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NANCY CAMPBELL

SUBJECT: ADDICTION RESEARCH

INTERVIEWER: NANCY CAMPBELL

INTERVIEWEE: JERRY JAFFE

SOURCE FILES: 01 - JERRY JAFFE I.MP3
02 - JERRY JAFFE II.MP3
03 - JERRY JAFFE III.MP3
04 - JERRY JAFFE IV.MP3
05 - JERRY JAFFE V.MP3

LENGTH: 372 MINUTES

[BEGIN 01 - JERRY JAFFE I.MP3]

NANCY CAMPBELL: How you would characterize the state of knowledge in the field of psychopharmacology when you first entered it?

JERRY JAFFE: Drugs that could treat psychiatric disorders were just beginning to be developed. As I recall, in 1952 chlorpromazine was being promoted only for the treatment of nausea. In a way, I entered the field in the 1950s, when I was an undergraduate at Temple University studying experimental psychology. I had an experiment running rats and testing ways to measure pain thresholds that I thought were better than how other people were doing it.

NC: How were other people doing it?

JJ: They were using a tail flick technique and similar methods that depended on reflex responses. Instead, I put the animals in a choice situation. They could choose no current, low levels of electric current, or between two levels of current. I thought the next step would be to look at what happens when you give the animals an analgesic, but the first issue was to develop the choice situation. That's what I did for my master's thesis in experimental psychology. I was aware of Hebb's work in Montreal at the time, and I thought that was the research direction I wanted to take. So I spoke to the chairman of psychology at Temple, Professor Hubert Hamilton. He said that you need to go to

medical school if you want to do research because the medical doctors seem to get all the grants. I didn't know much about that, but I applied to Temple Medical School and got in. My goal wasn't to become a clinician, but to become better prepared to do work in psychopharmacology.

NC: You've said that there were two breakthroughs that really interested you: one was chlorpromazine, and the other was the discovery of the brain reward system. Can you tell me what exactly excited you about those? Why were you drawn to them?

JJ: I was not aware of Jim Olds's work, published in *Science*, until I was in medical school in 1955. I became much more interested in it quite a few years after that, when I went to work with Seth Sharpless at Albert Einstein. But the idea of finding drugs that could affect behavior, that you could use to treat psychiatric disorders, became interesting to me when I was in medical school. In my second year I got a summer fellowship to work for an associate professor of pharmacology, Sydney Ellis, a very smart and kind man. He encouraged my interests in pharmacology and research, and he became a kind of mentor to me. We maintained a friendship until he died a few years ago. Then,

in my last year of medical school I found Abraham Wikler's monograph, *The Relation of Psychiatry to Pharmacology* (1957), in which he wrote about the importance of operational definitions. These things made a lot more sense than the tautologies that were inherent in psychoanalysis, which dominated psychiatry at the time. I thought, this is the only thing that's ever really made sense to me in psychiatry. If I'm going to do research, wherever Wikler is working is where I have to go. But I didn't know where Wikler was.

It turned out that he was in Lexington, Kentucky, in the Public Health Service. So for my internship I signed up for the Public Health Service. There was a doctor draft at the time. I knew that I'd have to fulfill my military obligation and the Public Health Service served that purpose, and I thought if I signed up, somehow I'd be able to get assigned to Wikler's lab. I was very naïve about how things worked.

NC: What did you learn, if anything, about addiction in med school?

JJ: I don't recall learning anything.

NC: So what you learned about addiction you learned from Wikler's book?

JJ: No, I didn't learn about addiction from Wikler's book. Wikler wrote about psychiatry and pharmacology. It was not about addiction, and I was not interested in addiction. I was interested in psychopharmacology.

NC: Was it his commitment to operationalism that spoke to you?

JJ: Yes, but also he reminded me of Professor Hamilton, who was a fascinating man, and perhaps the first real scholar I had ever encountered, given my limited experience and background. If you asked Hamilton a question, he would pause, and he'd maybe scratch his moustache a little bit, and you could almost see him going through the index cards in his head. Then he would say something like, Hmm, that study was done by Muensinger in 1922. And then he would cite the journal and the page number. He had an encyclopedic knowledge of his field. Wikler's monograph had almost 1,000 references. Remember, in those days, you didn't have computerized databases and the Internet for searches. I wondered how he could possibly have amassed that

amount of information, and synthesized it, and organized it, and put it into a book in such a coherent way. It boggled my mind. I thought, this guy has his arms around the science. His brain around it, anyway.

So, it was my goal when I joined the Public Health Service to go and learn something from Wikler. It turned out that it didn't work that way, because once you've signed up they put you where they want you. After serving as an intern at the USPHS hospital on Staten Island, I wound up with a choice of federal prisons, Indian reservations, or the hospital at Lexington for the rest of my obligatory service time. The residency in psychiatry was at Lexington, so I opted for Lexington and started to become a psychiatrist there, still hoping that proximity to the Addiction Research Center would give me the opportunity to meet Abe Wikler.

Fortunately, the researchers at the Addiction Research Center – Harris Isbell, Abe Wikler, Frank Fraser, Carl Essig, and others – held seminars on Saturdays, and the residents could attend them. That was how I got to know them. I learned a lot from them, probably more than I learned about psychiatry and behavior in medical school.

This was all before the DSM (Diagnostic and Statistical Manual) set out clear-cut criteria for psychiatric disorders. The idea at the time was that all the disorders were just different manifestations of unconscious conflicts, and that you had to understand the dynamics of these conflicts. Diagnosis wasn't very important.

NC: What happened during medical school that gave you the impression or reinforced your impression that psychoanalysis was dominant? Was there a negative attitude towards experimental psychology?

JJ: Did anything happen other than the fact that everybody at Temple talked only about psychoanalysis and believed in it? I once asked one of the lecturers where I could find the evidence for one of his statements about ulcerative colitis being a manifestation of psychic conflict. He looked at me as if I had said something unprintable or had done something unforgivable. I guess that's an attitude.

NC: So you had some personal experiences of that kind, where you felt like your experimental orientation did not meet up with the dominant psychoanalytic mentality?

JJ: I would say, yes, that's true. In the case I just mentioned, I learned not to ask questions like that again in that department of psychiatry. If I had posed a similar question in the department of medicine they would have had no hesitation citing published studies, often controlled studies.

NC: Did you have any prior experience with addicts before you got to Lexington?

JJ: I recall only one. During my internship, there was a patient at the PHS hospital on Staten Island who had peripheral artery disease and had already had a leg amputated, but could not be persuaded to stop smoking. I don't recall seeing anyone addicted to opiates.

NC: How did you learn at Lexington? How did someone like yourself, who was a relative neophyte in terms of addiction, get initiated into the ins and outs of detox and treatment? What was a typical day at Lexington like?

JJ: Medical officers who weren't part of the psychiatric residency training typically ran the medical aspects of the detox unit. They did many of the admissions and detox.

Although as residents we rotated through the admissions unit for a period of a month or so, the major job of psychiatry residents and staff was to provide group therapy. That wasn't too hard because all the patients were confined in the hospital/prison setting so they would come to the groups. Each day there were patients to see, groups to run, rounds, and administrative meetings. There were also didactic seminars, and there was supervision when you would discuss your patients with a supervising psychiatrist. We also rotated through Kolb Hall, a PHS psychiatric facility on the grounds that had nothing to do with addiction. We spent a month or two there taking care of psychiatric patients. There were senior residents and staff to supervise us. As in all residencies, you had a hierarchy of people that could teach you.

We also had supervision from the senior staff. Two of the senior psychiatrists on staff at the time were J. Fred Maddux and Sherman Kieffer, who was a very wise psychiatrist. I might even have had some supervision from Murray Diamond, who was the Medical Officer in Charge. There was no question that we had adequate access to senior staff and other residents who were senior to us.

One day a week we went to the University of Cincinnati for part of our psychiatry training at the Department of Psychiatry there. On Saturdays there were teaching seminars. We also had officer-of-the-day duty (including night) approximately once a week.

NC: What would you be responsible for when you were officer of the day?

JJ: Everything. All the medical problems, all the surgical problems. You could call specialists in, but this was generally not a sick population. It was not like being on duty in a general hospital. Some of the patients or prisoners would try to get a new doctor to prescribe something, but that was more of a game than anything else. Serious medical or surgical problems were uncommon.

NC: Why didn't you continue your second year in the psychiatric residency?

JJ: I thought that the patient population was too limited. There was a limit to what you could learn about psychiatry from patients with a very narrow spectrum of psychiatric disorders. Also, I was still interested in learning about

psychopharmacology and continuing in this residency was not going to accomplish that. But I had to stay in the PHS for another year, and in that second year I was given those assignments that they didn't give to the residents.

NC: What was the nature of those assignments and what did you learn from that experience?

JJ: One was running the detox unit, where I actually learned a lot. When you admit 3,000 patients in a year you get to meet a lot of people and you get to see a lot of variation. What else did I learn? I learned about life on a Coast Guard cutter in the north Atlantic in March. I learned how to pull a tooth while on that Coast Guard cutter. Part of what happens when you are not in the residency program is that the PHS can send you anywhere they need you. Since the Public Health Service also supplied medical officers to the Coast Guard, I spent six weeks as the medical officer on the Coast Guard cutter Mackinac.

NC: They really did give you the jobs nobody else wanted. Did you have many encounters with Wikler, Isbell, or Fraser when you were at Lexington?

JJ: Fortunately, I had many encounters. And speaking of encounters, I had a fascinating encounter with someone else entering the PHS when we took our Pennsylvania state licensing boards. It turned out that he, too, was being assigned to Lexington. He wanted to be a general practitioner someplace in western Pennsylvania, and he was bemoaning the fact that they had assigned him to this job in a research unit with some guy named Wikler.

NC: That's really a twist of fate. You would have really liked to have been there at the ARC, and they probably would have loved to have had you there.

JJ: I think so. Abe Wikler and I got to be fairly close. At Saturday seminars he could be sometimes arcane in his presentations. There were people attending the seminars who just wanted to be psychiatrists, and their eyes would glaze over. But to me, it was like being in heaven, and Abe noticed that I stayed awake. Also, the research staff and the clinical staff shared the same little lunch room, so I often had lunch with Wikler and with Isbell. Harris Isbell was terrific, too. If I recall correctly, both Wikler and Isbell took sabbaticals during the two years I was there, so only one of them was in Lexington at a time.

NC: Was it the case that there was much separation between clinicians and researchers?

JJ: Not at lunchtime. And you'd meet them walking through the hallways and certainly on Saturdays.

NC: Did you detect any devaluation of the clinical side among the researchers at the ARC? Did the researchers think the clinicians were doing a good job? Was there any kind of conflict or competition between researchers and clinicians in that era?

JJ: If there was any conflict or competition, I never perceived it. It was certainly not at my level. They were just terrific. You could talk about issues and problems, and specific patients. And Abe Wikler had a great sense of humor.

Ironically, about three or four years after I'd left Lexington, Abe retired from the ARC and I got a letter asking me if I would be interested in applying for that position. By then, I had finished a post-doc in pharmacology and my residency in psychiatry and was teaching

at Albert Einstein. But as flattering as it was, it didn't seem right for me at the time.

NC: Do you regret that decision?

JJ: No. I'm sure I could have done better science and certainly the ARC had the infrastructure for better science, but I think by that time my inclinations were already moving more into the area of treatment.

NC: When you were at Lexington, was there anything that you would have described as clinical research, on the clinical side, or treatment evaluation, or anything of that sort?

JJ: John (Jack) O'Donnell was doing follow-up studies to find out what happened to addicts after they had been in treatment at Lexington.

NC: Can you give me a feel for how you would characterize Lexington at the time that you were there? Would it be fair to say that it was off the beaten path?

JJ: It was off the beaten path with respect to general psychiatry, but it was the center of the world for learning

about drug addiction. You knew you were looking at a very limited psychiatric population, but they were doing their best to train you and to talk about broader things than addiction. We saw patients with various psychiatric disorders at Kolb Hall, and they brought in guest lecturers. Wikler brought in some very smart people. We were learning neurology from Erwin Straus, who was a phenomenologist. He and Wikler would have these wonderful discussions about the philosophy of science. As I look back on it, I was exposed to some very, very smart people at Lexington who I think could hold their own with any of the people in better known academic centers. Nevertheless, my decision was to learn more pharmacology elsewhere because I was still interested in that area in general.

NC: So how did you go about learning more about pharmacology?

JJ: At the time NIMH had some postdoctoral fellowships and I was offered one. I narrowed my choices down to two places. One was an institute at Michigan for the study of the brain where Sam Gershon ran a program at the Ypsilanti State Hospital. Sam was doing the kind of work that I thought was interesting. He was administering drugs to people to look

at the reactions that characterize the drugs. But then Sydney Ellis, who had given me the student fellowship in medical school, said to take a look at Alfred Gilman's department at Albert Einstein, in the Bronx. The fact that the department was headed by half of the team that wrote *The Pharmacological Basis of Therapeutics* certainly interested me. When I visited, I met people like Murray Jarvik and Seth Sharpless, and there was just something about the people and the place that made me feel, this is it.

NC: There must have been considerable contrast between Albert Einstein and your experience at Lexington.

JJ: The contrast was really interesting. I felt that I was involved in world-class science. Seth Sharpless was doing some fascinating work, and so was Murray Jarvik in psychopharmacology. Seth had come from McGill, and was actually doing studies to follow up on some of the work that Jim Olds was doing. My postdoc position was called an "interdisciplinary fellowship." You could work with different people, and there would be interdisciplinary seminars. It was a mix of neurologists, physiologists, pharmacologists, and psychiatrists working together, but my work was primarily in pharmacology. Every fellow was

assigned some lectures to give, some seminars to present, and some research to do. When I first met Gilman, he said, "We're pleased to have you here. What would you like to do?" Nobody had ever asked me, in my entire life, what would you like to do?

NC: Did you know what you wanted to do?

JJ: Not immediately. I began talking with Seth Sharpless and Murray Jarvik about what they were working on. Seth was working on supersensitivity. Somehow, whether it came from him or from me I don't remember, from our discussions an interesting idea emerged that supersensitivity could be a model for the rebound withdrawal seen when certain drugs are abruptly discontinued.

NC: Could you explain denervation supersensitivity?

