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Herbert Barry III, Ph.D.
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Dear Herb:

Enclosed is the transcript of the interview I did on August 16, 1991 with Bob Schuster as part of the APA Division 28 Oral History Project. Bob has edited if for clarity and I have touched up a few minor problems, but what you have is the entire interview. Were it to be published in any way, it would need some further editing.

If you need anything else from me, please let me know. Best wishes in finalizing this project.

Sincerely,

Robert L. Balster, Ph.D.
Professor of Pharmacology
& Toxicology

/wn

Enclosure

cc: Dr. C.R. Schuster

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INTERVIEW WITH DR. CHARLES R. SCHUSTER
INTERVIEWED BY DR. ROBERT BALSTER
AUGUST 16, 1991
NATIONAL INSTITUTE ON DRUG ABUSE

Balster: Let's begin by thanking you for taking time to participate in this oral history project. I want you to say hello to Walter Norman who will be transcribing this and will be hearing a lot more of your voice than he is used to.

Schuster: Fantastic. Hi Walter; I hope that I can be reasonably articulate and as well understandable, because I know what a tough job transcribing is.

Balster: This history, as I understand it, is to focus around the role of organized psychology and specifically the APA Division of Psychopharmacology in the history of behavioral pharmacology. I thought it might be a good idea to begin by having you talk a little about what was going on around the time of the establishment of the Division in 1966 by telling us a little about where you were then, what were you doing, and matters related to that. So where were you in 1966 and what were you up to?

Schuster: Well, in 1966, I was at the University of Michigan in the Department of Pharmacology with a joint appointment in the Department of Psychology. People who were there at that time were John Falk, who I believe still was there, although he may have left about 1966 for the University of Arizona, Bill Stebbins who was in the Department of Otolaryngology, and of course Jim Woods, who at that time was my laboratory technician. Jim had come from the University of Virginia without having finished his dissertation. I will say as an aside that he didn't finish it after he joined my laboratory, so at one time I fired him to try to get him to finish his dissertation. Three months later he still had not done anything, and I thought "Well, it's his problem." He was a great worker in the lab so, I had him come back and start working again. He still had not finished his dissertation, when in about 1967-68 I decided that I was going to go to the University of Chicago. Within a few weeks, he had written his dissertation, submitted it and

shortly after he received his doctorate degree. So, in 1968, I was about to go to the University of Chicago, and I spent a year in transition where I was 2-3 days a week in Chicago, and 2-3 days a week still at the University of Michigan. There were a couple of other people at the University of Michigan at that time who I think were important in the history of Psychopharmacology. Julian Villarreal was a pharmacology graduate student. Since he already had a medical degree, he was on the faculty as an instructor. He did three dissertations (one with Ed Domino, one with me, and finally one with Mo Seevers) before he finally decided that one of them was good enough. Julian really did some exceptional work, but unfortunately, much of it was never published.

Balster: And of course, there was Mo Seevers.

Schuster: Yes, of course. The reason for my going to the University of Michigan was that in 1962, at the Committee on Problems of Drug Dependence meeting, Travis Thompson and I were asked by Nathan B. Eddy to give a talk on this new procedure we had developed, which was drug self-administration. The CPDD meeting that year was held in Ann Arbor, Michigan. When we got to Ann Arbor, I became desperately ill, and Travis had to give the talk that I was supposed to give. Afterwards, I talked to Gerry Deneau, who was a Professor at the University of Michigan, about the fact that they had also developed a drug self-administration procedure, but with more of a pharmacological emphasis in their approach, Mo Seevers commissioned him to see whether not I could be recruited to join them. At that time, I was supposed to go to work at the Institute for Behavioral Research, in the facility that many many years later Dr. Taub got into trouble with housing monkeys there. Travis and I had a grant for $28,500 from NIMH, which was to support both of us, as well as our research. Travis had decided he was going to go back to the University of Minnesota, and I was offered the job at the University of Michigan. I jumped at it with alacrity, because the chairman of that department, Mo Seevers, was probably the leading pharmacologist in the world in the area of substance abuse. A little known fact is that in 1929, he had done some studies on cocaine, which are now known as sensitization procedures. He published on the fact that you can give a dog an injection

of cocaine once a day at a dose that was not convulsant, but after 8 or 10 administrations became convulsant.

Balster: What do you think accounted for the fact that Seevers was one of the earliest pharmacologists that recognized the value of behavioral procedures and the power of behavioral pharmacology to play a role in dependence science?

Schuster: First of all, you would have to understand that Seevers believed that pharmacology was the bridging discipline between biochemistry and medicine. Seevers himself was trained as an anesthesiologist as well as a pharmacologist. He felt that whole organism studies were the province of pharmacology and it was the bridge between the more molecular studies of biochemistry and medical applications in the clinic. He was interested in observable phenomenon in the whole organism. We would talk about that as behavior. He would not have thought of himself as a behaviorist perhaps, but he certainly was. It's also of interest to note that Dr. Seevers, and his colleagues, had begun some of the earliest work on the behavioral and subjective effects of drugs in humans, when, in the 1930s, they would get together in someone's home and take a drug of the evening. Heroin, cocaine, barbiturates, alcohol...

Balster: In combination, I assume...

Schuster: Sometimes in combination, yes, and knowing Dr. Seevers, probably very often in conjunction with gin. But, he told me one story about, I don't remember specifically who it was, who received an injection of drug, and, as Mo said, turned blue and fell down on the floor. "Scared the shit out of us." The cavalier manner in which drugs were given for experimental purposes to humans during that period of time in comparison to the rigorous IRB approval that it necessary now is remarkable. I might add that, to the best of my knowledge, there was no one in that group whoever suffered any serious adverse effect from any of the drug experimentation of that era.

Balster: We know now about all the important behavioral studies in the area of drug abuse that were going at that time with you at Michigan with Steve Goldberg, Jim Woods and others. How many other laboratories at that time were involved in what we would now call behavioral pharmacology research in the area of drug abuse? My perception is that most of the research at that time was focussed an mental illness and psychiatric drugs. Who else was working in the drug abuse area?

