work on. And I decided that the pain blockage might lead to something that would be of potential benefit to people. The fact that it identified a substance that is released by the vaginal stimulation that blocks the pain, maybe it's possible to make a therapeutic agent out of it. Whereas the beta rhythm, the rhythmical behavior, would be conceptually, development that would be more conceptual, but I couldn't see any application to it. So I decided to put my energy toward studying the mechanism of pain blockage. And then that's what I have been working on and continuing to work on it.

COHEN: With your work with animals, have you been running into any opposition or any kind of problems with the anti-animal experimentation movement, if that's the right term?

KOMISARUK: Not at all. And even though I work on pain blockage, which implies that I have to produce pain in order to block it, and even though this work has been reported in the media and in the press and on television, we've never gotten any negative contact about it, any criticism. The only thing that we got criticism was from people in Rutgers who were afraid of the impact that our work might have. And they tried to limit the exposure that I've had. And warnings from people at Rutgers. But not from the outside.

COHEN: Has this been a recent development? Any difference in the public response, say, through the, say, seventies compared to the eighties?

KOMISARUK: No, no. No, as I said, the response has always been very positive from the public and the press.

COHEN: Have you seen any... therapeutic connections between your work and, let's say, the development of new medicines?

KOMISARUK: Well, we have—I've been collaborating with Frank Jordan.

COHEN: In chemistry?

KOMISARUK: Chemistry. And we have funds pulled from the university for a patent application for a substance that we've synthesized, based on this mechanism.

COHEN: Oh!

KOMISARUK: And we have a patent application submitted.

COHEN: Chemistry and IAB?

KOMISARUK: Yes.

COHEN: I wanted to ask you just a couple of questions about the Dana Library and to what extent you have utilized it, its collections at all? What can you say about that?

KOMISARUK: Well, even though it's been helpful to me over the years and I've used it—the journals and for current journals and back issues—it's limited; we could always use many more journals. I've had to go to the medical school library frequently for journals. But I've used it extensively over the years.

COHEN: Do you use the interlibrary loan service?

KOMISARUK: Occasionally.

COHEN: Occasionally.

KOMISARUK: Yes.

COHEN: I'm surprised that you say occasionally.

KOMISARUK: Well, for journals, for articles that I can't get at the medical school.

COHEN: You do depend upon the medical school.

KOMISARUK: Yes.

COHEN: More heavily on then Rutgers.

KOMISARUK: Yes. Because if I'm writing a paper, I find it much easier to just go to the medical school and go through the stacks and get the journals that I need; rather than to go to Rutgers and finding that I can only get a third of the articles that I need, and then I have to go to the medical school anyway.

COHEN: That's true of your colleagues, too.

KOMISARUK: I would imagine so. But not everybody uses as many medically oriented journals as I do, see. I don't know about the people who are more involved directly in behavioral studies, you know; with more emphasis on the behavioral studies.

COHEN: You use the computerized Index Medicus from the Dana Library?

KOMISARUK: No. I use—we have a Medline connection.

COHEN: You go to a direct Medline connection. Okay. So you're doing it online. I didn't know. I remember years ago when David Lehrman was there—he was building a library of reprints, articles, reprints. In fact I used to do the microfilming work. Send it off ...put the jackets. What ever came of that collection?

KOMISARUK: We have that collection still there. In fact I use it sometimes.

COHEN: Is it adequate anymore?

KOMISARUK: No, it's not adequate. It was a tremendous job to index it. And that became an overwhelming job for Danny.

COHEN: Yes.

KOMISARUK: Because what he did was, he had to look at each article and decide what categories to file it under. And then we had—that was before computers.

COHEN: Yes.

KOMISARUK: And we had a card, there was a set of cards.

COHEN: Yes, I remember it.

KOMISARUK: The punch—large ones.

COHEN: I remember, yes.

KOMISARUK: I remember about a foot and a half square. [Laughter] And there was a grid of I think 100 by 100 graph paper.

COHEN: Needle it.

KOMISARUK: And with a wheel. And it was—and it holds a journal.

COHEN: Yes.

KOMISARUK: A little drill press.