JJ: Here is an example. If you cut the nerve leading to the nictitating membrane of the cat, and give a small dose of epinephrine, the nictitating membrane contracts. It becomes increasingly sensitive, until over two weeks it's far more sensitive. In the literature this phenomenon was called

"denervation supersensitivity." By taking away the input, the post-input structures become more sensitive.

Seeing this as a model for physical dependence was a very simple idea. If you think of morphine or a barbiturate as acting to decrease neural input, wherever the neural input was reduced those neurons would become more sensitive to the normal transmitters. When you took away that drug, whether morphine or barbiturate, you would expect to see rebound hyperactivity when the normal input came back. That is essentially, and is still described as, the nature of withdrawal. It's rebound hyperactivity. All the neural systems that were deprived of normal input become hyperactive. The general notion had been described by Wikler, who noted that opiates suppress polysynaptic reflexes, and they get more active during withdrawal. The pupils contract; they expand during withdrawal. Over and over, you would get a response opposite to the activity reduced by the drug. At Lexington, they talked about counter-adaptation theory, but did not speculate on the mechanism. This supersensitivity model was not a molecular explanation, but it was a step in that direction. By 1961, when I got to Einstein, the idea of receptors had not been that well developed. Later, Collier published a paper about

changes in receptor numbers playing a role in withdrawal, but that might not have come out until our work was in press.

NC: Had there been any talk of receptors when you were at Lexington?

JJ: Not as I recall. We didn't know that there were receptors for the opiates, or for the barbiturates, yet. The notion of receptors was beginning to be discussed, but it took another six years before they discovered an opiate receptor, and even longer before they found a receptor for the barbiturates. But clearly Seth Sharpless and I were saying something happens in the cell that is deprived of normal neurotransmitter input. We didn't know whether it was on the surface of the cell, or in the machinery inside the cell. We said that in one of our papers. We called it "disuse supersensitivity." When I arrived at Einstein, Seth was already working on denervation supersensitivity using slabs of the brain. In that work, you undercut the brain slab and thereby deprive it of the input from below. When you stimulate it electrically over time you get longer and longer bursts of electrical activity.

NC: Did you do this in human brain?

JJ: No, we used cat brain. I suggested that we look at decreasing input by using barbiturate-induced deep sleep. At the time, it was believed that it took several weeks to get addicted to barbiturates, but when I really looked at the literature, I concluded this was because they didn't suppress the input enough. I thought that if you have only a modest decrease in input supersensitivity will take longer to develop. But if you can really suppress it, the process will take less time.

So we kept the animals in an almost coma-like state for a few days and then tested for seizure threshold. Since barbiturates raise seizure threshold, a withdrawal rebound hyperactivity would consist of a lowered seizure threshold. In cats, we measured seizure threshold beforehand, put them on barbiturates, and after abrupt withdrawal we measured the seizure threshold again. Our question was, how quickly would you see supersensitivity if you can induce deep barbiturate sleep. When we kept animals down for just three or four days we saw the change in threshold.

We were drafting our paper about supersensitivity in the brain as a mechanism for withdrawal phenomena; by that time it was late 1962 and Emmelin in Sweden had published a paper suggesting the existence of pharmacological denervation supersensitivity in the central nervous system. Although Emmelin was working with the salivary gland, blocking the salivary gland with atropine, he said his discussion, try to imagine this happening in the brain with a drug: as you block transmission in the brain, perhaps you get supersensitivity within the CNS. The idea was in the air.

NC: Did you write it up?

JJ: We wrote up the barbiturate supersensitivity. Basically the idea was that chronic blockade by drugs in the CNS causes supersensitivity.

NC: I take it that Seth Sharpless was not that interested in morphine addiction or tolerance to barbiturates?

JJ: No, not specifically. He was interested in disuse supersensitivity.

NC: Did your experiences at Lexington factor into your making that kind of connection?

JJ: Yes. Seth wasn't aware of the notion of withdrawal syndromes consisting of rebound hyperactivities. You had to know the literature about addictions, barbiturate addictions and other addictions, to know that.

NC: Was it any more than knowing the literature? Was it also having had the experiences that you did in terms of detox and seeing people go through it?

JJ: Not necessarily. If I had been interested enough in addictions to read the literature, I would have been aware of the rebound sensitivity notion. Nevertheless, the thing that got me reading that literature was being sent to Lexington.

NC: If you don't have a Lexington to be sent to, then you...

JJ: Then you have no reason to learn all about what constitutes barbiturate or opiate withdrawal because we

didn't treat addicts back in those days. The policy was to avoid bringing them into the medical system.

NC: Did Emmelin's publication basically end your work in that area?

JJ: Not at all. It was just that we didn't have the pride of saying, look, we are the first to have postulated blockade in the CNS results in increased sensitivity in the CNS. This is not denervation supersensitivity, this is pharmacological denervation sensitivity. That was the title of Emmelin's paper: "Pharmacological Denervation Supersensitivity."

NC: Would that have been the prize that you would have liked to have had?

JJ: Yes, that would have been gratifying. We had formulated and tested the hypothesis independently. But, who knows for how long Emmelin had had that idea?

The work I did at Einstein with Matthew Friedman involved cholinergic blockade in the CNS and then looking for the rebound effect. We looked at what happens when you give a

cholinergic blocker like atropine or scopolamine. We looked for a simple model and we chose body temperature, which goes down with a cholinergic agonist such as pilocarpine or oxotremorine. We postulated that if we blocked that cholinergic system and then withdrew the blocker we would get a rebound response, the temperature regulating system would be more supersensitive to cholinergic agonists, and we would get an exaggerated response to pilocarpine or oxotremorine. This was exactly what we showed. Basically we were trying to show a general principle, that if you block neurons of a particular type adaptation develops, so that when you give the normal agonist, you get a rebound effect.

At that point I got an Early Career Investigator award from NIH to continue with the work on the nature of physical dependence. This was now 1964 or 1965. It had been and continued to be a very busy few years for me. There was the residency, the fellowship, teaching in two departments, research in the lab, and writing the drug abuse chapter for the third edition of Goodman and Gilman’s textbook. I also took night calls in the Bronx to supplement my income, as I had a wife and 2 young children. And patients, drug addicts who knew me from Lexington and had come back to New York and relapsed, were calling me to ask for treatment.

By that time, Bill Martin and Abe Wikler had conducted some clinical work with cyclazocine, a long-acting narcotic antagonist developed at Sterling-Winthrop. It was the first long-acting, orally effective opioid antagonist to be developed. Nalorphine was short-acting and had a lot of dysphoric effects. Cyclazocine had some dysphoric effects, but it was long-acting. Wikler had postulated that what perpetuates addiction is the reinforcement you get each time you shoot up. The pharmacological effects of the drug induce positive affect and also alleviate withdrawal distress. Withdrawal distress also becomes linked to environmental stimuli. He reasoned that the best way to treat addiction might be to allow people to shoot up but get no effect; that this could be done by blocking the effect of the drug with another drug; and that eventually this blockade would lead to extinction of the response. Since a drug user who is taking an antagonist won't be physically dependent, there won't be any withdrawal, and eventually conditioned withdrawal will be extinguished, too.

In those days you didn't need an IND, or if you did they were easy to get, and there were no IRB's. All I would have to do to test the hypotheses would be to get the drug, since

I already had all these heroin addicts who wanted treatment and who couldn't get onto the methadone program that Vincent Dole and Marie Nyswander had started.

NC: Wait. How did you have all these patients? How did you maintain your connection with addicts once you were at Albert Einstein?

JJ: They found me. They would show up at Einstein and call me there. They knew that I was in the department of pharmacology. How do addicts know? Patients know everything. This is one of the early things you find out.

NC: Had you had somehow acquired a reputation for treating folks in New York?

JJ: No, but I had had some media exposure that might have accounted for it. I was invited to be on a radio program, Les Crane I think, that they might have heard. I never really asked, how did you know, how did you find me? Word got out. People came. There was a real network among those people, and they couldn't get onto the methadone program that had started in late 1964.

NC: Do you mean Dole and Nyswander's program?

JJ: Yes. By that time they were slowly expanding their program at Beth Israel Hospital. Anyway, I was able to get cyclazocine from Sterling Winthrop. I told some of the addicts who had contacted me and wanted treatment with methadone that there's a theory that narcotic antagonists might work, and asked if they were willing to give it a try, and many were willing. The amazing thing was that the drug didn't give them any reinforcement; it only promised to block the effects of the heroin they were taking, and they still wanted to try it. They still wanted to get off heroin. That was the main finding -- that heroin addicts seeking treatment are motivated. They don't come in to treatment to get high. At least some of them really wanted to stop. I'm not saying all, but there were some very motivated people. I had no trouble recruiting patients.

I did this work with Leon Brill and David Laskowitz. When patients came in, we detoxed them, then put them on cyclazocine, and we conducted therapy groups. We published a couple of papers on this work. It was interesting and it wasn't methadone. It attracted some media attention and was written up in Time Magazine. We published a couple of

papers on these studies. Laskowitz continued to be interested in the antagonist approach and to write about it.

By that time, I had also become familiar with the therapeutic community approach to treatment. I had visited Daytop Village and met the people involved, and I'm pretty sure I had already visited Synanon in Santa Monica. So now there were three treatment options being tried: methadone, therapeutic communities, and narcotic antagonists.

NC: Were you pretty convinced that cyclazocine would work at a theoretical level?

JJ: Not entirely. You couldn't know until someone did the experiment, but if I hadn't thought there was any chance that it would work, it would have been unethical to try it. It looked like it made sense and it did block the effect of heroin. At a minimum, it might prevent a death from an overdose. So it had some advantages and we started using it. The Department of Psychiatry got me a few beds in a sparsely used TB hospital on the Bronx Municipal Hospital campus. So we had a place where we could withdraw patients from heroin before starting them on cyclazocine.

I was also interested in the issue of opioid tolerance. One of the older hypotheses about why opioid maintenance wouldn't work (this was before anyone had heard of methadone) was that addicts would always escalate the dose, would want more and more, would never plateau. If the physician prescribed more and more opiates, it was argued, the addicts would sell them, thus creating more addicts. I wanted to ask a question about the influence of contingencies. What would happen if you said to an addict, I will provide this dose of this drug for you under these conditions: If you insist on more we'll end this experiment and detox you or switch you to methadone. Also, if you come up positive for illicit opiates (I used some of Vincent Dole's techniques and thin layer chromatography) – we'll end the experiment. I established a number of conditions.

Marie Nyswander had sent me a patient who refused methadone because he insisted he needed the thrill that came with shooting heroin. Later Marie told me she thought he was schizophrenic and was happy to unload him. That patient agreed to my conditions. I began providing him with injectable oxymorphone, which he picked up at a local pharmacy near my office at Einstein. So the question was, How long would somebody stay at the exact same dose and

remain abstinent from illicit opiates given the contingencies that I described? This patient was stable for about a year and a half. I should add that I was visited periodically by an agent from the Bureau of Narcotics. They wanted to make sure that I was doing this research with their permission.

NC: What were your interactions with them like?

JJ: They would check up on me and I didn't mind. I thought, that's their job, I'm doing mine.

So, this one patient was able to remain functional at the same dose of oxymorphone. He never used another opiate because I was testing his urine at least once a week. He frequently complained (not demanded) that he wanted more, but he managed. I always offered to put him on oral methadone; he always refused. At some point I began to use oral methadone with other patients and I immediately recognized that this was much easier on the patients and on me.

NC: Easier in what sense?

JJ: In the sense that they didn't complain or call asking for higher doses. I had only a few patients on methadone, maybe only a half a dozen altogether. They also picked up their medicine at the pharmacy. I don't remember whether it was every day or every other day, but it was certainly frequently. In terms of drug use they did well. They weren't using heroin. We did all the testing in my lab. I was seeing these patients myself. What I became convinced of was that there were differences in the kinds of patients who will accept one treatment versus another, and that patients on oral methadone functioned rather well.

I was also considering how to address the issue of potential diversion. The diversion issue was a real problem then and is still. The effort to minimize it places a real burden on many patients who have to come for their medicine every day. Back then you had to come for medication every day. I thought, surely we can do better. I knew about LAAM (l-alpha-acetyl methadol), which has a longer duration of action than methadone, because I had been given a full set of reprints from Lexington. I had read them all and noted the work on l-alpha-acetyl methadol. (It wasn't called LAAM then; that was a name I gave it when we first published on it in 1969.)

What I didn't know was where I could get the drug. A study had been done at Lexington many years before, I think in 1946, and it was now '64 or'65. When I called Merck, the company that made it, they suggested that I should call Paul Blachley in Oregon. The drug had been tried as an analgesic in a study there, but the study was stopped when they observed some toxicity. They had not allowed for the drug's slow rise to steady state. When the work was stopped, Paul Blachley, a psychiatrist I knew, had about a hundred grams of medication left over. So I proposed working together on an experiment using l-alpha acetyl methadol and he sent me his supply. Then I went over to talk to the department of psychiatry at Einstein about getting some space where I could conduct the study and give out the medication to patients. I couldn't use the local pharmacist for this study because it was still an investigational drug.

Recall, there was the old TB hospital where they had let me use space to detox patients. Some of the floors of that hospital were filled with old iron lungs and some were empty altogether. But the space was now "owned" by various medical departments. So on one floor I found a very large janitor's closet. It was a generous-sized janitor's closet,

big enough for a nurse to give out some LAAM and keep some records. That's all I really needed. So I said, how about that? They waited a week and then said, no, the janitors have a special need for that room. Since there was almost nobody else using the building, it was clear to me that they were not interested in having addicts coming there for treatment. Now I had the LAAM but no place to actually carry out the experiment. Almost by chance, at that very time, Daniel X. Freedman took over the chair of the department of psychiatry in Chicago and offered me a position there.

NC: Can you just go back and clarify exactly the dimensions of the experiment that you just described? How many patients are we talking about on oxymorphone?

JJ: To the best of my recollection I never had more than two patients on oxymorphone.

NC: How many on cyclazocine?

JJ: There might have been 18 or 20. I don't know that we ever got to the random assignment part of the study. The first question was whether people would accept it.

NC: Were you administering cyclazocine IV?

JJ: Cyclazocine is given orally.