Schuster: Well, you are right. I think that the driving force behind behavioral pharmacology, the principle driving force, was the use of behavioral techniques for screening for new drugs that would be useful for the treatment of psychiatric disorders. There were a dozen or so industrial laboratory groups that were involved in such drug screening, including the one that I was involved with (Smith, Kline & French) under the direction of Len Cook. There were relatively few researchers interested in drug abuse at that time. Travis Thompson who had been working with me and at the University of Maryland, went back to the University of Minnesota, and continued with an interest in the area. Then he was joined by Roy Pickens, who is now the director of the ARC and later came Dick Meisch and numerous other people that Travis trained. There was also from the Michigan group Jim Weeks, who ~~although he~~ joined an industrial company UpJohn in Kalamazoo, continued to do research on his own time into drug self-administration. He developed the drug self-administration procedure used with rats. There were other people who had an interest in drugs of abuse including people like Murry Jarvik and others who ~~where~~ studied LSD, and other hallucinogenic drugs, but, it was with a very different emphasis than the group that I was affiliated with.

Balster: You and Travis must have been writing Behavioral Pharmacology in the mid-60s. This book, of course, many people believe to have in a way established the field, and established the methodological approach for the field. Tell us a little about your recollections of that?

Schuster: Well, Travis and I decided that we were going to write the book while we were still working together at the University of Maryland, but we really did not begin to write until he went back to the University of Minnesota and I had gone to the University of Michigan. We did a lot work by mail, but we also very frequently would get together. I would go up to Minneapolis, or he would come down to Ann Arbor. It was one of the most exciting projects that I ever engaged in. What is of interest is that, at the time, I met a guy by the name of Hans Kosterlitz who had a very big reputation in the field of pharmacology, but not in the area of drug abuse. He became interested in opioids and had developed an isolated tissue preparation mainly the ilium from the guinea pig, which you could put into a bath and it would contract when electrically stimulated. Morphine and other opioids would block this electronically induced contraction. He came to my laboratory and saw how we were studying behavior. He said, "My God, this is just like the kind of work I do! You establish a baseline, and you wait until it stabilizes, and then you give drug to observe its effects." This is what he was doing with the electrical stimulations of the guinea pig ilium -- let it stabilize, put in a dose of the drug, record the effects on a kymograph, wash the drug out, and then put in another dose of the drug. He would look at what we were doing in behavioral pharmacology and say, "why aren't European psychologists like you?" He complained, "I've never been able to talk to a psychologist before!" He said it was impossible in Europe to talk to them, but that we were doing science in a way he could relate to.

Balster: It is certainly one of the strengths of your book that it does try to put behavioral pharmacology into a pharmacological perspective which probably didn't exist at that time.

Schuster: That's right. That was certainly in large part because of the fact that I was working in a pharmacology department. It was a privilege to be there, because you see at the time medical school pharmacology courses included a 4-hour laboratory each week. As a faculty member, I was in charge of one of the laboratory sections. We did every classical preparation in pharmacology and I had to learn how to do them, the underlying physiology and how drugs worked in these preparation. Here I was, a

psychologist doing heart/lung preparations, doing stop/flow, kidney preparation, etc.; and so the translation of those kinds of procedures which basically work very similar to what we doing with behavior was so apparent to me, that it very much colored the way that I viewed behavioral pharmacology.

Balster: You and Travis were young guys when you wrote Behavioral Pharmacology. You must have been Assistant Professors at that time. It takes a certain amount of chutzpah to go out and write a textbook. There were obviously more senior people to you that would have been in the field of behavioral pharmacology who might have been appropriate authors for essentially the first book of that type in the area. What led you to think that you and Travis should be the ones to do this?

Schuster: I don't think we ever questioned it. I think that we just felt that this was a new field, it was a field that we both wanted to be in, and were very excited about. I don't think that it ever occurred to us that we were audacious to write a text on Behavioral Pharmacology.

Balster: I guess many of us would date this approach to psychopharmacology to Peter Dews and others in the mid-50s. So this is around 10 years later, thereabouts.

Schuster: Yes, I think that you have to step back and say, that clearly in the early 50s, when I went to work at Smith Kline & French, that was really the initial period of behavioral pharmacology. As you know, it was precipitated by the fact that Smith, Kline & French had been able to buy a drug called chlorpromazine from Rhone Poulanc in France, and Len Cook and some people found that it had a very specific action on blocking shock avoidance behavior in rats, without affecting escape behavior. This was very exciting to them, and they realized that if they could use a behavioral procedure like this for finding a drug for the treatment of schizophrenia, that maybe other behavioral procedures would be of value. So, about that time, E.J. Fellows, the Director of Biological Research at Smith, Kline & French was approached by a psychologist by the name of Don Bullock who was an assistant or associate professor at the University

of Buffalo, but he came from Philadelphia originally. He said, "Why don't you let me set up a behavioral pharmacology laboratory here at Smith, Kline & French?" Well, Don Bullock was a polio victim; he was paralyzed from the waist on down. He had been a student of Keller at Columbia but he really didn't know anything about pharmacology. Well, E.J. Fellows was not about to hire a psychologist. So they gave him a year's grant and gave him a dog room in the back of the laboratory and I was hired to be his assistant. After six months, Don, who was very outspoken, was told on a Friday at 3 o'clock to have his things packed and to be out by 5. I came in the next Monday morning and sat down dejectedly in this room which had about 18 operant boxes in it, and Len Cook came in and said, "Well why aren't the rats running?" And I said, "Well, I thought the program was over." And he said, "No. You are in charge of if now." I had a masters degree at the time and had not thought of myself as being in line for this job. So, we brought in 2 consultants - Charlie Ferster, who was recommended by Peter Dews and, I'll think of his name in a second, he went out to California, but he was a Yale at the time at the Institute ~~for Better~~ Living. Karl Pribram... they were our 2 consultants for the program. Anyway, to make a long story short, I have to tell you this, we ran about 60 rats a day under a CER procedure. It was a 15-minute session, 9 minutes of food-reinforced behavior on a variable interval schedule and 3 minutes of a tone followed by electrical shock, and then 3 minutes afterwards. I had an assistant and we were busy all day long working as fast as we could to put the rats in and out, and dose them with drugs orally. We also had monkeys who were trained using operant techniques. After a couple of years there, management decided the program had gotten so big, and because we had managed to be able to purchase some of the first professionally-developed equipment, they decided to hire to Roger Kelleher to come in to head the program up because they felt the program needed a director with a doctorate degree. It was at that point that Joe Brady, who was the Sigma-Psi lecturer for that year, contacted me and said, "We are going to be setting up a behavioral pharmacology lab at the University of Maryland. Would you come down here and help us set it up?" When I arrived there, and for the first 6 or 8 months, they didn't even have an analytical balance. The only drugs that we could use were those that came in ampules that we could dilute, because we had no way to weigh drugs. So, in a sense, my history was having gotten into behavioral pharmacology really at its inception in the pharmaceutical

Sigma Xi?

industry. Then I joined Joe Brady, who had established one of the very first strictly behavioral pharmacology labs to be funded by the federal government.