COHEN: Yes.

KOMISARUK: It'd drill holes through the things, and you'd put a knitting needle through the categories that you want.

COHEN: Holograph cards, I think.

KOMISARUK: I don't know what they call them.

COHEN: I think that's what.

KOMISARUK: It was a very clever idea, and everybody liked the idea. You know you put it through. In fact I bought cards—you could buy cards maybe three by seven with holes punched in permanent, and do that yourself on a smaller basis. And I was doing that for a while, punching across, you'd stick a needle through, and all the ones of a certain category would fall through. That kind of thing. Of course now you do it by just selecting the words: critical filing, index words with the Medline.... So we have that system, and then that became too much to handle, too much for him. He just couldn't handle it; it was a full-time job—more than a full-time job—just reading and categorizing the papers. So we ended up just putting them on microfilm and having microfilm cards. We probably have 7,000 microfilm cards on file; we still have it, and I still use it. But it's arranged by author. So, in other words there's an older article that I'm looking for that I can't find in the files, I go to this old file and I look it up by the author and there it is. It's very comprehensive. But it just stopped. You got it. That's an archives and is useful. But one is a filing system. We never got the filing system up because computers took over. I mean that was before the time of computers. And in 1972—It's really amazing that it was 1972, and the most advanced computer was the Olivetti with a magnetic card. They always had the most advanced developments in computers because Danny was our gadget man. Yes. So he loved to get the newest gadgets. And the newest gadget that he got was the Olivetti computer.

COHEN: Olivetti computer? What was that?

KOMISARUK: The Olivetti computer was this behemoth. It must have weighed about a hundred pounds, 150 pounds. And it was cast aluminum or cast iron or something. And it had a magnetic card. You put a magnetic card into the program for a T-test. But you had to program it yourself. So he was learning how to program and put it through and then it did statistics.

COHEN: Oh, I see.

KOMISARUK: It was a statistical computer

COHEN: Was it a PC?

KOMISARUK: No, then we got a lab computer which was the PDP-12, which took up two rooms.

COHEN: I think I remember that.

KOMISARUK: It was the most advanced computer. It was \$65,000, a laboratory computer to analyze neurophysiological data.

COHEN: What year was that about?

KOMISARUK: That was around 1970—about 1970.

COHEN: Nineteen seventy! So what was happening in the area of computerization as far as the Institute was concerned, by the end of the seventies, the beginning of the eighties?

KOMISARUK: Well, the things that we had in the—the PDP-12 was the state of the art and took up two rooms eight by ten each. In fact we had—it was a double room—to hold the thing. And it was about.... [End of Tape #2]

COHEN: We were talking about computing machines in the IAB.

KOMISARUK: The computers.

COHEN: Something as big as the sofa I'm sitting on.

KOMISARUK: Right. Exactly. Eight feet long. [Laughter] It was a desk with a built-in computer with an IBM, I can't remember what it was called. But it had magnetic tapes, and scared all the secretaries. We could not get a secretary to actually use it [Laughter] because it was so complicated to use. I remember before that the way of typing grant applications on carbon paper or using carbons.

COHEN: Yes.

KOMISARUK: And you make a mistake, and the secretary went crazy and had to erase all the carbons. Because you had to make five or seven carbons. This was before Xerox.

COHEN: Yes. [Laughs]

KOMISARUK: I mean it wasn't that long ago! [Laughter] It was the 1960's—1960's!

COHEN: Well, I remember the first Xerox machine that the Dana Library had before the move to the new campus. I used to clean the drum. [Laughs]

KOMISARUK: It wasn't that long ago.

COHEN: [Laughs] No.

KOMISARUK: I probably still have some copies of articles made on Thermofax paper.

COHEN: Yes, yes.

KOMISARUK: Pink pages and you had a laminate drawer of pink papers.

COHEN: Yes, heat processing.

KOMISARUK: Heat processing. Like tissue paper. It fell apart and crumbled apart. And, yes, grant applications were done on carbon paper because there weren't photocopies. And there was a big question as to whether these.... I remember an issue about doctoral dissertations. When Xerox first came out, there was a debate as to whether it was legal to—whether it was permissible—to submit a photocopy of your thesis because the rules said that it had to be a carbon copy. It wasn't that long ago.