NC: Was the Bureau of Narcotics interested only in the oxymorphone and not the cyclazocine?

JJ: That's right.

NC: So it didn't matter to them what you were doing with cyclazocine? Did you ever write the oxymorphone up?

JJ: No, I never wrote it up, but I talked about it. One lecture I gave was at New York Medical College where I talked about the patient Marie Nyswander had sent me because I thought it was interesting that someone can be maintained at the same dose of an opioid for a long period of time if you had the contingencies properly arranged. But you can't consider a single case proof of anything. So under the right circumstances – and this came from Wikler's emphasis on contingencies – and under the right contingencies, people (at least some people) can continue to get some of the

reinforcing effects that they want from the same intravenous dose over very long periods of time.

NC: At the time were you familiar with the more behavioral work at the University of Michigan? Would you have a nascent language of reinforcement, of drugs as reinforcers?

JJ: I'm sure I was familiar with some of it because the idea that drugs are reinforcers was essentially Wiklerian thinking, but the language was already in wide use. By 1961 or '62, Gerry Deneau and Tomoji Yanagita at Michigan had developed very elaborate equipment that allowed monkeys to self-administer. I think I first saw it at a meeting of the Committee on Drug Addiction and Narcotics in 1962.

NC: Was that your first CDAN/CPDD meeting?

JJ: Yes. The Michigan researchers took us for a tour of the research facilities where, I think, we saw the animals self-administer opiates. The idea of drugs as reinforcers was generally accepted. Getting animals to self-administer was actually not entirely novel. Even in 1956 Sprague had them licking at drinking tubes to get opiates, and Wikler had

animals licking etonitazine from tubes. However, he never had them working hard to get it.

NC: Would you consider self-administration a paradigm shift?

JJ: Yes, once you linked up the idea that we can judge how much work animals are willing to do to get something, you raise the level of sophistication and the questions you could ask. Equipment which forced the animal to press a lever allowed you to measure the strength of the reinforcer by the amount of work the animal would do.

NC: When did you first hear about methadone being used as a maintenance agent? Surely at Lexington it was used in detox, but it was not used as a maintenance agent.

JJ: The first time, probably, was sometime in 1965, very shortly after Vince Dole and Marie Nyswander began using it. Lots of people were talking about it. Vince came up to Einstein, I think to ask whether we would be a site for one of the methadone programs. They had already published on the work they had done at Beth Israel. The initial Rockefeller study was only eight patients, and they were not

outpatients. The Rockefeller work didn't look at outcomes in the community over time. The Beth Israel study reported on the functioning of a larger number of ambulatory patients.

NC: At the time, would you have known that your mentors at Lexington would have looked askance at that?

JJ: I don't know that I thought much about what they would say about it. They would probably have looked askance at my using oxymorphone. They didn't look askance at using cyclazocine because that was essentially testing their theory. An equally valid theory was proposed by Dole, that after a while drug users develop a metabolic disorder that causes narcotic hunger. An antagonist can't deal with that. Dole's view was that you have to provide an opioid agonist that satisfies that hunger and allows them to behave normally. Vince was a very articulate, persuasive man. I didn't agree with everything he said. I didn't agree that once on methadone the drug users were perfectly normal and free of psychopathology. I think by that time I actually had a lot more experience with drug users and their psychopathology than he did. He had dealt with just eight people when he came up with that theory. After that it was

Marie Nyswander and Joyce Lowinson working at Beth Israel who took care of the next 45 patients.

NC: Many people accused him of publishing his 1965 JAMA results prematurely, of making a conclusion that maintenance would work before he really had the data to back it up. Do you agree or disagree?

JJ: You need to have a reasonable number of ambulatory patients. If in the presence of real world stimuli they weren't using heroin and they were working productively and not having difficulty, it was plausible to make the claim that this is a different approach. So I think Vince was right when he proposed oral methadone maintenance as a new treatment. I suppose having a concurrent group randomly assigned to detoxification would have been more rigorous, but at the time most of us were convinced that almost all of those detoxed would have promptly returned to heroin use.

NC: In your interview at ACNP, you claim to have done the first ambulatory stabilization on methadone.

JJ: To the best of my knowledge, I did. That takes me to Chicago. Up to that time, to get onto the methadone program

in New York you needed to get through an interview. Some people were rejected; they were thought to be unmotivated, or too psychopathic, or too unstable. That's why Marie sent me the patients that I worked with on oxymorphone. Patients who were accepted had to agree to spend six weeks at Beth Israel. That is a very big, expensive, front-end load for treatment, given what a hospital bed cost. I had already put people on methadone without first putting them in a hospital at Einstein. I developed a technique of putting people on a modest dose that I knew would not be lethal, but that would probably suppress withdrawal. I would give them a dose and say, look, this may not hold you, but you'll come back tomorrow, we'll talk again, change the dose, and gradually build you up. Was the effect as dramatic as keeping them in the hospital for six weeks? No. They were still tempted by all kinds of things out there. But from a public health perspective I didn't see how you could treat a lot of people, all of whom were demanding treatment, if you had only six beds in a hospital that turned over every six weeks. All you can do under those circumstances is treat maybe 50 people a year. When we started the program in Chicago we had hundreds of people on the waiting list immediately. For the heroin user there's a serious risk

of dying every day you're out there, and that was not something I was willing to accept.

Our methadone treatment in Chicago involved ambulatory stabilization. We also developed an inpatient unit at the University of Chicago Billings Hospital for people who wanted to detox. There were many who didn't want methadone, so we detoxified people in the hospital unit. Some were willing to try cyclazocine. John Chappel and I did a controlled study of cyclazocine in Chicago. I don't know whether there were other people in the country doing ambulatory maintenance. Our first ambulatory stabilization in Chicago began on the first of January, 1968. I still have a copy of that prescription in my files.

NC: What were the circumstances?

JJ: One of the patients I treated in New York, a musician, called me and told me about a musician friend of his he wanted me to see. He was a kind and gentle man who played the clarinet, was addicted to heroin, and had Hodgkin's disease. He had been refused treatment for his medical problem at the University of Chicago hospital because he was an addict. This made me very angry. We had been preparing

for some time to start the Illinois Drug Abuse Programs by June, 1968; but after meeting this young musician I decided that we could wait no longer. I started by prescribing methadone for him, which was dispensed from the hospital pharmacy. That's how it began, and there were a number of people who were angry at me for starting prematurely, and even one resignation. But we still had some staff already hired and we got the program going. Incidentally, Patient #1 did very well. His Hodgkin's disease was treated and he went into full remission. He eventually became a counselor in the program.

NC: Let's backtrack and talk about how you got to Chicago.

JJ: The first half of the story was that I wrote a grant to compare LAAM to methadone and both of these to detoxification; but then I couldn't find space to carry it out at Einstein - despite their having a nearly unused hospital with lots of empty space. I concluded that this was not a medical school that cared very much whether or not I got a grant or conducted a study. So, I had this bottle of LAAM that I got from Paul Blachley, and I had this offer to go to Chicago from Danny Freedman, and I decided to accept it. That's how I got there. There was about six

months of lag time, and I spent it at Rockefeller with Vince Dole and Marie Nyswander. In December of ’66 I went to Chicago.

NC: What did you do with Vince and Marie during that period?

JJ: I learned what they did and how they did it. I tried one study to determine whether methadone altered sensitivity to inhaled CO_2 (5%). That was standard method for testing the sensitivity of the respiratory center of the brain. The activity of this area is reduced by opioids. And the idea was to compare controls to patients on methadone maintenance. Unfortunately some people get very panicky when they inhale CO_2. Our first subject, a control, was such a person. We stopped that line of research. During those 6 months I was also a consultant to the State of Illinois Advisory Council on Drug Addiction and I made frequent trips to Chicago to advise on legislation and plan a program for treating heroin addicts.

NC: During this time would you say you were consciously moving away from being self-identified as a

psychopharmacologist to being someone who was specializing in addiction treatment?

JJ: That's what happened. Was it conscious? It was conscious in that I knew what was happening. I had the knowledge to be helpful to people; I was a physician. I could have said, let somebody else do it, I need time to work in the lab. But, ultimately I decided that I'd do it. When I went out to Chicago, I still had a Career Development Award, and Danny Freedman gave me a lab where I was supposed to continue working on the nature of dependence. That grant was going to pay most of my salary. In the meantime as an advisor to the governor's Advisory Council on drug abuse I had substantial input into developing a drug abuse treatment strategy for the State of Illinois. After much discussion, the Illinois legislature was willing to appropriate a large sum of money for treatment, but there were conditions attached which involved my agreeing to run the program I had essentially proposed. I recognized that I was facing a choice between continuing to do laboratory research and becoming a clinician/administrator. Although it was a form of blackmail, I felt I didn't have much of an ethical choice. A million dollars for the first year of the program was very big money in the 1960's. I thought there was

something fundamentally selfish about saying I would rather work on my Career Development Award research. But I knew I could not keep the lab going and do all the things that building this treatment program required, so I gave up the Career Development Award, much to the dismay of Danny and others at the University of Chicago who expected that it would pay my salary.

NC: Did that present a particular problem for Danny Freedman?

JJ: I think so. Even though my salary would now be paid by the State of Illinois for my work as director of their drug programs, I was giving up a grant with significant overhead for the University. The state program, some of which was run through the University, would be providing only a small amount of overhead and was, perhaps, of a lower level of prestige.

NC: What was your relationship with him like? How had you met?

JJ: Danny gave a talk on LSD at a symposium that I'd organized at Einstein on drugs of abuse and society. He was

smart, charming, and there was nobody like him in psychiatry. You had to like Danny. He was just smarter than everybody else.

NC: How did he know what he knew about drugs?

JJ: He had done some work on LSD and had spent some years working at NIH. He had worked with Conan Kornetsky. He had an interest in everything and seemed to know everything. He had just become editor of the *Archives of General Psychiatry*, so he read all the manuscripts coming in.

NC: What was the state of treatment in Illinois? Had there been any publicly funded treatment in the state of Illinois prior to this time?

JJ: It depends on how far back you go. There was nominally a civil commitment program, but to the best of my knowledge nobody was using it.

NC: So Illinois went to civil commitment about the same time that California and New York did?

JJ: I don't know when they passed the legislation. To the best of my knowledge there was no publicly supported treatment in Chicago. A small place, St. Leonard's House, did some group therapy and had a grant from OEO. Everybody said that the only way you could get treatment was to plead guilty to a misdemeanor and go to the Cook County lockup, where a kindly nurse would give you some tranquilizers. There may have been some private psychiatric hospitals in Illinois providing inpatient detox; and there probably were psychiatrists who were willing to help addicts get at their unconscious conflicts. Psychiatry has never been completely disinterested in treating the addictions. So if you had the means, I am certain that some help would be offered. But the question is, how effective are the methods, not how sincere or well meaning is the practitioner. There is still no convincing evidence that individual psychotherapy can make a difference. Now, I say that, even though I have occasionally used it successfully. I have treated high functioning addicts with interpersonal psychotherapy, and they got better. But individual cases are not proof.

Back to Chicago, as for the street addicts, the hundreds and hundreds of street addicts, (those using heroin with no means to pay for treatment), I don't think they had access

to help. I don't recall if state hospitals in Illinois were accepting addicts, and I don't recall anyone telling me that's what he or she had done. So in sum, to the best of my knowledge, there wasn't any publicly supported treatment.

NC: How did you get set up in Illinois?

JJ: I moved by family to Chicago in a great blizzard in January 1967, but I had been there many times over the preceding few months to consult with the governor's Advisory Council.

NC: What were they looking for when they came looking for you?

JJ: They didn't come looking for me. They came looking for Danny, and Danny was busy, so he sent them to me. At the time, they were looking for somebody to tell them whether they should have a therapeutic community, or a methadone program, or a big detox hospital, or listen to the police who were not enthusiastic about any form of treatment. The Advisory Council consisted of lawyers, an internist, a local judge, a policeman, the head of the narcotics bureau, and some others. They had wide ranging discussions about what to

do. It's hard to know what they really wanted, but they wanted to explore what options there were in order to decide what to do.

NC: Why did they call on your expertise?

JJ: What expertise? I was an assistant professor who had written a book chapter and published a few papers. They could have called Harris Isbell for expertise; they could have called Vincent Dole; they could have called lots of other people.

NC: None of them were going to come out to Chicago and set up a treatment program.

JJ: I wasn't going to set up a treatment program, either. I was just supposed to be a consultant. I had a Career Development Award to conduct research. I had a laboratory. I was going to be an academic psychiatrist. When I began, I thought I was only consulting.

NC: So what happened?

JJ: I told them that there are various approaches to treatment. You can use antagonists; you can develop an opiate maintenance program; you can have therapeutic communities; detoxification programs. You can select among all of these things. But I don't know what's good for Illinois. If I were you, I would set them all up and compare them and see what's good for your population. What else was there to say at that time? Clearly, methadone in 1966 was controversial, but it seemed effective. Lots of people didn't like the idea of maintenance, but they didn't know much about the other approaches either. In the end, the Advisory Council couldn't see any great objection to trying methadone.

NC: Did the methadone issue drive the advisory council's investigation?

JJ: Possibly, but I'm really not sure what they were considering before I got there. Once I put all the methods on the table, I suppose they were considering all of them. I don't know who else they asked for advice. But I believed if you were going to have anything that worked, you had to have indigenous people working in the program. You couldn't just have carpetbaggers coming in.

During my visits to Chicago I spent some time at St. Leonard's House on the Near West Side, where I got to know Father Bruce Wheeler, who was running it. They were doing the best they could. They had a place where people could meet, and they believed groups could help. When I learned that Illinois was set on starting a program, I immediately suggested that some of the more active recovering addicts at St. Leonard's go to New York and spend six months at Daytop Village learning how a therapeutic community works, and I arranged for them to do that. At first, Daytop had an attitude of, we're not going to give away our secret methods, but they eventually agreed.

NC: Was it as if they didn't want to give away their proprietary medicines?