Balster: This gives me a chance to ask you about the relationship of this field as it was developing to the organized disciplines to which it relates, namely psychology and pharmacology. What was your perception of the way in which this field as it developed related to what you knew as organized psychology of that day or academic psychology as it existed at the University of Maryland or elsewhere?

Schuster: Well, that is really related to a separate problem, and that is that, at that time, behavioral analysis, which was the underpinnings of the type of behavioral pharmacology that I was doing, was not well accepted by classical psychology. In the Department of Psychology at the University of Maryland, for example, T.G. Andrews, who was the head of the department, was well known as a factor analyst; he was a statistician. McGinnis was there, who was known as a social psychologist. We were viewed, those of us who did behavioral pharmacology, because the basis of that was in behavioral analysis, as being out of the mainstream of psychology. We felt, frankly, a little proud of that; being the renegades. The unbelievable thing about the behavioral pharmacology laboratory at the University of Maryland that Joe Brady and Sherm Ross had founded, was the fact that we had so many of the premier behavioral analysts. Dickie Hernnstein was there, Jack Findley, Stan Pliscoff were there, and all kinds of people came through that laboratory. This range of people came to the University lab because Joe Brady was still at Walter Reed and assigned to the University of Maryland. Some people who were supposed to be in the Army were at the University of Maryland instead because of interpersonal differences that existed between various people over at Walter Reed. So Joe separated them by sending certain people over to the University.

Balster: Could you give a specific example?

Schuster: The funniest thing was when these poor guys who were in the Army had to go over to Walter Reed and be the officer of the day. Half of them had never bought uniforms, so they would run around and scrounge up a shirt here and hat there. I can remember Dickie Hernnstein with a hat that went down so far that his ears would press out, go off to Walter Reed, knowing nothing about what it meant to be the officer of the day. Joe Brady's empire at that time was incredible. To go back to the main point, I think that we were influenced in that stage of our development primarily by behavior analysis. It was subsequently that many of us got hired in departments of pharmacology where the stronger pharmacological influence came in. Also, I think in terms of my own career, and Travis Thompson's too, we began our research believing that we were going to be studying the problems of drug abuse from a behavioral view point. I think we were somewhat disappointed by the fact that when we set up our drug self-administration procedures, we did not have to have organisms who had some type of very specific behavioral history, or the concurrent environmental conditions didn't have to be of a certain nature, in order for drugs to serve as reinforcers. It just popped into place. I mean, you allow them have access to drugs and they took them. And so, we became diverted from looking at behavioral variables somewhat into looking at what kinds of drugs will or will not be self-administered. And so we, as I've said...

Balster: Got trapped into pharmacology?

Schuster: Got trapped into pharmacology, and I think that it is only comparatively recently that we realized that there is a great deal more to drug abuse than simply the primary reinforcing effects of drugs.

Balster: Let's jump forward in time a little bit now to what you recall about the development of psychopharmacology within APA. You say that it was sort of on the outs with psychology. But what do you recall about the thinking that went into this type of an affiliation with the major psychological association?

Schuster: I think that psychologists, at least the ones who were interested in pharmacology, always wanted to continue to have a very strong affiliation with psychology because, even though we might have gotten into departments of pharmacology, we were still viewed as not really being pharmacologists. The true discipline of behavioral pharmacology, where people were trained in both pharmacology, so that they had credentials there, as well as trained in behavior analysis was rare in that era, and so we were still known as psychologists even when we were in departments of pharmacology.

Balster: Just psychologists.

Schuster: That's right. Yes. And I have to say that this may be an apocryphal story, but we were not helped by the fact that even great psychologists like John Harvey, who was at the University of Chicago, and who was teaching medical school pharmacology at that time, referred to the pancreases in the plural form, because he assumed everything was bilateral, people, you know, had 2 kidneys, 2 lungs, etc. And being an expert in pharmacology above the neck, when he got into the more general fields, he revealed our abject ignorance, ...so it didn't help. I think we found a need to continue our strong relationship and affiliation with psychology; therefore, a large group of people began to be interested in establishing a Division of Psychopharmacology in APA. I think the first president was Murry Jarvik and the early leaders were people like Vic Laties, John Boren, and Larry Stein, etc. All of these people who became presidents of the Division were very active in the initial phases of the establishment of the Division. I must say that, although I was involved in the APA, really the main organization for me was the Behavioral Pharmacology Society, which had been formed largely by a group of industrial behavioral pharmacologists. As I remember it, Len Cook and I attended probably one of the first meetings, which was in a hotel room in Chicago when a bunch of scared psychologists got together and said, "Hey what are you guys doing?" because they had all been hired by pharmaceutical companies. And they said, "Listen. You got anything that works? We've got to share it guys, because, you know, the companies, they expect us to produce!" And Len Cook, who was trained in pharmacology and had many years of experience in industrial pharmacology, said "Are you

nuts? This is a competitive business! He was flabbergasted by the naivete of the early people in the area. I won't reveal the names of the people who were there, but I think reflection would tell you that they are among the early leaders in both the Behavioral Pharmacology Society and development of Division 28.

Balster: It does seems like a lot of the same players were in BPS and Division 28. The ones who were in Division 28 must have honestly been more connected with the American Psychological Association to be able to develop the sufficient resources to get a division established, etc.

Schuster: Right. Well, I think that the American Psychological Association recognized early on, I mean experimental psychologists and others, that drugs were useful not only because of the potential for becoming therapeutic medications, but also because they were useful for teasing apart some of the brain mechanisms underlying motivated behaviors and so forth. Neal Miller, and the people that he ~~would have~~ trained, used drugs as tools for understanding feeding and drinking and other behaviors. I think that there was a great deal of support for formation of the division, not just because of what I would call more strictly behavioral pharmacology, but the more broad base support, because of use of drugs as tools for understanding the central nervous system.

Balster: Did you regularly go to American Psychological Association meetings? Was APA a place where you and the other behavioral pharmacologists presented important research?