COHEN: I think I remember that.

KOMISARUK: What was it—1960, late 1960. And we had the Marchand calculator that was a mechanical calculator, you know, to do statistics on that definitely weighed about—over a hundred pounds. And, you know, you'd type in the numbers, and it would go chk chk chk chk chk. There was a thing spinning around. And the biggest advance I remember was a tremendous advance: the Selectric typewriter.

COHEN: Oh, yes.

KOMISARUK: That was a great innovation.

COHEN: Yes, yes.

KOMISARUK: And the ball, and typing with it as opposed to the individual keys.

COHEN: The baskets with the typefaces ...

KOMISARUK: Yes, changing the typefaces. So we were always at the forefront, but this was the development of technology.

COHEN: What has been the impact of modern computerization on the whole operation?

KOMISARUK: Well, for many years we were trying—Danny was trying for years and years to have a computer system for observing behavior, for recording behavior. And there was programmer after programmer to get the best. Get the best trying to develop—and finally we had a method. They were all very—now it's very easy. Now there are good programs for analyzing, for using a keyboard to score it, to record behavior durations, different keys for different behaviors. And then you can analyze the duration, the number of times for each event. But for many years it was hopeless, that people's theses got lost in the computer. They put data on the computer, and it was never seen again. It went in, but it never came out. [Laughter] Like the roach motel. Really. It was a serious problem, yes.

COHEN: Is that right?

KOMISARUK: You know they said.... The computer program would say, okay, you know we're going to do this, and we're going to have it working for you. So put your data on it, you know. And students would do their dissertation work and put it in there. There was always some glitch, and they would....

COHEN: Oh, wow!

KOMISARUK: It was terrible.

COHEN: Did this all change with the PC?

KOMISARUK: Yes, it all changed with the PC.

COHEN: It all changed with the PC, yes.

KOMISARUK: Word processing. In fact it wasn't very long ago—it was maybe six to eight years ago—that I remember having a conversation with Winona Cunningham, the IAB secretary, who had been using the Selectric, and I said, "Winona, you really ought to learn how to use a word processor because it would make things much easier." And she refused to do it because she said she's a great typist. She was. She was practically error-free and lightning fast. And she didn't want to learn how to use a word processor, and she was very, very reluctant to learn. And, of course, now she uses it all the time. She doesn't—she hardly ever uses the typewriter except to type up forms or something. That was just eight years ago.

COHEN: Yes.

KOMISARUK: And mostly, years ago, struggling, trying to convince a secretary to use a word processor rather than a typewriter.

COHEN: Maybe the answer to this question is obvious. But what impact has computerization had on productivity?

KOMISARUK: I think it's improved it. I think that a major advantage is the secretary's nerves, they've improved.

COHEN: Improved?

KOMISARUK: Yes. Because now the faculty do their own manuscripts. It's much easier to type up.... I find that it's.... I type up a manuscript, and I can move paragraphs and move sentences very easily and keep changing things right on the screen instead of scrolling things and giving them back to the secretary. And she retypes it, and then I change it, you know, that was driving her crazy.

COHEN: Yes.

KOMISARUK: And driving me crazy. And I would feel guilty doing that because it's not fair to impose this kind of thing on the secretary. And I'd be reluctant to make another change.

COHEN: I know how you feel.

KOMISARUK: But now—

COHEN: Do you compose on the computer?

KOMISARUK: And that's another thing that I had to learn to do because people told me that I should really train myself to compose on the keyboard. And I was very reluctant to do that. You've got to write it on paper and then transcribe it. But after a while, it just became much easier to do it right on the screen.

COHEN: Okay, a couple of windup questions. Is there anything that we've talked about that you'd like to go back to to elaborate on or footnote?

KOMISARUK: Well, the more recent administration. I guess you don't want to get into them.

COHEN: Well, if there's anything let's say you want—anything in the present of course is very much rooted in the past, if you can relate what's happening now to what was happening in the early years.