JJ: Well, in some ways that was just how people viewed their skills and methods at that time. What I had told the Advisory Council was this: No single one of these methods is likely to work for all addicts seeking treatment. Should you only have methadone? There are lots of people who would not want that. Would a detox unit be okay by itself? Probably not. We were fairly certain that detox was typically

followed by relapse within the subsequent 6 months. But maybe you need to have it anyway so nobody can say you're forcing them into methadone maintenance. Therapeutic communities are fairly selective, hard to set up, and might not work. It takes leadership with some charisma to make this kind of treatment work, and they don't serve many people. So in terms of a major epidemic, you're not going to make it with only a TC or even several TC's. I told the Advisory Council that the approaches or modalities needed to be compared in terms of effectiveness.

I spent some time exploring what it would take to get things up and running. I even had discussions with Synanon about setting up a unit in Chicago.

NC: Were you involved in any of the lobbying, or in the structuring of the legislation?

JJ: I was involved in the structuring of the legislation. Jim Moran, a lawyer on the Advisory Council who later became a federal judge, actually wrote it. Once I agreed to run the program, the Advisory Council put it before the legislature. During the entire time we were working on a plan, people from the governor's office knew and approved of

what was taking shape, as did Dr. Harold Visotsky, who was head of the Department of Mental Health for the state and also a member of the Advisory Council. The legislation with the appropriation was passed just as I was arriving in Illinois. That's how I got to be a clinician/administrator.

NC: So they finally got you out of the lab and into a laboratory of another kind.

JJ: It was a laboratory of another kind. Starting from scratch with three different treatment modalities, with virtually nobody trained in any of them, took a lot of time and a lot of effort.

NC: Let me ask you about what you said earlier about being convinced that any viable effort would require indigenous people. How did you become convinced of that?

JJ: I'm not sure exactly how. I think if you look at what is needed for people to change you recognize that part of what changes people is hope, a belief that they can change, that change is possible, that there are people of goodwill willing to help them. What I saw in Dole and Nyswander's program was that they were hiring some ex-addicts on

methadone to help people both as counselors and examplars. AA is not just self-help, the members are also exemplars. This kind of instilling of hope has been going on since the Washingtonians. You help other people, and in helping other people, you help yourself. You needed that sense of, "yeah, I knew him from the street, and look what he's doing. I can do it, too." That's important.

My view was that we needed authentic Chicago people to participate in building the system. If I could have brought doctors in who were well trained from other places, I would have done that, but the truth was, there weren't that many in the whole country. Neither the state nor the university were paying salaries that would make it attractive enough to bring people into Chicago.

NC: Right. There was a lack of clinical training, but at Lexington, hadn't you had what amounted to clinical training in this area?

JJ: I was a psychiatrist who had seen addicts in an institution. My task now was to build a system that could treat addicts not in an institution. The data from Lexington were already at hand. Keeping people six months or a year

at Lexington did not prevent a high level of relapse. Under those circumstances, you can imagine what my response would be to the expense of a closed facility. Society views the drug addict as some kind of amalgam between a patient and a miscreant. They're willing to see him treated, but not as well as any other patient. It can't be too expensive and public resources for these kinds of activities are always limited. The question when you are responsible for using those resources is, how can I maximize their impact, given the size of the population in need? In Chicago, I would never have willingly spent what a closed institution costs. That doesn't mean that at a reasonable price a residential facility doesn't have its advantages, particularly when you're dealing with people with unstable housing. But the costs have to be reasonable costs because a dollar put into such a facility is a dollar not put into something else. You have to say, what's the best allocation? That's what I was busy doing for the five years at IDAP.

NC: Had you had any administrative experience before IDAP?

JJ: No.

NC: What difficulties did you encounter as you were getting IDAP up and running?

JJ: The most difficult thing, I suppose, was the conflict between knowing that you need to expand and recognizing that you didn't have enough time to truly train the staff in the procedures. I also knew that even a less fully trained staff providing some services was probably better than nothing, and that we would gradually expand the programs with a clinic here, a clinic there. I believed we had to get it going. People had to have some place to come in out of the cold, even if it wasn't the Taj Mahal. That was the critical issue.

It was always a conflict. If you're any kind of a good clinician, you can spot what's wrong. But if you stop to fix it, it means you're not busy doing everything else that needs to be done. You need to fix and expand concurrently, and they are almost incompatible. The state was willing to give us more money. From a cost perspective we were running an exceedingly efficient operation. Nevertheless, you just can't do a decent job when you're always functioning with the view that scandal could erupt at any time. There are

always people willing to find some deficit and write it up as if it characterizes the whole thing.

There obviously were potential conflicts of interest between the university and the state, although they were both very cooperative. Depending on how I arranged for services, I could take money out of the state pocket and put it in the university pocket. It was an awkward situation. The state did not pay a great deal of overhead to the university, and yet it was getting lots of benefits from the university's activity. How do you compensate a university for the resources it makes available? And not just for its physical resources, but for its powerful influence in the community? Very difficult to do, especially if you are concurrently a faculty member and the head of a state program. Usually it's the overhead that does that. But the situation in Illinois didn't provide for that kind of overhead so there was always some tension. Recruiting was also a problem. Some people who wanted to work in the program thought they were coming to the University of Chicago, to the Department of Psychiatry. But if I was paying them out of the state funds, I couldn't say, well, now you can be an academician and just give me a few hours a week. That is basically dishonest. Even when I was awarded a sizable NIMH grant for

treatment starting in late 1968, the conflicts persisted. I had to decide who would work on the grant to the University of Chicago and who would work for the State of Illinois; and even for those on the grant, how much time could be allocated to efforts not spelled out in the grant.

At the same time, IDAP could not have been set up solely as a state organization because it would not have had the flexibility that a university affiliation gave us. And it couldn't have been done solely as a university operation because the university would not have wanted want to take on the clinical responsibilities, particularly for neighborhoods not in their immediate vicinity. IDAP developed programs in Rockford, Peoria, and East St. Louis.

NC: Did you do evaluation research?

JJ: We tried to do studies of the programs we were setting up. I guess you could call it evaluation research. You're reporting on the outcome, what happened when you did ambulatory studies, and how well people did, what happened when you used different dosages of methadone. We probably should have published more, but there's a limit to the energy you can muster while developing a state-wide program.

There were some additional university people there who were doing research, and I was encouraging them to do it. I can't even remember how we paid them at the time. As I mentioned, the university finally got a service research grant from the federal government a year later. I was the principal investigator, and we paid some people from that. Patrick Hughes did some work on epidemiology, and with John Chappel we did a placebo-controlled study of cyclazocine. Some faculty members at the University of Chicago law school carried out a study on the impact of treatment on crime and arrest rates. Bob Schuster was recruited from Michigan and he set up his pharmacology lab and collaborated on some clinical studies. We finally did the first studies on LAAM.

But, as in any situation where you're conducting research, you really can't direct other people to do everything. They have to take some initiative and have the competence to do research. You can give them the time and the support from time to time, but you can't lay the questions out with that much precision. Pat Hughes did the epidemiology, and Bob Schuster, Ed Senay and I did several studies with LAAM. We demonstrated for the first time that patients could be stabilized on LAAM given only three times per week. Bob Schuster and I did some other clinical studies. Some of it

got published, and more could have been. But there was always the tension between doing one more study or opening one more clinic. In Illinois there was never a point at which there were not more people who wanted treatment than we could serve.

NC: For you, was it that the moral divide is such that treatment, in essence, wins out over research in that kind of situation?

JJ: Yes, you could say that. That was my role. Very early on, since there were more people asking for treatment with methadone than we could admit, we did a waiting list control to determine if people are better off on methadone than on a waiting list. When an opening came up for someone on the waiting list they would be called. I had met one of the people who was randomly assigned to the waiting list, and when we were able to admit him I called, and his brother said he had died of an overdose. That experience lives with you for a long time. Was it ethical? The program couldn't take everybody. What should we have done - first come, first served? I wouldn't have known how to do that because patients were applying for entry at different sites, so for a while we took them randomly.

We knew there were serious risks associated with heroin use, and if you got people into treatment, you reduced those risks. After about a year we were able to set up a therapeutic community, and then we had methadone programs, a detox unit, and a program for youth, and a TC.

NC: About how large was your methadone program?

JJ: The programs grew continuously, so it depends on what point in time you look at them. By 1971 we had admitted almost two thousand patients, most of them to ambulatory methadone.

NC: Besides the youth program, did you have any other special population programs?

JJ: At some of our methadone programs counselors had different philosophies. There were some ex-addict counselors who had been in therapeutic communities who were fairly tough on patients who didn't adhere to the rules and didn't come regularly, or who continued to use heroin. They would press for discharging them. Later we'd find out about patients who dropped out and ask why, and we'd be told it

was because they didn't show up, or because of some other rule infraction. So we started a new program. I don't recall whether we referred to it as the "losers clinic" or the "recycle clinic." It was called Second Chance. John Chappel took that on. We found that if you bring people back in who were dropped from one clinic, they do much better the second time. Of course we wondered whether with better trained, more compassionate counselors, they would have done just as well the first time? I still don't know.

I think we also developed a special program for pregnant women, but I don't remember recall whether it was distinct clinic. From time to time, some staff members urged me to find a way to provide more primary medical care within the drug treatment programs. I was acutely aware that to do so on taxpayer money would not be fair to the many working people who paid taxes but had no medical insurance themselves, so we provided only minimal outpatient medical services. But we did arrange to support two acute care medical beds at a hospital on the north side of Chicago.

NC: Was there any treatment geared specifically to women, or did they get mixed in?

JJ: Mostly they got mixed in. Our methadone clinics all had coffee for the patients. Mothers were always welcome to bring their children to clinic with them. When we started the residential program at Tinley Park women who needed to move in could bring their children. They talk about it taking a whole village to raise a child. Well, at Tinley Park there were a lot of adults and a smaller number of children. But the children were now surrounded by non-doped-out adults, so perhaps some had never been cared for quite as well. Also at Tinley Park there were people being maintained on methadone who hadn't been doing well on their own because they were drinking. They could move in for a while, as well. It was, if you will, a therapeutic community that accepted people on methadone. It was a very tolerant and unusual place. If you only wanted to stay two weeks, it was okay. If you wanted to stay three months, even better. We did detox there, too. Dr. John Lowney, a psychiatrist who lived not far from Tinley Park made rounds with a nurse. We had to give out medication for people on methadone anyway, so he could also detoxify people there. It didn't require a lot of special staffing. If Synanon and therapeutic communities could detox people on a couch with no medical support at all, (and by that time you could do alcohol withdrawal on an outpatient basis with only

vitamins), why did you have to have a thousand-dollar-a-day hospital bed to do what could be done as an outpatient?

NC: What was the inspiration for Tinley Park? How did it come about?

JJ: Tinley Park came about as a result of several distinct influences coming together at a single point in time. One was my belief that I needed to show that treatment was more cost-effective than arresting and incarcerating drug users. That led me, perhaps inappropriately, to seek to do the most I could with available resources.

The second was the recognition that some of the patients being treated in our ambulatory methadone clinics needed more support than could be provided on an outpatient basis: a place where there was more structure and where in a matter of a few weeks we might be able to influence problems such as excessive drinking or continued heroin use. While our therapeutic community was occasionally willing to refer someone to our methadone programs, they were reluctant to admit anyone to their facility who was unwilling to commit to the longer term treatment the TC espoused.

The third influence was my observation that exceedingly few of the patients admitted for detoxification to the University of Chicago hospital unit required any serious medical consultation. It was a very expensive way to effect opiate detoxification considering the high relapse rate. (At the time we had not yet fully developed our aftercare program.) I thought that if I had a residential facility that was not part of a hospital with all of its overhead and availability of round the clock medical staff, we could treat a lot more people with the same resources.

And the fourth and critical factor was Harold Visotsky, head of the Department of Mental Health, of which IDAP was a part. Harold told me that there was an empty staff building on the grounds of a state mental hospital in Tinley Park, not far from Chicago, that IDAP could have. With what it was costing us to run a 15 bed detox unit at the University of Chicago, IDAP would be able to operate an 85 bed facility at Tinley Park, with two contract medical beds at a community hospital for those who needed more acute medical care. Further, I had just recruited David Deitch and several of his senior staff from Daytop Village. David had had a falling out with the Daytop board of directors, and he and his staff were at liberty. I thought that with David’s help

we could create something at Tinley Park that had never before existed. Tinley Park had the beds, it had the structure, it provided food, it had a psychiatrist and a nurse, so it largely replaced the hospital based detox unit, and much to the displeasure of Danny Freedman and the University of Chicago, I decided to close that detox unit and establish Tinley Park.

NC: Was it kind of a closed institution?

JJ: No. You could leave. It was pretty much like any therapeutic community. You're here because you want to be here. If you want to leave, let us know. It was hard to leave because of where it's located, (Tinley Park is about 20 miles south of Chicago), but nobody was compelled to stay. It was voluntary. Tinley Park had nice grounds, and a couple of times a year we had picnics there for the whole IDAP staff and their families. People from our therapeutic communities, methadone programs, detox unit, abstinence group, all came down, played volleyball, had food. There was a certain sense of camaraderie and not rivalry.

Later, we were able to replicate bits of Tinley Park in the city when the Salvation Army gave up its big building on the

South Side of Chicago. We acquired the building and set up something the staff decided to call Safari House. It had residential beds, it had methadone maintenance, it had detox, it had outpatient, it had vocational training activity, all at one location on the South Side. It was run two men who had gone through our detox unit at the University of Chicago and then had done so well that they became important members of the IDAP staff.

NC: Was the multimodality language yours?

JJ: I don't know whether it was mine. I may have made it up, but I'm sure other people had used the word multimodality before that. I don't claim originality for it, but that's what we said the IDAP program was.

NC: There was an epidemiology cluster at IDAP consisting of Patrick Hughes and Noel Barker and Gail Crawford. You seemed to decide you were going to look at behavioral approaches, and epidemiological approaches. Those both seemed fresh at the time. What did you think you were doing going in those directions in particular. Why did you invest in those directions?

JJ: There was a paper by De Alarcon and Rathod published in 1968, titled "The Spread of Heroin Addiction in Crawley New Town." They saw that heroin came in, and then it spread from group to group. Part of the idea was that if we could identify a beginning epidemic, maybe we could get in and treat it and abort the epidemic. Basically the hypothesis was, maybe there are mini epidemics. Somebody's bringing in some dope, and it's spreading. So the first question was, where are the epidemics? Where are the users living? Are they clustered? Are they diffuse? What's the nature of how they interact? Pat Hughes wanted to do that, so I said fine, go do it.