Schuster: Yes, I think that all of us regularly attended the APA meetings as well as some of the regional meetings. One of the principal ones being, of course, the Eastern Psychological Association which almost rivaled the APA for those of us who were interested in behavioral pharmacology. But at both of those meetings, we went, I think, not only because of the good science but also because it was always a very good social experience, where people who were interested in this fairly narrow discipline could get together. The meetings were excellent. I remember one meeting, and I think that it was in Cincinnati, when I was

going to give a talk and it actually was a satellite session of the American Psychological Association. I was going to give a talk on a topic that was not behavioral pharmacology but purely behavioral analysis. I was preceding a talk by B.F. Skinner. The talk I gave was on behavioral contrast, which was one of the earliest studies of that phenomena in pigeons. Jack Finley, who was also giving a talk was rooming with me. We started to the meeting but stopped for breakfast first, and he said, "Bob, I know you're very nervous because this is the first talk you've ever given." By this time, we had walked into restaurant, which was a very big and crowed ~~restaurant~~. And he said that I may even feel like getting nauseous and throwing up! He then proceeded to have dry heaves in this restaurant. I was a nervous wreck! But he was the one with the butterflies in his stomach!

Balster: How did psychopharmacologists find each other at APA since it has always been that the convention was large. Now of course, we have the famous Division 28 hospitality suite which serves to bring people together. How did you people find each other?

Schuster: There were sessions where psychopharmacology or behavioral pharmacology was discussed, and I think that was largely where people would get together and see one another and make their social arrangements for later. Parties were in rooms, although they were not formal hospitality suites then...

Balster: Was it a responsibility of the Division president in the 60s to have suites?

Schuster: I can remember going to rooms that were very large, whether they were suites or not, I'm not quite sure. We would crowd into them and usually this would be around the leadership of Division 28, whether it was the president or one of the other big guys in the organization, I don't know. I found some of the early divisional activities almost a little intimidating, because you know, people were there like Vic Laties and Bernie Weiss, etc. Although, Travis and I were writing a book and doing things of that sort -- these guys were clearly senior to us. I, for one, was a little bit intimidated by them.

Balster: I'm sure when you initially began going to APA, your affiliation was with Division 25, and the materials that you would have submitted were submitted to Division 25. Did behavioral pharmacology research appear in symposia and sessions with more basic behavioral things? Do you see these as better integrated then than now?

Schuster: Actually, as I remember it, they were always fairly separate in a sense that symposia would be organized around behavioral effects of drugs, although people would give reports on new methods that might be useful for looking at drugs. But I think that they were fairly distinct. But the questions that were of interest at those kinds of meetings were behavioral questions. I remember one time when I was reporting on the effects of amphetamines on DRL performance, and somebody asked me about how I had constructed my IRT distributions, whether or not I had excluded the rapid responding that would be in the first bin. Actually, Joe Zimmerman had done the analysis. Although I was giving the paper, I blanked for a moment in terms of how it had been done, and it was probably Vic Laties or Bernie Weiss that was asking me this. I was really mortified, because at that time, I was still a graduate student even though I was fairly well-known. My history was somewhat strange, because I'd been in industrial pharmacology -- when I had a masters degree, and had gained a lot of experience and a lot of contacts with the big names in psychopharm, because of that job. It was after that I went back to graduate school. I was actually paid as an instructor. Because I had all of the industrial experience, I could only take a limited number of courses per semester, so it took me a year longer to get my doctorate. So I was an accepted member of the group, even though I had not finished my graduate school career.

Balster: Moving now a little bit ahead. I was looking over the early list of presidents and I guess they would have been viewed as senior scientists in the way you said earlier, and then along comes Travis Thompson, in what was that, 1975?

Schuster: '74-'75.

Balster: '74-'75. He was elected president. In fact, I noticed that he was elected president before Joe Brady, who of course was his advisor. Travis would have been a fairly junior person to be president in 1975.

Schuster: Well, Travis had always been very much involved in the Divisional activities in one way or another; these may not have been necessarily in elected positions, but he had been very active in the organization. I remember that he would always attend all of the business meetings of Division 28. I sometimes got there and sometimes didn't. But he had a strong interest in it. And I think it's fair to say that Travis, because he was in department's of psychiatry and psychology, whereas ~~in my own career~~ I was in a pharmacology department at that period of time, felt a much stronger affiliation with the American Psychological Association than I did.

Balster: Were you involved in the Division formally before you became president? What where the circumstances that surrounded your running for office and what can you remember about that?

Schuster: Well, after Travis became the president, he got me involved in this [illegible] - oh, I helped to organized the convention one year.

Balster: Oh did you? Were you the program chair for a year? Do you recall what convention that was?

Schuster: I honestly do not, but remember that we fought for hours. We fought for space. We fought for the great meeting times, the same problems as now.

I also was involved in the whole issue of selection of people for fellowship status within the APA, which became a very big deal, even back then. I suspect that Don Overton has been ~~super~~ involved in

that issue as far back as I can remember. He developed these elaborate, semi-objective grading scales to help people to get fellowship status within APA. I also served on council. I was the council representative for Division 28.

Balster: Oh you did? You mean on the APA Council of Representatives?

Schuster: Correct.

Balster: Prior to your service as president?

Schuster: I believe so, and I have to confess to you that it was a bizarre experience for me, because, I went to these meetings, which were very very large, in Washington, D.C., and did not understand 99 percent of the issues that they discussed. At that time there were long and bitter discussions on the use of selection procedures for employment, whether or not there were racial biases, etc., etc., and issues that I did not understand. I sat there just sort of perplexed most of the time. I was very happy when I got off that assignment, because it was just not relevant to me. I did not have anything to contribute, and never in the year that I served felt that the issues were very critical to Division 28. Although, that's not completely true. Even back then, there were issues that related to experimental psychology vs. the professional applied practice. There was a strong cleavage between these groups even back then. For my voting, since I didn't understand the issues, I would simply see where the experimental psychologists were voting and follow their lead. Today, I would have taken the assignment more seriously.

Balster: Many of the people that were involved with the Division, including of course yourself, had worked in the pharmaceutical industry and were very familiar with psychiatric medications and their use in treating mental illness. What was your vision at that time about whether or not practice-based applied psychologists

had a role in using medications? Was there any sense of trying to bring the information that you knew about medications to psychologists in the service delivery area? Was that ever a goal of the Division?

Schuster: Well, I don't know that it was an important issue at that time. I think that the principal emphasis was on how to discover new medications using laboratory procedures. The only place where this came into an applied area was with people like Og Lindsley. What he did was the beginnings of human behavioral pharmacology, in which he had schizophrenics under experimental conditions where they were engaging in operant responding, reinforced with things like seeing a kitten getting milk. He would then investigate how drugs affected their responding. It was not clinical in a sense of looking for how that related to improving their disordered thoughts.