KOMISARUK: Well, I think—I'm concerned about the future of the institute because I fear that there's an attitude in the field of science today to reify a molecular approach, molecular biology, genetic mechanisms, and control of synthesis of transmitters and control of brain development and things like this. And I feel that the study of behavior is viewed as passé and old-fashioned. But in reality behavior is the output of the brain. And I think that there's a danger that the administration may be getting a message from the ambience, whatever that ambience is, whatever constitutes the ambience, that the age of the study of behavior is past. And therefore the institute has outlived its usefulness. I get the feeling that there's some kind of—that's in the wind. And I'm dismayed by it because I think it's a big mistake. I think that understanding behavior is can guide the studies of the brain. And there are many questions of behavior that still need to be answered. We don't know nothin' basically. We don't know what consciousness is. We don't know the mechanisms of learning, of forgetting, of pleasure, even pain. We don't

understand perception. And to think that it's old-fashioned is big mistake. And the big advantage of the institute is that it focuses on function, and the adaptive function. What good does parental behavior do? Why should an animal put itself out to show parental behavior? Is it altruistic or does it get something out of it? You know to expend effort taking care of the young. Or what drives it to mate? You know we still don't have the foggiest notion of how that works. But nevertheless, there's an attitude that behavior is old-fashioned, the study of behavior's old-fashioned. It's a big mistake, and I think that there's a risk of the institute getting swamped by the neuroscience effort, which I think would be a big mistake.

COHEN: Is there an operating as a kind of molecular reductionism?

KOMISARUK: There's a reification of molecular reductionism. That that's the way to go. And that's, you know, an attitude that gives you more important information than studying behavior, which I think is a big mistake.

COHEN: Yes.

KOMISARUK: But I think the administration might be tempted to feel that way and to short-change the institute. And I think that has to be fought.

COHEN: Is there anything that we haven't touched on at all that you—any questions I should have asked and didn't ask perhaps?

KOMISARUK: I think you've asked just about everything I can think of now. We've touched on all the major things. I guess one thing I wanted to say is my personal development, that why I got into the study of behavior, started out as an engineer student, an engineering.... I remember speaking to my high school advisor, and she told me—she asked me.... This was—I graduated in 1957, the year of Sputnik.

COHEN: Oh!

KOMISARUK: And my science advisor—my college advisor—asked me one question. I never saw her in my entire high school career except for one five-minute interview. And she said, "Do you like math and science?" I said, "Yes." She said, "Well, become an engineer." So I—
[Laughter]

COHEN: [laughter] Like That!

KOMISARUK: Well, yes. Because everybody was—the whole country was being mobilized to beat the Russians. And so I went for engineering, and I decided that I didn't like math. And you have to do math as an engineer. So I decided that I had to change. So I decided that the thing that fascinates me is gravity and magnitude because an invisible force is acting in the distance. And that fascinated me. How does that work?

COHEN: Like an...

KOMISARUK: So I decided to become a physics major. And then after taking a year of physics in college, I realized that the best I was going to get doing an answer to that was a mathematical description of the magnitude of gravitational forces of—magnetic forces. But I would never have an intuitive.... I remember getting an intuitive grasp of what—how does the force act a distance?

COHEN: Well, nobody would know that.

KOMISARUK: No. [Laughter] And I asked some of my friends who were physics majors, and they all confirmed that the best we can do is you're learning the mathematics to describe it. So I said, what else is there? You know then I decided that the next question would be consciousness was a rewarding endeavor. To figure out how brain activity produces consciousness, conscious awareness. So that would be a worthy endeavor, to spend my life investigating this. I decided I would go into some area of—I was going to figure out consciousness. So that's sort of—that's how I got—

COHEN: The rest is history.

KOMISARUK: Yes. Danny was the closest thing to that.

COHEN: Yes.

KOMISARUK: He appreciated those questions. And so a great mind that I just, you know, was magnetically attracted to.

COHEN: ... himself probably. ... [Laughs]...So... Thank you very much.

KOMISARUK: It's my pleasure. I'd hate to have to transcribe this. [End of Tape #3]

[End of Interview]

Edited by Gideon Thompson