NC: Why did he want to do it?

JJ: He wanted to do epidemiology, but he was also a physician. I needed to build depth in terms of doctors who were willing to work in this system. It's the kind of system where, if you don't have redundancy, you are at great risk. His price to work as a physician in the program was support for his epidemiological interests, and that was fine with me. He knew something about it because he'd been at the NIMH narcotic group, DNADA.

I had read the Crawley New Town paper, probably as part of writing chapters for Goodman and Gilman. The idea of intervening in an epidemic is that you first have to spot where it is. I guess everybody goes back to the Broadway pump.

Another thing we were trying to do, in a very primitive way before the technology we had really allowed it, was to see how we could use computers to keep track of everything we were doing – admissions, medication, lab tests, patient records, finances.

NC: So you were also invested in using new information technologies and urine testing technologies.

JJ: Yes, we built our own urine testing lab using Vince Dole's techniques. We didn't have much alternative because the commercial options that you have now didn't exist.

NC: What techniques were you using?

JJ: We had been using thin layer chromatography even when I was at Einstein. In Chicago I used the lab that they gave me for my research grant to set up the urine testing lab,

and we hired a full time lab director with a PhD in chemistry and added gas chromatography.

NC: And at the time, were there any concerns or thinking about civil rights issues with urine testing?

JJ: It was a clinical test. I would not have thought, if you were testing a diabetic's urine for sugar, there would be a civil rights issue. There was nothing in the air at the time about testing and civil rights.

NC: When did you begin to hear concerns about methadone as a genocidal agent from the black community? Had you heard that before you got to Chicago?

JJ: A little bit.

NC: Did you hear more or less of that concern in Chicago than in New York?

JJ: A little less, I think.

NC: Did you also, in Illinois, run into the conflictual division between TCs and methadone that had developed in New York City?

JJ: I started the TC in Chicago. I signed a personal note on some of the property. I hired the people. If there was any tension between the TC's and methadone, they were smart enough not to let it come to my attention.

NC: What do you suppose made the difference between Illinois and New York? In New York even then there were entrenched divisions.

JJ: I think it might have been the size of the egos of some of the people running the programs in New York. We didn't have anybody of that stature running our therapeutic community. Also, I controlled the budget. If you were going to criticize methadone, you were criticizing the person who was giving you your budget. And at that time I don't think anybody would do that. Furthermore, we brought methadone people into the therapeutic community so they could see how group therapy worked, and we rotated staff from the therapeutic community into the methadone program,

so they could come see how a methadone program works and offer the methadone staff some ideas on group process.

NC: Did you specifically set out to intermix modalities in that way?

JJ: Yes, I brought the people from the therapeutic community to our clinical staff meetings, and they sat next to people who were running methadone programs. We talked about what we had to do together.

NC: Are you saying those kinds of divisions and conflicts did not arise during your tenure at IDAP?

JJ: Not that I ever detected. There were some friendly rivalries. John Chappel and Matt Wright and Jeannie Peek were always kind of pleased that they had this Safari House going that wasn't using methadone. But the rivalry was more friendly.

The people in the therapeutic communities - at least back then before they became much, much bigger - saw themselves as part of IDAP. There were several programs involved, like Gateway Foundation and the BRASS Foundation that I

structured as independent, not-for-profit organizations. One of the problems you deal with when you're running a state program is how to accept volunteer efforts and charitable contributions. Typically, people don't give to the state, but they give to foundations. It's a way to amplify the resources available, and a way to give people just a little bit more initiative. They're not just dependent on the state. They have initiative to go out and do things. That was important. The bulk of IDAP's money came from the state, but the not-for-profits were also independent enough to raise money, have a board of directors, and do other things. We even set up a methadone foundation that could create independent programs and find ways to get their own grants. You didn't want people to always be state employees. There are limits to what a state employee is allowed to do. There are advantages to being semi-independent. BRASS became an independent methadone provider.

NC: Is BRASS an acronym?

JJ: Yes, for Behavioral Research and Social Services, or something like that. There was a time early on when the Gateway therapeutic community was run by someone I hired who

turned out to be more of a sociopath than I thought. I had sent some relatives and acquaintances with drug problems to Gateway and I got some stories back that told me that the leadership's behavior was unacceptable. I told the board of directors that we had to make a change, and they said no, we like our director. So I said fine, you keep him, but we will be setting up a new therapeutic community, and the new entity will get all the money. I wish you and the board of directors every success. It was the last week of the month before the checks had to be written, and I had already set up BRASS as a TC and hired new staff. Then the board of directors decided maybe a change was the right thing to do. The two TC's amalgamated and continued as Gateway, so BRASS became an empty shell. Later when we needed another methadone program, we used the BRASS shell and gave it a grant to get it started. BRASS is still operating in Chicago.

By the way, Michael Darcy, the head of Gateway now, came to Chicago as deputy director of BRASS, which was so named because people accused me of having brass apparatus because I was willing to have this confrontation with the Gateway board of directors. In the beginning of the process I told them, look, if we're going to play a game, I'm going to win.

I have the checkbook. I would not tolerate a corrupt organization.

NC: Why did you bring in Bob Schuster? What did you think you could gain from turning to more behavioral approaches at the time?

JJ: Well, actually it wasn't behavioral approaches that I was interested in. I was interested in nicotine at the time, and he had done some interesting work on nicotine. He had been one of the few people to administer IV nicotine to look at its effect on smoking. I had developed some interest in smoking. That was my primary interest in collaborating with Bob. But he had lots of other skills, and I thought he would be an interesting person to have.

NC: Can you tell me about your interest in nicotine, when it formed, and what you were thinking about it?

JJ: When I wrote the drug abuse chapter for Goodman and Gilman in 1964, I was looking at all the addictive disorders in a single chapter, so I read as much as I could. There wasn't that much on cocaine that I could access. There was a little on cannabis, but not much. There were a lot of

publications on opiates, and on alcohol, and barbiturates. The really interesting thing was that nobody was willing to see smoking as an addictive disorder. I was trying to understand the difficulty. Knapp and others had described withdrawal from smoking, so there was some evidence for that. Maurice Seevers at Michigan didn't agree with viewing smoking as an addiction. He called it a habit, not dependency. But I did not see it that way and I put it into the chapter the way I saw it.

It seemed to me that there were people for whom smoking was a very compulsive disorder that met all the criteria for an addictive disorder. As I mentioned, I had seen a man with Buerger's disease, (peripheral arterial disease), who continued to smoke, despite having amputations. If you say to somebody, if you continue to smoke, I'm going to cut off your arm, you'd think maybe you'd get a little bit of behavior change. However, Al Gilman, who happened to be a chain smoker, said, no, nicotine belongs in Murdoch Ritchie's chapter on ganglionic blockers. He couldn't buy the idea, given the context in which drug addicts were seen as morally depraved dope fiends, that compulsive smoking should be seen as an addictive disorder. He wasn't a morally

compromised, depraved dope fiend, and neither were the other smokers he knew. So, they couldn't be addicts.

In the first chapter I wrote for the Goodman and Gilman text (3rd edition, 1965), it says, regarding smoking, "It wouldn't be surprising if, in the future, people begin to recognize smoking as a compulsive disorder." I thought I should do some work on smoking as an addictive disorder so people would pay it some attention. So that's the background for my interest.

NC: What was interesting about Bob Schuster's work in that area to you?

JJ: Remember, the context was if you give methadone, people stop self-administering heroin. Well, what happens if you give nicotine? Will they stop self-administering smoke which contains nicotine? That's what he was doing.

NC: Had you worked with Murray Jarvik when you were at Einstein? He was also working on nicotine.

JJ: Yes. Murray had already begun to be interested in smoking and cigarettes. I was there at the time and that

probably was part of my interest. Murray had some smoking monkeys.

NC: Yes, I've thought about writing an article called "Teaching Monkeys to Smoke" about his work on figuring out the apparatus.

JJ: I also thought it was important to work on smoking because when you look at the broad dimensions of the problem, there were a lot more smokers than there were heroin addicts. I was interested in general principles of addiction rather than exactly how to treat heroin addicts. Was there some general theme, some common mechanism? Being interested in broad issues is a kind of a curse because if you're too broad, you never really get down to the details of any one thing. But when you're asked to write something that covers all of them, as I was early on for G&G, it gets you involved in all of them.

NC: Let's talk about writing the Goodman and Gilman chapter. The first time you wrote it, how did you go about writing it? It seems like a daunting task.

JJ: Well, it was a daunting task. It was the third edition of Goodman and Gilman's textbook, but the first time it was to be multi-authored. Al Gilman and Lou Goodman wanted people they knew to produce the chapters. Al Gilman asked me to write it. I had seen addicts at Lexington and I was working on the biology of physical dependence. Al Gilman asked me to write the chapter after I'd given a couple lectures on addiction. I knew something about the topic because I had prepared the lectures and I had read all the work from Lexington, which formed a fairly large portion of what was known about addiction.

NC: Would you say that the ARC had provided the primary basis for knowledge about addiction at the time?

JJ: Not entirely. With respect to barbiturates and opiates, yes, that was probably the primary basis, but there was also some historical material. Also, the Lexington researchers rarely dealt with treatment, so you had to fish that out from wherever you could because nobody was doing much treatment. Some of the material on alcohol and alcohol addiction was from Lexington, but much came from other areas. They had done some work on amphetamines and LSD, but

a lot of that came from other sources, as well. And they hadn't done anything on nicotine or tobacco.

Of everything I read on nicotine and tobacco, only one paragraph made its way into my chapter for the 1965 edition. In the fourth edition, 1970, they gave me a whole column. In the fifth edition, 1975, they let me take over nicotine entirely.

Back then, in writing both the Narcotic Abnalgesics chapter and the Drug Addiction and Drug Abuse chapter for G&G, pieces of the previous edition were incorporated. It was a directed and authorized form of plagiarism that had to do with the publishing technology of the time. The publishers had the printing plates from the earlier edition. We were told that it was preferable, when text did not need revision, to leave it place by (literally) cutting and pasting the copy between revised sections. I think Lou Goodman had written most of what had been included in previous editions on opiates, and also various scattered pieces on addiction. If I couldn't say it any better and I wasn't saying anything new or different I was encouraged to leave those pieces alone.

NC: Which pieces did you inherit? Did you inherit the history section? What did you decide you needed to replace?

JJ: I inherited anything he had written that belonged in my new chapter on addiction and in the opiates chapter, but if I read something that needed to be changed, I changed it. If it was already there and it was correct, I left it alone. So I'm sure there were pieces in the third edition that were left over from the second edition. By the fourth edition I couldn't tell whose writing was whose, but I kept changing it as things evolved. Some things never evolved, so there may still be words from the second edition in the tenth (or eleventh) edition.

NC: Now, in that first round, you were already confronting the definitional problem? Can you talk a little bit about how you came to be critical of the term "addiction"?

JJ: I wasn't critical of the term "addiction" so much as I recognized that it was used in many contexts without a very precise operational concept of what it was. The most important part was that people were equating addiction with the withdrawal syndrome. If you showed a withdrawal syndrome, you were hooked, you were addicted. But Wikler

and his co-workers had shown that you could give opiates for one day, and if you gave an antagonist - back then they only had nalorphine, which was a partial agonist, so it wasn't quite precise - you could show withdrawal. So, you could see that physical dependence probably began with the first dose. I don't think I was the first to say that. I think maybe Wikler was.

If physical dependence begins with the first dose, then just physical dependence is not what we mean by addiction. Otherwise everybody who's been given opiates in the hospital postoperatively would have to be defined as addicts because they had some degree of physical dependence, even if it was latent. But it could be demonstrated. And for some patients the syndrome, though subtle, could be observed clinically. Patients felt achy and somewhat dysphoric when opiates were stopped after a few days. So physical dependence could not be use as a synonym for compulsive drug-seeking behavior.

Now, how do you clarify the relationship? You clarify it by spelling out everything I just told you and write that you can be physically dependent without being addicted and addicted without being physically dependent. For example,

you're locked up in a jail and you can't think of anything but getting drugs, using drugs, even though the acute withdrawal syndrome has largely dissipated, and even though the risks of using smuggled in drugs are very substantial. But the focus in life is still drug-seeking. The two are related, but they don't map on each other precisely. I did my best to try to convey that idea.

At the time I was writing the chapter for Goodman and Gilman, I had just written a paper on rapid physical dependence on barbiturates. So I believed that if people were taking a certain amount of sedatives you could probably demonstrate early physical dependence, if you had an antagonist, but they weren't addicted to barbiturates. Until we had an antagonist for the benzodiazepines – which didn't take place until about 30 years later, in the 1980s – you couldn't demonstrate rapid onset of benzodiazepine physical dependence. But it was predicted that the adaptive changes that lead to this withdrawal syndrome begin very early, if not with the first dose. Anyway, that's how it came about, and that's how I wrote the chapter.

NC: One thing that struck me about the 1965 chapter was that there was not yet any language in it about drugs as

reinforcers. None of that language is there then. By the 1970 edition it is.

JJ: Well, actually it is in that first chapter. Under the heading of Etiology there is a section headed Learning, in which the repeated reinforcement of drug taking is mentioned. It is true that the heading of Learning and ideas of reinforcement were given more emphasis as the chapter was repeatedly revised. One of the difficulties of writing chapters is you're given a very rigid page allotment. In my chapter for the 3rd edition I tried to be as economical as I could be and I still came in 20 percent over my page allowance. That usually resulted in lightning bolts coming down from Lou Goodman or Al Gilman. But Al Gilman called me in to his office and told me that Lou Goodman said, we can't cut this. Put it all in extract (i.e., smaller) type.

NC: So that's why the type size differences.

JJ: Yes. They kept to the page limit, but they had to put it in small print because they couldn't find anything that wasn't worth saying.

NC: That must have been very flattering for an assistant professor.

JJ: It was. Wikler's notion of drugs reinforcing the behavior was in there, as was a little bit about conditioning.

NC: In the next edition reinforcement is used in headings, so it's more organizationally present. I was interested in not so much were the words there, but was there a change in the way you were thinking about the field?