Balster: It seems to me to be an important historical issue, of how it was that the use of medications didn't become very much integrated into applied psychology. What led to the thinking at that time that medications didn't play a role? I don't quite follow what would have been done differently that might have integrated this material better into applied psychological service delivery?

Schuster: Well, now that I think about it, one of the people who thought about these issues earlier was Travis Thompson. But Travis thought of drugs the way that I believe they should be thought of, and that is enabling individuals to be able to be brought under the control of behavioral contingencies. He wisely moved into an area that was so neglected by physicians and everyone else, namely the treatment of the severely retarded, where he was able to study the interaction of medications and contingencies of reinforcement. He was able to do things that he would have not have been able to do if he had gotten into an area where there was more competition from physicians and traditional psychologists.

Balster: Early behavioral pharmacologists had very good connections with the leadership in psychiatry. Persons involved in the development of psychopharmacology such as Jonathan Cole, Bernard Brodie, and

Dan Freedman and these folks. I mean you had a much better rapport and probably had better entre into their leadership circles than you did into the leadership circles of psychology. Would you say that's true?

Schuster: That's true. Yes. Skipping ahead, when I went the University of Chicago, I remember that I went there primarily because I wanted to become involved in clinical matters, and went jointly as an Associate Professor in the Department of Psychiatry, and as the Associate Administrator for the State of Illinois Drug Abuse Program, which was a comprehensive treatment program. We established a therapeutic community called Gateway and a Crisis Intervention Center for adolescent runaway kids who were using drugs, which was called Flash Tire, and numerous places of that sort. I ran experimental methadone maintenance clinics. I began to get involved in the use of drugs for the treatment of addictive disorders. A little known fact is that Jerry Jaffe and I, back in the late 60s maybe early 70s, had a patient who was using cocaine who was also on methadone. She was very highly connected in the drug distribution network, and so she got really good cocaine. There was a long-acting phenothiazine preparation. We gave it to her. She came back very angry. She said that she couldn't get the usual effect from cocaine. So we actually looked at medications for the treatment of cocaine addiction 20 years ago. Cocaine use simply was not a big enough issue with most patients that we pursued it much further, but we worked on problems of LAAM and so forth. But this was in relationship to very specific behavioral disorders, namely individual's heroin addiction and the propensity to self-administer heroin.

Apart from drug abuse, in the other areas of general psychiatry, I think that there was less of an interest in Division 28 in those areas than there was in the animal methodologies utilized for finding new drugs.

Balster: As I said, despite the fact that the research focus of the division members was primarily laboratory based, many of you were very well known to and connected with the people who were very instrumental in establishing the use of medications in psychiatry etc. You definitely didn't have that same name recognition, or did not have contact with, organized psychology as you did with medicine and psychiatry.

Schuster: That's correct. Very early on, certain of us started attending meetings of the American College of Neuropsychopharmacology which was initially started by a group primarily composed of clinicians.

Balster: But ACNP also did not in general have too much involvement in applied psychology.

Schuster: No we didn't. Well, clinical psychology had people who worked as hand-maidens to psychiatrists; that is, people who had developed mood scales and diagnostic instruments. Those types of applied psychologists were members of the ACNP, or people who were statisticians, since psychologists often were quite well trained as statisticians.

Balster: What about the early service-delivery psychologists in the area of substance abuse treatments? Again, were they connected to the scientific research in the Division's work? Who would you name right now as people who were active in the mid-60s in substance abuse treatment?

Schuster: I think that there has always been and continues to be a schism between the applied psychologists and the experimental psychologists in the areas of substance abuse disorders. It is one of the major disappointments to me how little impact we've had. Now, that isn't to say we haven't had some. Don't misunderstand that. But, I think that there are relatively few people who go back and forth between these two areas. A lot of the efforts which we have now at the National Institutes of Drug Abuse are related to how we can better get linkage between treatment practitioners and the experimental basis of treatments developed by behavioral pharmacologists.

Balster: I can't but help to wonder what might have been done differently to impact on service delivery then when the division was forming within the American Psychological Association. The APA, of course,

does have a way of moving information around different levels. I'm just trying to think through how it could have come out a little differently if Division members had been more interested in applications?

Schuster: I think the problem really is that the conceptual foundations of the two groups were so disperate, and communication between them has always been difficult. Getting a radical behaviorist who is doing studies of variables that we manipulate in the laboratory into a room with an individual who is psychodynamically-oriented is not usually productive. At that time, in the earlier 60s, when individuals were very heavily psychoanalytically-oriented meant that there would virtually be no communication between these two. That's been the problem. Now, what has happened is that a lot of the substance abuse practitioners have adopted some of the terms which we use; for example it is not at all uncommon for them to talk about drugs as reinforcers. But, they really don't understand the concept of reinforcement; they equate this with euphoria. Although they use the terms, I'm not sure that using common terms is not further obscuring the fact that we are still not communicating. And that I think is very sad. The other thing that has been difficult for the practitioner is that they see us as manipulating single variables and looking at a specific instance or type of behavior or class of behavior. And they are saying, "God, we've got to change this person's lifestyle." We have failed, although Jack Finley I think was on the road to doing this, to build larger units of behavior which are still under the control of contingencies. Those larger units of behavior may be more relevant to the treatment practitioner, than a lever pressing response and how that is affected by a drug or by an environmental manipulation. We have a long way to go in behavioral analysis.

Balster: It should be interesting at the APA convention this Fall, where Travis Thompson has organized a symposium on the scientific basis of drug abuse treatment, which is intended primarily for practitioners. It will be interesting to see how that goes.

Schuster: I will be interested in attending it. My experience with those who are totally devoted to practice has been that, when our research demonstrates unequivocally a given phenomenon that they believe from

their clinical experience is true, they are elated by this. If, on the other hand, our research results in some ways vary with their particularly unique experience in treatment, it is either dismissed as being irrelevant or simply wrong. There is nothing that is more compelling to most people, myself included, even though I am an experimentalist, as one's own experience, as opposed to reading an article in a journal. ~~And therefore, even though~~ our own experience does not allow us to extract the critical variables that are responsible for the phenomenon we are observing. Nonetheless, if we think that we know the critical variables, and we have this experience, boy, I mean you play hell in trying to change that individual's attitude.

Balster: Although we want to focus on history, I think it will interesting in this context to address this issue from your position as Director of the National Institute on Drug Abuse. Your phone must ring from various professional groups trying to ply their wares in the service of this public health problem. Where do you see service-delivery psychology? I mean, how often are they ringing your bell, and where does applied psychology have a role in technology transfer?

Schuster: Well, one of the reasons why I find job at the National Institute on Drug Abuse so challenging is because I think that science, simply for the satisfaction of curiosity is a luxury which our society cannot afford. Therefore, it is essential I think, not that everything we do have immediate relevance or direct applicability, but that we are sensitive to the fact that data may have some implications for the applied practicing psychologist, who is attempting to deal with individuals who are substance abusers. What we're trying to do to improve this situation falls into two categories. First of all, wherever there are meetings involving practicing physicians, psychologists, or treatment prevention practitioners, we are sponsoring a research section at those meetings. We want to try and bring to them the very latest in the science that we think is relevant to them as practitioners. Secondly, we are attempting wherever possible to get our researchers involved and sophisticated about what the clinical problems are that exist, not necessarily so they can change the course of their research, but simply to be attuned to what portions of their research

may have relevance to these clinical problems. I think that we are being more and more successful. I have to say that people like yourself are very much the model that the training of basic researchers in the behavioral pharmacology substance abuse should have in mind. You have a strong background in behavior, you have a strong background in pharmacology, and an acute awareness of the problems of the practicing therapist, who has to deal with the problems of drug abuse in the real world. I think it would be unrealistic to think that we are going to have a major impact in every area. And that's true for all of medicine incidently... (Interruption)

Balster: Bob, when we were cut off there, we were talking a little about the lack of continuity between science and practice in substance abuse treatment in areas in applied psychology. You were saying that this problem exists in medicine in general. Do you think that's true?

Schuster: Yes, I do. Let me tell you that back when I was a graduate student, I worked with a local surgeon, who was also a very good jazz piano player by the name of Bill Marcus. He was preparing for his surgical boards. He would come over one afternoon a week to the University of Maryland, where we had the psychopharmacology lab to help me with ~~only~~ catherization procedures for drug self-administration, and also did Therry-Vella loops and all kinds of things. He started to study the experimental surgery literature from Markowitz's book, and discovered that human surgeons had been continuing to do surgery for 10, 15, sometimes 20 years after experimental surgery showed that the procedure being used was not the best approach. When he took his boards, he almost flunked them, because he told the examiners that they didn't know where it was at! Needless to say this didn't go over very well with authoritarian surgeons! But I think that we have a better shot at solving this problem because of the dedication of people like yourself and others who I think are really devoted to ~~try and have~~ science have an impact on treatment practitioners.

Balster: Clinical psychology is paying increased attention to psychopharmacology, and some feel that psychologists should get involved much more in decisions about medications, possibly even achieving prescribing privileges. This of course is going to force them to revisit the science in the area. What is your vision on how that's going to impact?

Schuster: Well, I think first of all that practicing psychologists must be involved in the issues of prescribing, not necessarily that they have to do the prescribing, but they have to be working with the physician who is doing the prescribing. They have to understand the actions of these drugs, they have to understand the toxicity of these drugs, because they may be in a position to pick that up much more readily than the physician who is not seeing the client as frequently. I think the other issue is that if they are not aware of the fact that huge numbers of their clients are using illicit substances as well as alcohol, they can't be providing appropriate therapy. In my opinion, they should be knowledgeable about medications and drug abuse. So it is essential that clinical programs develop the appropriate coursework for clinical psychologists to be able to understand the actions of drugs. Now, if they are going to get involved in prescribing, I think this also represents a unique opportunity for us to really educate people about the behavioral and other actions of drugs that are important therapeutically for the treatment of psychiatric disorders. I look at this as a challenge to develop curricula.

Balster: I share that vision. I think that it would be really a unique opportunity to create a new kind of psychopharmacologist, one that's equally smart about the interrelationships of behavior and drugs as well as the drugs themselves. If we just make another group of physicians, we really won't have done anything, but to take the unique knowledge that psychologists have, and apply that to better informed treatment, that would be great.

(phone interruption) Bob, at this point, we should return to our history, and have you tell me a little about what you recall about your term as President of Division 28. Do you remember who you ran against or anything related to the elections?

Schuster: To be honest with you, I don't? That may sound a little strange, but I do not remember. I guess I could find it someplace, but I would tell you this. At the time that I was elected, there were beginning to be great discussions of whether or not we were representing many of the members of Division 28, who were, in name members of the division, but who were the very people we have been speaking about, that is, practicing psychologists. Also, those psychologists who were primarily involved in clinical evaluations for the possible therapeutic efficacy of medications, who were therefore working with psychiatrists, and clinical pharmacologists to look at these new medications. I think that there was a perception that Division 28 was not really responsive to their needs and their interest areas. So there was a lot of discussion during the year that I was President about whether or not we shouldn't be broader, and try to incorporate more programs of that nature into presentations that were sponsored by Division 28. I think that continues to be a problem today. It certainly was an issue during the year that I was President.

Balster: You said that you were President for a year, but in actuality, you were President for 2 years. In fact, I notice that you are the last person to serve a 2-year term. I wonder if you attach any significance to that?

Schuster: Maybe that's because in the second year, I forgot I was the president. I don't know.

Balster: How did the Division run then? Did you have 2 meetings a year, as we currently do? What do you remember about the meetings?

Schuster: Well, I think that it was probably a little more slip-shod in those years, or at least -- I shouldn't say that for other Presidents, but at least during the 2 years that I was the President, we met only in association with the major APA meeting. The rest of the time we handled business by mail and so forth. As I say, I do not think that there were any unique problems during the 2-year period that I was the President, always the problems of scheduling of our symposia where they overlapped with Division 25, because many of our influential members, the more active members of 28, were also interested in going to Division 25 sessions, and so forth.

Balster: Who did you perceive as the active members around the time of your Presidency? Who were the ones who were attending these meetings, worked for the Division and involved themselves with issues?