JJ: I think that there could have been a little bit of both. I really can't recall what influences were impinging on me 35 or 40 years ago. More importantly, I was writing these chapters as I was doing the research and I was putting a tremendous amount of work into it. It was an organizational effort to take all the disparate pieces and put them into one chapter, and to pull out the common factors that underlie the phenomenon called drug dependence. The next step was to actually describe the separate syndromes.

NC: That was an interesting moment in terms of the lexicon because there were overall attempts to change the language of "addiction" to the language of drug dependence. There was an attempt at the level of the World Health Organization to move towards the language of drug dependence.

JJ: Well, the language was changing. I was writing in 1964 against the 1964 WHO criteria, and then in 1965 the WHO Expert Committee decided to change them. They recognized the shortcomings of habituation versus addiction, and they changed to drug dependence. I don't think their change in terminology influenced me, because the chapter had been submitted a full year before the experts at WHO decided to offer up a new categorization.

NC: You also seem to have had a very early and almost personal interest in narcotic antagonists. You saw use potential in them in a very clear way, and you wrote about that, as well.

JJ: In 1964, Bill Martin and colleagues at Lexington published their work on cyclazocine, an orally effective antagonist. I think Bill presented the theory of cyclazocine as treatment at a CPDD meeting, and by 1965 Leon Brill and I

started the first cyclazocine treatment study. Wikler proposed that antagonists would be used to block the reinforcing effects of opiates, and that such blockade would eventually result in the extinction of drug using behavior. I thought the idea sounded reasonable.

NC: When did you first start working with WHO and meet Griffith Edwards?

JJ: I met Griffith Edwards in 1969 when he came to visit me in Chicago. Then, in 1970, Dale Cameron, of the World Health Organization's Expert Committee on Drug Dependence, asked me to work with Griffith to develop a Working Paper on national strategies for the Committee. Working Papers become the basis for discussion at the next Expert Committee meeting. I don't know why Dale put me together with Griffith, but we worked together at Griffith's house in England in the summer of 1970 and developed a Working Paper for WHO. Only years later did I discover that Griffith had already held a conference at the Maudsley on national strategies.

NC: Was there anybody else at IDAP who you worked with that we haven't talk about? What about Ed Senay?

JJ: Ed was chief of the consultation liaison service in the department of psychiatry at the University of Chicago. He got interested in addiction and asked if he could play a role in the program. This was fairly early on. Ed eventually became completely interested in drug addiction, gave up consultation liaison, and when I left for Washington he became P.I. on the federal grant.

NC: In Michael Massing's book, *The Fix*, he talks about a conflict between you and Patrick Hughes over outreach and treatment slots. Can you explain that in more detail?

JJ: Sure. Pat wanted to have all of the new openings for treatment assigned to him so he could use them as incentives in his epidemiological work. He wanted to be able to say, Do this for me, talk to me, and I'll get you right into the program. I said, Pat, you can't do that. There are lots of people who want to come in, and not all of them are in your neighborhoods. We can do some of that; we can't do all of it. Treatment was a scarce commodity and many people wanted to get someone they knew into one of the programs. It was bad enough that I suspected some of the staff at the various clinics were putting their friends before other people. It

was very hard to keep an orderly and fair waiting list when you knew it could mean the difference between being arrested and not being arrested, taking an overdose or not overdosing. We couldn't make entry into the program contingent upon cooperating with somebody's research. It seemed to me we could do a little of that because the research was important, but it couldn't preempt ordinary procedures.

NC: How did your relationship with IDAP end?

JJ: I got "drafted" on June 17, 1971, when the President declared at a press conference that I would head his new drug office. But the process actually began in the summer of 1970. I had come back from Geneva, where Griffith and I presented the Working Paper to the Expert Committee, which took what we felt was beautifully crafted language on national strategies and chopped it into more bureaucratic prose. But that's what committees do. Then I got a visit from Jeffrey Donfeld. I didn't know him. He said he worked for Egil Krogh, who worked for John Ehrlichman, and I didn't know who they were, either. He said they worked at the White House, and that Bob DuPont had told him he should see my

program in Chicago, so I showed him around and we talked a little bit.

Sometime later that summer, Donfeld called me to ask what my response would be to the proposed FDA guidelines for methadone programs. The FDA, under the influence of Justice, DEA, and NIMH, was trying to stop the proliferation of methadone programs. They wrote a set of guidelines for getting INDs and for who could be admitted to treatment that were very, very restrictive. I responded with a six or seven page single-spaced letter to Jeff, or the FDA, I don't recall which.

This was still the summer of '70, as I recall, or maybe it was September of '70. Then Jeff called and said the White House would like me to form a committee to write a White Paper on what the federal government should do about treatment for addiction. Then he started laying down these various constraints. I had to recruit the people, arrange for them to meet, consider all the data, get it all written, and have in to the White House by December first. But, it would have to be done in complete secrecy and it could not be published. If anybody knew we were doing it, it would be useless to the White House. It had to be a secret document.

I understood later that if the Executive Branch wants to put forth a position and it leaks out, anything good in it will be claimed by the other side. They would say, the President's finally doing what we said he should do. If there's something they don't like in it, they'll criticize it before you even have a chance to explain.

I got the idea. But this was a difficult time to find anyone willing to work that way. It was the peak of Vietnam protests and the urban riots of 1968 weren't that far into the past. Some of the people I called said, if I can't publish it I'm not interested, and they refused to participate. Others said they were writing books themselves and they couldn't assure me that some of the things we would discuss wouldn't be put into the book. There were various reasons people didn't want to do it. It didn't pay much, it was secret, and it was for the Nixon White House. But I did get some people to agree to do it, and we wrote the report.

NC: Who was on the panel?

JJ: They were all people who knew something about drugs. There were psychopharmacologists, psychiatrists,

sociologists, a criminologist, somebody who was an expert on alcoholism. There were ten people - Jonathan Cole, Jack Mendelson, Helen Nowlis, Roger Smith a criminologist, John Kramer, Bill McGlothlin, Jack O'Donnell a sociologist from Lexington ARC who had conducted outcome studies, Gilbert Geis a sociologist, and Sydney Cohen a psychiatrist who had been director of the NIMH Division of Narcotics and Drug Abuse, and Ed Brecher who helped me write the report and who later wrote Licit and Illicit Drugs for Consumer Reports. They were people of considerable stature. I was very, very fortunate to be able to recruit them. And they all had to be willing to come together to work on this on weekends. We were able to access a number of additional people as consultants without necessarily telling them about the nature of the paper we were working on.

NC: Were you trying to kind of get some representation across the disciplines? Or didn't that really matter?

JJ: It mattered a lot. Helen Nowlis knew about prevention and school programs. Roger Smith was a criminologist. Jonathan Cole knew about psychopharmacology and certainly knew about the organization of NIMH and what it could do. Jack Mendelson knew about NIMH, and he knew

about alcoholism. Sydney Cohen had directed the NIMH, DNADA. John Kramer had worked in the California civil commitment program.

NC: At the time did you know that NIMH was also being asked to submit a report?

JJ: No, I didn't know they were writing an in-house report concurrently until sometime later.

NC: What was the process like?

JJ: We met at different places. It wouldn't have been fair for them to all fly to Chicago for every meeting. Eventually Ed Brecher and I just sat down and took all the transcripts and tried to put it together, and then they looked at it. It had to be short enough so people could read it. It had to be punchy if it was going to be read by anybody, especially at the White House staff level.

NC: Did it ever become available to the public?

JJ: I never published it and neither did the White House, but it didn't remain secret. It became available, because after I left government I let a number of people read it.

NC: What were your take-home messages in that document?

JJ: The take-home messages were about different drug problems. We had a problem with youth using psychedelics and marijuana. We had a problem with heroin. Basically, Jeff Donfeld said, look, if the President is willing to put a hundred million dollars into the treatment area, the prevention area, and the research area, what should we do? You guys are the experts; tell us how you would do it.

Our big complaint, expressed in the report, was that nobody should be asked to try to make policy in this way -- in six weeks without all the data, without thoroughly understanding what was going on in all the different agencies. We found out there were just too many agencies with a mandate to dispense money for treatment, prevention, and research. All of them had 5 or 10 million here, or 20 million there, and no coordination at all. There was no mechanism that said that these different groups ought to talk to each other. We uncovered 18 different agencies, all of whom were giving out

money often at cross purposes, or sometimes to the same people for the same purpose. There was no effort to find out how effective the programs were or what they were accomplishing.

We said there should be a cohesive way of looking across what you're doing, to evaluate it, to say what works and what doesn't work. Then we said, here's what we think you ought to do. First of all, you need more research. You don't have the necessary data to make sensible policy. The preamble to the report was partially plagiarized, (not fully since I had written part of it), or paraphrased, from the working paper Griffith Edwards and I had produced for the WHO, the paper on a national strategy.

NC: Apparently Bud Krogh told Michael Massing that the report added up to the interpretation that methadone maintenance was the only effective technique available. Would you agree with that interpretation of the report?

JJ: Not at all. But it did state that, on balance, methadone maintenance was a useful approach. As we were writing it, Jeff Donfeld was looking at what the other group was doing, and they were really anti-methadone maintenance,

raising all the objections to it. For example, it's giving the wrong signal, the minorities don't like it, there will be diversion, it will cause overdose deaths. So he kept telling us to answer this criticism, answer that criticism in the report.

NC: Who was he urging to answer the criticisms?

JJ: The ad hoc committee, but more specifically he kept telling me and Ed Brecher to address the criticisms. So the report got longer in terms of preempting or at least responding to criticisms about methadone treatment that Donfeld told us were being raised.

NC: Right. Did Donfeld tell you who was criticizing methadone maintenance?

JJ: I'm not sure he told us who was making the criticisms, but we got the idea that other people in the bureaucracy were making them, and these things were going to have to be dealt with. We felt methadone would be useful and needed to be there. In the final report there were four appendices on methadone dealing with pharmacological safety, regulation of programs, diversion, and accidental overdoses of methadone.

NC: But using methadone was not the main point of your group's report.

JJ: No, not at all. The main points were: you need to have data to make policy. You ought to have a coordinated strategy. You ought to have some coordination of all the money you're spending over all these agencies. Somebody ought to know what's going on and currently there is no mechanism. Maybe at OMB, somebody at the top of OMB would know where all that money was going. Considerable amounts were being spent and nobody knew what impact it was having.

Second, you ought to have a plan. What is it that you want to achieve? How did you want to achieve it?

Third, you need data to know whether your strategy or plan is working. You ought to be looking at things like accidental overdoses and emergency room visits – (this idea led to DAWN)- and you need national surveys. You can't know whether your policy is effective unless you put into place measures of the outcomes you want.

Lastly, there's just not enough money in research in this area. Actually it turned out there was a lot less in research than it seemed when we wrote the report because some of what they were calling drug abuse related research was really quite unrelated.

NC: Did you complete that document in time for the deadline?

JJ: Actually no. We were given an extra two weeks to deal with Donfeld's added questions, so it went in December the 15th.

NC: At what point did you realize that there was an NIMH document, a competing document?

JJ: I'm not sure. Might have been months later.

NC: Did you mention the multimodality approach in that document?

JJ: We may not have mentioned it directly, but we said that you need to have more than one kind of treatment. Opiate dependence was not the only problem we were dealing with.

There were multiple drug abuse problems. Methadone could only deal with heroin addiction. What do you do about barbiturate addiction and amphetamine addiction and other kinds of addiction? Kids using LSD couldn't use methadone. There was cannabis use. We thought there ought to be some support for therapeutic communities. Did that imply multimodality? It would have been ideal to have less rivalry and bickering, and more focus on the populations that need help. In no way did that document suggest that methadone maintenance was the only thing. If there was any emphasis on it, it came because there was so much counterpropaganda or counterargument on why methadone maintenance should not be expanded. Donfeld was saying, unless we do this, you won't have any of it. It's a tool that we think will work – he was looking at Bob DuPont's program in D.C., which was predominantly methadone and which was effectively reducing crime. He believed we didn't have much data on the efficacy of the other things, so if we want to have impact, we ought to use what we have, and the guys at NIMH, OEO, and Justice want to cut it off.

NC: As I recall, you did not know at the time that committee met that there was a problem with heroin addiction among GIs.

JJ: No, not at all. That was a big surprise. The question of drug use in the military was not even on the horizon when we wrote the report. After the report was submitted, some time in February of 1971 I got a little note from President Nixon thanking me. I'm sure somebody wrote it for him, and he signed it or maybe the Autopen signed it. I never knew.

NC: So you went back to doing what you were doing.

JJ: I had never stopped doing what I was doing. Nobody had said, take some time off of the Illinois Drug Abuse Program, drop everything, and do this. IDAP was still a growing program that was very busy and expanding.

NC: When did you next hear from the White House?

JJ: I didn't hear from them again until April 1971, right after Congressmen Murphy and Steele began talking to the White House about drug use in Vietnam. Jeff Donfeld called me and said the White House would like to consult with me, and asked me to fly in. Donfeld was the point guy. Krogh had many, many responsibilities, and Ehrlichman had the whole domestic agenda. Krogh handled drugs and crime and a

few other things. Jeff was his assistant on drugs. So that was the pathway. Apparently they had been trying for some time to get information from the Military about drug use problems and kept getting reassurances that there was no problem. Then the reality of the situation became apparent.

NC: How soon after that call did you end up going to Vietnam?

JJ: It was a few months later. The first thing that happened was that they called me and said they'd like my views on what to do about drug use in the military. But again I was told not to tell anybody about it. There was a continuing obsession with secrecy. But, how do you come up with a solution when you can't get any consultation? I believed that if the White House said you can't talk about it, that you shouldn't talk about it. What I was fairly certain about was that when the congressmen said 15 to 20 percent of GIs were addicted, they didn't have a clear notion of what they really meant by "addicted." It could be that there were 15 to 20 percent that were addicted, or it could be that this was the usual hype. More likely, it could be that they had a lot of people using, some of whom were addicted.

There were a lot of questions and it wasn't clear what to do, but it was clear that there was a sense of urgency. For example, what happens when you take addicted people who are trained in military tactics, put them on a plane, and 18 hours later set them free in the community? Given the public's perceptions of heroin addicts as dangerous people, community experts and people in Congress were proposing civil commitment, confinement, and other draconian measures. People were talking about an epidemic, the idea that one addict makes ten addicts.