Schuster: Well clearly, Don Overton, Herb Barry, Vic Laties, Bernie Weiss, Travis Thompson -- along with a younger group of people who were coming along, including many of the graduate students who had recently received their doctorate degrees from these people. Obviously, in my own case, Marian Fischman, Chris-Ellyn Johanson, and people of that type. Many of those people continue to have a very strong interest in Division 28. Joe Brady has always had a love/hate relationship with the American Psychological Association, and as you know, he engaged in a bra-burning shortly after his Presidency of Division 28 because he didn't want to belong to any organization that had "PSYCH" in its name. Joe remains one of the people that I think has maintained the strongest integrity as a behavior analyst. There's lots of good behavior analysts, but many of those who are strict behavior analysts have not existed in the kind of environment that he has, which is a psychiatry department. To maintain as steadfastly the purity of language, and the clarity of his thinking, is a real role model for all of us. I think he always felt uncomfortable a little bit in the American Psychological Association, because too of many us began to slip a little bit in our verbal behavior, and even started to talk about things like subjective effects as if there were something real there, which he would prefer to deal with simply as instances of verbal behavior.

Balster: You know, Joe followed you in office. That's sort of an interesting situation I suppose. A little role reversal there in passing the baton to your advisor. Any delicacies there?

Schuster: Well no, actually, Joe has always been one who has pushed others ahead of himself for their good, and for the good of the fields that he has been affiliated with. I think that if he had a choice, he would have done it that way.

Balster: I know that Joe was president of other divisions prior to being the President of the Psychopharmacology Division. In fact, that is one of my first memories of being associated with the Division's executive committee. Joe at that time was President and we were going through our annual concern about getting enough apportionment votes to get a council representative, and we were taking about the vote 10 campaign. Joe Brady, in his characteristic voice says, "Well I always give 10 votes to the Division that I am the president of."

Schuster: I like that.

Balster: What was your perception, at that time, of the relationship of the Division to the central office and the leadership of APA? Did you feel that you were being paid attention to by the leadership? And did you have contacts with them on a regular basis? What was your perception then?

Schuster: I think that we felt that we were not in the mainstream, that they did not really have much of an interest in us. The "old-established" divisions, I mean 3 and those that represented what I would say are the more traditional topics in psychology departments, we felt, were given the major attention by the central office. On the other hand, the people who handled the convention, I had the feeling that in the two years that I was president, that they really tried be accommodating. But as far as central office was concerned, I think that really we didn't pay that much attention to them.

Balster: As I recall, a sort of watershed of our estrangement with the leadership of the American Psychological Association came, I believe, when John Falk was the president and the APA filed an amicus curae without consulting anybody, claiming that there was not evidence that medications were effective in the treatment of mental illness. Although I recall at one time they said they had consulted you.

Schuster: That's right, they said that. That was not true. There may very well have been a telephone call to me, but if there was, I never responded in an affirmative fashion to what they were saying. We very, very vociferously disagreed with the conclusions of the American Psychological Association in regard to the amicus brief.

The data establishing the effectiveness of anti-psychotic medications for the treatment of schizophrenia is very solid. More importantly, medications make it possible for the schizophrenia to come under the control of reinforcement contingencies for decreasing unwanted behaviors and strengthening desirable behaviors. The medication makes the psychiatric patient more amenable to the other kinds of therapy, which are essential. But to pit one vs. the other is just silly.

Balster: Moving a little bit forward to where we are now, one of the perceptions I have is that although the Division in some respects began with a strong emphasis on psychiatric medications and mental illness, now there seems to be a very substantial proportion of the Division's interests in the areas of substance abuse. In fact, at a recent APA Convention, 14 of 21 symposia were devoted exclusively to substance abuse issues. Do you have any thoughts about why substance abuse research has come to dominate the Division's interests?

Schuster: Well, I suspect for one thing that the fact that we have excellent animal models in the areas of substance abuse, which allow us to study many interesting things. We do not have nearly as well developed animal models for the other psychiatric disorders such as schizophrenia, affective disorders and so forth.

I think that's one issue. I think that number two, it's simply also a reflection of society's interest in this problem. And I think that all of us like to work on problems that are timely. And drug abuse clearly has been a timely topic. I also have to say that I think that the major increased funding that NIDA has provided, to the field in general and psychology in particular, has also been of great importance. We calculate that almost 25 percent of the approximately 1200 grants and contracts that NIDA has have a psychologist as principal investigator. That's a significant number of grants and contracts, between 250 and 300. That's going to give rise to a lot of data, and a lot of people who are going to want to report those data at the psychological association meetings.

Balster: One of the things that I think was an important activity for the Division sometime ago was that it was our perception that organized psychology and APA seemed to focus a lot of its attention on activities of the NIMH.

Schuster: That's right.

Balster: And a group of us had gotten data similar to what you have just described and met with some of the APA leadership and said, "Look, there is a lot of research being done by psychologists being funded by NIDA." I think that now there has been a substantial shift and appreciation for the role of psychology in the other ADAMHA Institutes.

Schuster: Well, at the APA meeting this year, I am giving a talk on the ways in which psychologists can be involved with the general area of substance abuse disorders. One of the things that I have to say is that we have clinical psychology training money, for training clinical researchers, and in the first two years of this program, we have not received one application from a clinical psychology program. I keep reporting this and I hope that will change. It hasn't so far. I think they will wake up to the fact that this is of great value to them for clinical psychologists to have a formal training program in substance abuse.

Balster: Let's close this discussion of the Psychopharmacology Division with just a little statement from you on your perception of what it's done, and what it's doing, and perhaps even what it should do? What's your view about how the Division has played a role in the field?

Schuster: First of all, I can tell you something very personal, and that is that, there is no question of the fact that, as the first psychologist to ever direct an ADAMHA institute, and with our possible move to NIH, and the only non-M.D. as a director of an NIH Institute, there's no question of the fact that the American Psychological Association is one of the organizations that lobbied very effectively for my getting this position. Clearly, it was Division 28 that was responsible for getting APA to do that. I think that the Division is also important as a force for maintaining Congressional support for the National Institute on Drug Abuse, NIAAA, and NIMH, as well as the NIH Institutes that also have behavioral research. One of the things that is appalling to me, and I just read this, is that in 1968, in American universities, there were 110,000 university-based scientists. Today, there are 225,000. In 1968 until today, there's only been an increase in 20 percent in dollars for the funding of basic research, even though there has been a doubling in the number of people who are involved in research. I think that Division 28 has done a great deal to insure that there is adequate funding for research that involves psychology. Obviously, we need to continue that into the future. I think we have to be able go the legislatures and to the people who control the dollars that are spent on research in this country and continue to show them the relevance of behavioral pharmacology research to the problems that are germane to society-drug and alcohol abuse, smoking, psychiatric disorders, etc..