In my view, the first thing we needed to know was, how big is this problem? Given that under the code of military justice at that time a heroin user could be given a bad conduct or dishonorable discharge, I didn't think we could expect too much in the way of honest responses from the men if we just asked who was using. But one way to find out who's using is to do urine tests, and at the same time we could arrange to use that same urine test as a small deterrent. What I proposed was that they needed to have both the epidemiological data and some kind of deterrent that tells the heroin users, you probably should stop using drugs before you go home. It was a simple idea. I said the

thing to do was to test everybody before they left Vietnam. Those found to be using would have to stay a little longer for treatment. That would tell you how many people are using heroin to the point where they have trouble stopping and would also assure that no one who is physically dependent would be simply discharged in the U.S. Pretty soon the men will get the idea that you don't leave for home so readily if you're found to be using heroin. The longer stay in Vietnam would represent a deterrent to use. In contrast, if you find them using and immediately ship them home, you're going to have a high probability of continued use in the U.S. Some of the rationale for my proposing this arrangement was based on Wikler's ideas of conditioning, in which withdrawal symptoms become linked to the environment. If treatment for withdrawal was needed, it would be better to do it in Vietnam than in the U.S.

Jeff Donfeld and Bud Krogh asked me to present my idea to John Ehrlichman, and shortly afterward Jeff and I went over to talk to people at the Pentagon. I was soon arguing with generals who wanted the men found using heroin to be sent home as soon as possible. I said, you do not want to do this. What you want to do is say to the servicemen, if you're positive, you stay in Vietnam a little longer. The

generals were talking about putting the men found positive for heroin on a slow boat and detoxing them. I insisted that drug positives needed at least a two week detox in Vietnam.

At the time, there were about a thousand men leaving Vietnam every day. So the trick is, how do you test a thousand people in a day? It was not technologically possible with thin layer chromatography, which takes skilled interpreting. There's a certain art to getting it right. But I knew about a new drug testing technology, the free radical assay technique (FRAT) that had only recently been developed. I had a FRAT machine on order for the Illinois Drug Abuse Program. With that machine and a little bit of urine, you could get results in a minute. At the time it could only screen for opiates.

A few days before the visit to the Pentagon, knowing that the military might want to use the technology in Vietnam, I called Bill McGlashin, president of Syva Corporation, the company that made the machines, and asked how long it would take them to make another one. As head of IDAP, I may have been his first customer for this $25,000 machine. I said, I can't tell you why I'm asking, but could you take a risk and put some people to work double shifts on this? He agreed to

do that, and it looked as if we would have two machines. I would give up the one from Illinois; he would produce another one.

When Jeff Donfeld and I went to the Pentagon and presented my ideas they said, Well, this can't be done. Besides, we were thinking of doing it ourselves. It was really bizarre. It can't be done, but we were thinking of doing it ourselves and maybe we can do it in September. This was May the 30th, and a thousand people were leaving Vietnam every day. I felt like a dumb kid saying to all these generals, I can't believe that the greatest army in the world can't get its troops to piss in a bottle. The Secretary of the Army was sitting there. I said, look, I know you're busy with a war, and you've got other things to do, but if you get me a telephone, I'll call some civilians. If you'll give us some transportation, we'll set this thing up because, I said, I think the President really wants something done sooner. So they called a recess and went into another room. Then they came back and said, okay, June the 17th we'll have it up and running. I said, that sounds good to me. We talked some more. They didn't really trust the machine, so they arranged to set up a bank of gas chromatographs to verify all the tests that the FRAT method turned up as positive.

One problem that I didn't realize was that the electricity was not that good in Vietnam. You have to have steady currents to work gas chromatographs and the FRAT machine. You have to build facilities where you can detox people. It's not a trivial undertaking. In retrospect it was beyond belief that the military were able to do all of that in a matter of a little over two weeks.

It was quite an undertaking. They got Bill McGlashin and the FRAT machines and the guy who knew how to put them together to Vietnam, and they got the thing set up.

In the meantime, Krogh and Donfeld talked about how to announce what the White House was going to do, what would be their big initiative on drugs. They used a couple of ideas from our ad hoc committee's White Paper; for example, the notion of a coordinating mechanism for federal programs, and also somebody to have oversight for what's working, what's not working, and for planning. They came up with this idea of a Special Action Office in the White House to oversee the drug initiative. Krogh and Donfeld didn't tell me about most of what they were thinking. Krogh asked what I would be willing to do, whether I would be willing to help

somebody in Washington. I said, look, I don't want to be somebody's assistant here. They apparently took this to mean I didn't want to be vice president if I can't run the whole thing. But what I really meant was I just didn't want a job in Washington. I already had a job. I certainly didn't want to be second fiddle to some politician. So, without warning or agreement, I was introduced by the President as the person who would head his new program. Later, Krogh called it "appointment by ambush." That was Krogh's term. Actually, you may have seen that with respect to another appointee on "The West Wing."

NC: I adored "The West Wing."

JJ: When they want somebody for a job who seems reluctant they announce that he's accepted an appointment. I was in Washington and wasn't expecting to stay overnight. They said, you have to stay over, we need you here tomorrow. Somebody went out and bought me a shirt that was too big. I was sitting in the Cabinet room on the 16th or 17th of June. The President had invited in the congressional leadership, and they were briefed on the new drug initiative. President Nixon said, "And that man over there, Dr. Jaffe, is going to run it." Now, at my age at the time (37) you don't say,

"Mr. President, who the hell told you that?" It meant a cut in salary and moving my family. What do I do with my house in Chicago? They don't even pay for transportation and relocation at that level position in government. That's all on your own, not like the Public Health Service or even academia. This time I had to pay for my own move.

NC: How did you as a political person really feel about working for the Nixon administration?

JJ: It's very hard to say. Clearly there were lots of people who were virulently anti-Nixon. I was not among them, but I had not voted for him. But I was working for a Republican governor. Perhaps that's why Nixon thought I was acceptable, because Governor Ogilvie liked me. He liked me because I wasn't asking for huge amounts of money, and I was making the drug programs work for the state.

How did I feel about it? I guess I felt that you have only one President at a time. He's the President, and what he wants to do makes good sense. What I also felt was, this is the first time we've really had a bite at the apple – an opportunity to expand treatment and research, to pursue something other than strict law enforcement. Everything

he's doing is exactly what we wanted to do. We were really going to make treatment happen.

NC: How had you felt about the Vietnam war?

JJ: I felt it should have not have happened, but I didn't see that letting everybody come home addicted would necessarily improve things.

What I knew is that you had a lot of decent Americans carrying out what they were told was their duty. If I could be of help with this drug problem that some of them were having, that would be fine, but I couldn't control the other things. What I was also concerned about was this: the North Vietnamese are not stupid. If they thought that 25 percent of the Americans were on heroin, what does that do to their planning with respect to how successful they could be? What does that do to their plans for attacking Americans? How does that change the equation? If you think that everybody is stoned or drunk on the opposing side, you have a very different view of what you can do than if you think they're all ready and alert, and they have firepower and everything else they need. I thought that a widespread belief that a substantial proportion of troops were addicted to heroin

could put a lot of people at risk in ways that I couldn't really work out. I thought that if putting some of our program of detection and treatment into place we could get a better perspective on it and bring the problem under control, it would make the guys who were over there safer, simply because people would not have a perception that they were all stoned.

NC: Speaking of safety, I've always had this question about Vietnam. Was there an overdose death problem in Vietnam?

JJ: Yes, there were overdose deaths. There were data to that effect. That was one thing that alerted people like Krogh to the fact that the military wasn't giving them the whole story. The White House was more than a little annoyed that they had been continually reassured by the Military that it was all under control. Krogh had made several visits to Vietnam.

So I got drafted on the 17th of June, 1971. They escorted me out totally unprepared and wearing an oversize shirt to meet the press. I couldn't believe that they were shoving me out in front of the White House Press Corps with zero preparation about what I should say.

NC: You could have said anything. Did your wife, Faith, know at that point?

JJ: You can ask her. No, it came as a surprise to her. It came as a surprise to me. The people at the University of Chicago were furious that I hadn't given them a heads-up. But how could I have given them a heads-up? First of all, I didn't seek or want the job, and two, I didn't know it was coming.

NC: What happened with your relationship with Danny Freedman?

JJ: My relationship with Danny Freedman had been a bit strained and subsequent events strained them even more. As a department chairman he was under great pressure to bring in grants. He saw this big grant that I had, $800,000 to run this program and my other money from the state, and didn't see why I wasn't willing to share it freely. He would propose putting someone on the program payroll and I would ask what he'd do, and Danny would say, oh, there's no quid pro quo. But you just didn't put people in no-show jobs. This wasn't the New Jersey waterfront. But in all fairness

to Danny, as I became more deeply involved in the expansion of the clinical program I probably did some things that were not as supportive of the department as I should have been. Danny, the department of psychiatry, and the University of Chicago had all been absolutely critical to the establishment and success of IDAP. Danny helped me recruit and the University found space for offices and clinical facilities. And I believe he may have felt that he and the University did not get enough credit for their part in its success. He may also have felt annoyed, perhaps even betrayed, when I began exploring alternatives to living in Chicago. Frankly, I just couldn't tolerate the Chicago winters.

NC: Right. So this had been a historical issue between you and him.

JJ: Yes, and toward the time of my going to Washington it grew. I think the major thing had to do with issues of control of resources and how the department could benefit. That was his job. At the same time I had this tremendous conflict of being simultaneously the head of a grant at the university and the head of a state program. Then to top it off, I left abruptly, seemingly without notice. I resigned

from everything. The White House didn't say, come in a month, come in two months. The job began that day. I was even told on that first day that I was going to go to Vietnam. The plan to go was announced fairly quickly, but I didn't go until early July. The book, "Heroes and Heroin" by Av Weston describes much of what I just described.

NC: Yes, and there was also a television show.

JJ: Someone at the White House arranged to have these people from ABC record everything. Frankly, I wasn't paying much attention to what they were doing. I was pretty much overwhelmed by the responsibilities that had been shoved at me, which were innumerable.

NC: Who did you pick to go along with you?

JJ: I chose Beny Primm. I had known Beny for some time, and I knew he had been in the military. He was in the 82nd Airborne. So he knew something about the military and he knew about addiction, and I thought he could relate to the African American troops better than I could. We brought a few people with us from our programs in Chicago and New York. Matt Wright went along from IDAP.

It was a hectic trip. I got very sick on Air Vietnam as we were coming from Hong Kong into Vietnam. For two days I had a high fever and was in the hospital, so it's a good thing I brought other people because they could go out among the men and look at what was going on, then I looked at the data coming in.

Returning home, we stopped in San Clemente and reported to the President. I had the data on the number of confirmed positive urines, and I told him that it looked like the number of people who were using heroin to the point where they can't stop was a lot lower than we were led to believe. It was about 6 percent, not about 20 percent. I cautioned him that it varied from unit to unit and it depended on who was leaving that day, which was one of the reasons I told the press we'd give out the data only once a week. One day it could be 8 percent, one day 2 percent. The Air Corps personnel weren't using heroin to the same degree, and neither was the Navy. It was mainly a phenomenon among Army enlisted men.

NC: Ground troops. Was that solely because of availability?

JJ: My guess is that it was availability because drug use really didn't correlate with combat versus support away from combat. It was where you were and who was selling heroin.

NC: Not unlike the drug problem more generally.

JJ: Yes.

NC: After you met with the President on that, what did you begin to put into place?

JJ: I had to hire new people for the new office in Washington. However, we had no legislative base. The Special Action Office was a White House office created by Executive Order, so the whole budget came out of the White House budget. In order to get the legislative authority for this Office, we had to negotiate with Congress. So I had to pick somebody who could take on that task as general counsel.

NC: So that's how you got Paul Perito?

JJ: Yes. Paul had been general counsel to the House Select Committee on Crime, chaired by Claude Pepper. We had to think organizationally. How are we going to do all the tasks the President had assigned to the Office? How do we have oversight of all these agencies, and perhaps after a couple of years have fewer agencies with more clear-cut missions? How do we deal with prevention? How do we get treatment up and running where it is most needed? What do we do about methadone now that we've said it ought to be part of the treatment mix? Dealing with methadone was a major undertaking all by itself. There was low-level chaos because there were lots of people from other agencies who wanted to come to that office, yet I'd have to tell each one that this was only a two- to three-year office. Do you really want to give up that terrific job you have at OEO or HUD, because this job isn't going to be here in three years. I was really committed to the concept of a temporary office with extraordinary power over the agencies. And I believed that that kind of office shouldn't exist in perpetuity. So I warned everybody interested in joining us. Some believed me; some didn't.

Then we started testifying. Half our time was spent preparing for and giving testimony because drug abuse was a

hot topic. Everybody wanted to hear about it, and it was good television. Between getting ready for testimony and giving testimony we had to hire people and tell them what to do. Most of our new hires didn't really know much about the field, so we were training them and teaching them. There was a real conflict if I hired a trained person from a particular city or state or agency because then there would be nobody in that place to take the new federal money who would know how to make treatment programs grow.

NC: So you couldn't rob the agencies.

JJ: Well, it wasn't even the agencies. I couldn't even rob the periphery. There were so few people working in the field at the time.

NC: Did you feel you faced growing a whole field very, very quickly? This would have been especially true when it came to treatment. That era created a cadre that shaped the kind of treatment we have today.

JJ: Yes. Every time I was thinking of recruiting somebody who was really good, I said, but if I recruit that person, who would be left in that area to whom I could give the

money to run a good program? That was always a conflict. I found a couple of good people. John Kramer came from the University of California. He was very knowledgeable. I stole Lee Dogoloff from Bob DuPont, who had a fairly deep layer of people in his Washington program. Paul Perito brought in a couple of people who were knowledgeable about how things got done in Washington. Some were people who had worked in the Congress on narcotics issues and at least knew something about the field. We had to draft the bills we put before Congress, an art unto itself, and Paul recruited Grasty Crews, who was a superb craftsman of legislation.

Ultimately we had to deal with both expanding and controlling methadone. The methadone story is complicated. The problem was that we already knew of abuses of methadone. We knew that the FDA had no way of really enforcing the IND (Investigatory New Drug Application) once they gave it out. Some doctors who had gotten IND's were writing prescriptions for huge amounts of methadone. There were already for-profit clinics that were giving out 300 milligrams at a time. Just 100 milligrams is a lethal dose, so if somebody sells a 300 milligram bottle, there could be several overdose deaths.