Balster: You're calling really for a very strong advocacy role for the Division and for APA. That's probably a different vision from when the Division was created. Was that on the minds of people of that time?

Schuster: I think there was a different type of advocacy when it was created. I think the goal then was to legitimize the field. They wanted to say to other academicians, "Psychopharmacology is a discipline, and it has arrived!"

Balster: Do you think that they succeeded?

Schuster: Oh, I think so. No question about it. I think that a lot of the advances of neuroscience today, although you can talk about them as being breakthroughs in terms of molecular biology, are predicated on many of the insights ~~which~~ that psychopharmacologists have provided and perhaps the most important thing, our type of whole organism research will re-emerge. I can't say how quickly. The fact is that molecular biology is of interest only when it can be related to the integrated organism and the functioning of the integrated nervous system, which is responsible for interacting with the environment to produce behavior. And I think that psychopharmacology has developed the kinds of procedures to ~~really~~ get at interesting problems. How do molecular biological events interact with themselves and environmental influences? That's what we have studied, and that's what we know about.

Balster: You said earlier that in addition we have to reach up into the clinical areas of practice. Is it not the case that behavioral pharmacology should also reach into the neurosciences? We sit in the middle of these two important areas and can ~~really~~ bring them together.

Schuster: We have to be broad-based. I think for example, that we have to look at the contributions of behavioral genetics to our area, how sensitivity to drugs differ, and how genetic differences may be responsible for increased vulnerability to problems of substance abuse. Again, always with the caveat that we are sophisticated. We know that no variables effect behavior in an inexorable way; one must consider the interaction of genetic factors with the environmental determinants of behavior.

Balster: What you are saying has had such as big influence in the field. This approach, as articulated in the Thompson and Schuster book, dominated the way this field has developed. You know, I think we will be a little remiss if we don't take this opportunity to reflect a little on our shared personal history. It was in the late 60s that I first met you. I'm not sure if I have told you my story of this meeting. As you recall, I was at that time a graduate student and doing my research at what was then called the Houston State Psychiatric Institute, with a relatively little known, but I think remarkable, man named Robert Harris - Bob Harris, who you know of course. Bob had this notion that we should be doing intravenous drug self-administration research in monkeys. This was just four years after your paper came out with Travis Thompson, and was at the time that the paper came out by Deneau, Yanagita and Seevers. He wanted us to get into this field. So the only way to do this, it seemed to Bob, was to get this Schuster to come down here and show us how to do it. Now having worked in this field for 20 years, I realize how crazy this was that Dr. Schuster was going to come down from Chicago, and in one day show us all we needed to know to set up and run an intravenous self-administration lab for rhesus monkeys. I recall very well, you coming in looking every bit like a Chicago gangster, as I recall a black suit, a black shirt and a white tie. You came rolling in for about 6 hours, you did a surgery, showed us how fit animals to harnesses, and told us everything you could tell us about self-administration. It was remarkable that we were able to get anything going in the area from this short visit.

Schuster: Well, let me put it this way, I remember that day very very well and remember Bob Harris with a great deal of fondness and respect. He was one of the better people that I have ever interacted with. He spearheaded in an area that I think should be redeveloped, and that is looking at the conditioned reinforcing effects of drugs. I think that we have exhausted the demonstration of the primary reinforcing effects of drugs, in the sense that it is now a well-established notion. I think even with drugs like cocaine, we have not really given the necessary attention to the fact that cocaine use sets the occasion for a variety of other behaviors to occur, which are highly reinforced. Cocaine use often occasions partying, sexual behaviors, and so forth. Bob Harris was really way ahead of his time in thinking about those kinds of issues.

He also, of course, was a marvelous human being, and I spent a lot of time with him on a personal basis. His passing away as a consequence of his alcoholism disorder was a great tragedy. I think it is important just to remember that he never drank up until he was in his 40s. But I also know that both his brother and father were alcoholics, and once he began to drink, he very rapidly succumbed to the problem of alcoholism.

Balster: Bob, that was the very beginning for me of a long association with you from which I've benefitted enormously, intellectually, professionally, in every other way. I want to say to you that the way you have conceptualized research problems, and hearing you articulate them again today, is what has shaped my own personal view of the field and the way research should be done.

Schuster: Well let me just say that I have been very lucky in having a lot of good students and post-doctoral fellows, and it's people like yourself who are the basis of my continuing to have the kind of enthusiasm and interest I have in the field. I've got to say that you guys have taught me a lot. I've learned a lot through my interactions with post-doctoral fellows, particularly the couple of years which we spent together in Chicago. And secondly, it's formed the basis for a very long term relationship which you and I have had, professionally and personally, it's been very beautiful.

Balster: I've really appreciated you taking this time away from the telephones at work to provide these reflections. I also know that this is a particularly difficult time for you. Your mother has just recently passed away and I know this has been a very sad time for you. This might be a nice chance for you to tell us little about the role of your family in your career, and how it's impacted on your life.

Schuster: Well, I think one of the things that I was very fortunate in was that my parents and my older sister were very encouraging about intellectual kinds of activities. As my mother reminded some of her friends a few months ago, my parents and my older sister always taught me to question dogma. I haven't

stopped. They also gave me the self confidence to take a chance on being wrong -- that's important if you are going to try to do innovative science.

Balster: Bob, I know one of the people that has been really important in your scientific career and in your life has been Chris. And of course, I was there when you met.

Schuster: Yes, and you disapproved of our relationship.

Balster: Yes, I did. At that time...

Schuster: Yes, right. I can only say that I think I'm probably the luckiest guy in the world. I not only have a lovely wife, but a really stimulating intellectual scientific partner in everything that I do. Although we don't any longer work at the same institution, we come home at night and we talk about what she is doing in her research, and what I'm involved in at NIDA, and I can only say that I can't tell you where my work stops and my personal life begins because they are all one. It gives a richness to my life that I feel indeed blessed to have. And I also think Chris-Ellyn has been very important to me during this period of time that I have been director of NIDA. There have been incredible kinds of political pressures and other kinds of things that could get you off the straight and narrow path, in a sense of sometimes it's just easier to let things go through than to fight. She has always given me the support and the backing and the integrity to continue to try to do things that are really in the best interest of science and to not be persuaded by the short-term expediency of political pressures.

Balster: I think those of us who know you and know about how you do your work realize what a terrific team you and Chris are.

Schuster: Thanks a lot.