There were some influential people in New York who vehemently opposed methadone, such as Dr. Michael Baden, deputy chief in the New York City medical examiner's office. Baden was married at the time to Judianne Densen-Gerber, a very prominent person in the therapeutic community movement who was vocally anti-methadone. Baden did something that has plagued the field for years. He defined a methadone-related death as one in which he detected methadone in somebody's body. If you were walking across the street in New York and were hit by a bus, and you'd been on methadone, you were a methadone-related death. Baden also had connections to Myron Farber, a reporter for The New York Times, who wrote more than one story claiming that methadone deaths exceeded heroin deaths. The whole drama of this rivalry between the pro-methadone and anti-methadone camps was played out in The New York Times' headlines. It wasn't always played out fairly.

But we recognized that if there were enough methadone overdose deaths as a result of diversion, the availability of methadone to treat the heroin addicts would be either curtailed or eliminated as a result of political action by its opponents. The U.S. was one of the few countries at that time where methadone maintenance was made available as

policy. The European countries had not done that. England had something of a system for doing so, but it was not particularly well organized or thought through. We had to craft a system that would allow us to say to people that methadone was useful, but only within certain parameters. This is a deviation from FDA procedure. When the FDA approves a drug, it is essentially telling doctors that the drug is available to be prescribed lawfully, and that there is no penalty for prescribing it even beyond the indications on the label. The prescribing of an approved drug is left to medical judgment. How do you tell a doctor, you don't have authority to prescribe this drug above a certain dose? Or, you can use it, but you can't decide how much a patient can take home. Or, you can use it, but only if you do certain tests at certain frequencies and provide other services with it.

NC: You run the risk of being accused of interfering with the practice of medicine.

JJ: There's no question about it. And you are justly accused. We did interfere with the practice of medicine in this country. It was precedent-breaking, in some degree. What we implemented was called a hybrid IND approval. I told

the FDA we needed to do this and that I would take personal responsibility if anything went wrong. Peter Hutt from the FDA actually crafted the concept and was absolutely brilliant.

NC: Did you have the authority to order the FDA to do that? What was your relationship with the FDA like?

JJ: Not really, but I was representing the White House. I told them I'd take full responsibility. You will not be the patsy. If somebody needs to fall on their sword, it'll be me. I didn't really want to be there anyway. I just wanted to go back to doing research and running a program.

NC: Have your own views on maintenance changed?

JJ: I'd been running a maintenance program for almost five years in Illinois. Have my views changed since then? The regulations that we put in place in 1972 were just that, regulations not legislation, because we felt that regulations could be more flexible and could change with the times. But they didn't. Every time the government wanted to change them and improve them, both the DEA and the service providers tried to protect their vested interests by

keeping everything the way it was. Nobody wanted to change. I tried mightily to effect some changes when I went back into government in the 1980's, and the remarkable thing was the degree to which the franchise holders, the methadone programs and the DEA wanted them just the way they were. It took more than ten years, from about 1986 to 1998, to change the methadone oversight system from one where virtually every detail of operations had to conform to a regulation, to a system of accreditation. Technically, you can be prosecuted if you break a government regulation, but you can't be prosecuted for not following a guideline under accreditation. You can lose your accreditation, but you won't be prosecuted. After I left the Special Action Office, the DEA got control over the methadone programs by separate legislation. They were not involved during my time there.

NC: What was your relationship like with the DEA?

JJ: I had good relationships with the law enforcement people. I knew Myles Ambrose and many others on the law enforcement side fairly well. In one area of their activity, we talked often about what their priority should be - methadone "leakage" or heroin? At the time they

realized that when somebody sells methadone, they're usually selling it to another addict who can't get into a program, and that this should not be where you put your resources. For the year or two I was there, that's exactly what happened. It wasn't and isn't a good idea for people to sell methadone. Bad things can happen if you sell it to somebody who's not tolerant. But there was a drama being played out in the newspapers about methadone deaths and methadone zombies. These stories helped the DEA persuade Congress to give it concurrent authority over methadone programs in 1974.

Also, there were some people who were previously getting money to run "rap groups" who felt threatened because they couldn't show that addicts coming to them for help were doing better. They bitterly criticized doctors who had the credentials to run methadone programs because the doctors were now getting the government money that used to go to them. Their attacks weren't out of pure principle; there were vested interests involved. Some people might have seen it in political terms, but you got the feeling that there were other motives, as well.

NC: Would you say that the entire issue of how to control and expand methadone was one of your primary activities at SAODAP?

JJ: No, it wasn't primary, but it was very time-consuming, and unfortunately accomplishing the establishment of a methadone treatment system seemed to have eclipsed recognition of all other achievements of those first two years. We could have spent full time on methadone for a while. We had to craft something, get it balanced. We had to look at what is it that we really expect to accomplish. We didn't want to have just doctors and clinics giving out methadone, at least at that time, because we believed that to be effective methadone needed to be combined with counseling and rehabilitation. But we recognized that if it should later turn out that counseling doesn't help as much as we had hoped it would, but only the methadone did, then the regulations could be changed to reflect that new information. But those changes never happened. We now have data that suggest that, at least in Baltimore, counseling at its current levels doesn't have much of an impact. It may very well be because programs have essentially been starved. Inflation has eroded the actual resources per patient treated with methadone. During the Carter years, for

example, inflation was 12, 13 percent. At that rate, if you simply hold the budget constant, in four years you will have cut the buying power of the programs in half.

NC: I'm sure you hear much more than I do about the lives of treatment providers, of counselors, being pressured.

JJ: They are pressured. They've become progressively more pressured, so you get people burning out. There was always some burnout. Drug addicts are tough people to work with. Pressure is a reflection of two attitudes. I don't think Jimmy Carter was supportive of methadone. Certainly, Ronald Reagan was not. He was anti-methadone. There were 12 years of eroding federal support. A lot of support came from the states, but some states are so anti-methadone that they don't pay for it at all. In those states you can't get methadone unless you can pay for it yourself. California, for example, has been almost all self-pay. That's one way to deal with something you don't like. You say, well, you can have it, but we don't subsidize it. It turns out that a lot of people can't afford to pay for treatment, or they feel so angry that they say, why should I take something that doesn't make me feel great like heroin does and pay for

it? If you want me to give up my heroin, you've got to give me methadone for free. Attitudes like that are prevalent.

But that has nothing to do with what happened 35 years ago, when the issue was taking the unusual step of actually regulating this one aspect of the practice of medicine. This generated anger from people already working with methadone, and it angered everybody not working with methadone because they didn't want it at all. We felt that if there was going to be enough time to demonstrate the general efficacy of methadone, it was necessary to regulate it. I don't know whether we were right or wrong or how it would have worked out differently. But at least the overdose deaths were kept to a minimum, diversion was kept to a reasonable level, and enough people found benefit in methadone treatment that the program continued.

NC: How fast did the scale-up happen? When you first came into the position, how many methadone programs were there?

JJ: I don't recall exactly, but I'm sure it quadrupled.

NC: What else were you doing in that first sort of flush of enthusiasm?

JJ: The list is long. First, the military problem didn't go away. Everybody wanted to hear about what was happening with the military: What are you doing about getting people into the VA for treatment? All of this was newsworthy and required attention. The VA had a special budget system: the central office gave money to the hospital directors and it was up to each of them to decide how best to use it, no matter how you tried to earmark it. The hospital directors had autonomy, so the money you intended for a drug program might not turn into a drug program.

NC: Was there no accountability?

JJ: There was in this case, because we sent our people out to visit each facility. If we found that they were taking drug program resources and not using them in the ways that were intended, we would go directly to the Administrator of the Veterans Administration and say, we need to have a talk. This was not a good thing to hear from a White House office in those days. President Nixon had announced publicly that he wanted me to "knock heads together." He really felt that the bureaucracy was – and it is – always trying to be more or less autonomous. It sort of resents anybody at the White

House telling it what to do. So SAODAP staff members went out and looked at what was happening, and if necessary we talked to those at higher levels.

Drug use in the military continued to be an issue for at least the first year. All the data and testimony are in the Congressional Record – what we found, and what was going on. I had many discussions about our programs with Senator Harold Hughes, who was a passionate advocate for treatment of people with alcohol and drug problems. As I mentioned earlier, drug use or possession led to a dishonorable or bad conduct discharge from the military, until June of 1971, when President Nixon asked for a change in the Code of Military Justice. So maybe a couple of thousand men had been given drug related bad conduct or dishonorable discharges. That meant they weren't eligible for treatment through the VA.SAODAP worked with Congress to get these bad conduct discharges changed. The penalty was put in place to deter drug use, but it wasn't particularly effective. Then there was the question of what to do about the ongoing drug use situation in Vietnam. What about the guys who just got there? Were they going to use heroin?

When I reported to the President in July of 1971, after coming back from Vietnam, he said, you've got to write a book about this Vietnam experience. I didn't exactly have time to write a book, but I arranged for Lee Robins to conduct a study and coerced several federal agencies to contribute to its funding. This led to her now classic follow-up studies of Army enlisted men returning from Vietnam. The Pentagon was reluctant to cooperate with this follow-up, perhaps because they were afraid of what they were going to find. We really had to pressure the military to give Lee Robins the records. I assigned David Nurco, one of SAODAP's consultants, to run interference for Lee to make sure that nobody closed the doors on her to keep her from getting the data. We also had to convince the VA and the Selective Service to cooperate because Robins and her coworkers wanted to pull a control sample from those databases.

NC: Had you worked with Lee Robins before?

JJ: No, I had not worked with her, but we may have been on some NIMH committees together. Danny Freedman told me about her and I had read her work.

But let me return for a moment to mention a few of the other tasks and accomplishments of SAODAP. Sometimes the emphasis on what SAODAP accomplished in establishing a framework for methadone and other opioid maintenance and expanding treatment completely overshadows its many other important achievements. Perhaps the most significant was the increase in support for basic and clinical research. I believe that in terms of percentage increase there has not been such growth in research support ever since those first 3 years. SAODAP also initiated the loosening of BNDD restrictions on providing Schedule I drugs to researchers. I've already mentioned Lee Robins' Vietnam work and the Career Teacher Program. We also supported a nationwide survey of drug use behavior by Jack O'Donnell, *Young Men and Drugs*, which deserves to be mentioned more frequently. SAODAP worked hard and successfully in crafting and passing the legislation that provided confidentiality for the medical records of people seeking treatment for drug and alcohol problems. And of importance for policy, we directed the development of epidemiological and surveillance activities (the National Household Survey, the Drug Abuse Warning Network [DAWN], and a brief method of estimating the number of patients in treatment) that are necessary to estimate the degree to which policies are reaching their stated goals. The

legislation we crafted that gave SAODAP its authorities included the creation of NIDA and the State block grant system. One program we created, Treatment Alternatives to Street Crime (TASC) was later renamed and morphed into the present day drug court program. We also wrote the first Federal Drug Strategy to at least mention that alcohol and tobacco should be seen as part of our national drug problem.

What made it possible to get so much done in that brief period was White House support, including a virtual carte blanche for recruiting employees or retaining consultants. There was no political litmus test. Also, the people who worked or consulted for SAODAP put in very long days and often seven-day work weeks. I was incredibly lucky to be able to get help with the research agenda from some already distinguished researchers and some who went on to develop illustrious careers in research or treatment. I need to mention at least a few, including Nancy Mello, Jack Mendelson, Roger Meyer, Alan Green, John Ball. I already mentioned Beny Primm, who continued to act as a consultant after the Vietnam trip. SAODAP also got great support from colleagues at CPDD, such as Leo Hollister who helped with the naltrexone development, and Sam Kaim and Walter Ling who helped with the first multi-site studies of LAAM. Jeff

Donfeld left his post at the White House Domestic Council to come to SAODAP and was invaluable in the development of the TASC project. Vincent Nowlis joined us to help with our education and prevention work. I need to point out that our interactions with NIMH were not all about back-stabbing. Without the help of Karst Besteman at NIMH, who had been my friend since our days at Lexington, much of the rapid treatment expansion that took place would not have been possible.

NC: What role did you play in the closure of Lexington?

JJ: I suppose it was my decision to close the hospital. The hospital and the ARC at Lexington were both under the Division of Narcotics and Drug Abuse within NIMH. Before 1966, people from any part of the country could come voluntarily to Lexington to be treated for addiction. After the federal civil commitment act (Narcotic Addict Rehabilitation Act – NARA) was passed, in 1966, NIMH decided not to admit any more volunteers to the hospital and to convert it into a research facility for the residential phase of the NARA program.

NC: Was that because of civil commitment?

JJ: I think so. But with respect to the hospital's continued activity, by 1971 I had the data that Wallace Mandell, of Johns Hopkins School of Public Health, had developed for NIMH on addicts civilly committed to Lexington. At the time, addicts were being interviewed on admission and judged by the Lexington staff as suitable or not suitable for treatment. There was a concerted effort to get only the most motivated people into the program. Those who weren't really motivated were rejected. Mandell did a follow-up a year later comparing a group that received treatment for six months at Lexington followed by six months of treatment in the community to a group that was rejected. The drug use rates were the same for both groups. That doesn't sound like a good investment if the least motivated are doing as well as those you took in.

It is important to appreciate the size of the entire drug abuse research budget, which was very small relative to the billion dollars a year they have now. A lot of what NIMH was calling drug abuse research was really a stretch. It wasn't that it wasn't good research - it just wasn't on drug abuse. Basically, there weren't a lot of people at NIMH interested in drug abuse. It was not a high prestige area

in psychiatry, where people were much more interested in depression and schizophrenia.

Based on the available data I was obliged to conclude that Lexington could be a terrific prison. It had a farm and a golf course. But in terms of outcome and cost it was not a good hospital. First, it wasn't getting results. Second, it was being called research. Prison space was needed and I saw a chance to move $18 million in resources to the extramural program without any significant loss of jobs in the community. That caused great consternation for a lot of people, but to me it made sense and it radically expanded the support for treatment.

NC: Was the ARC a factor in your decision?

JJ: No, the ARC was not part of that decision, just the hospital. The ARC actually got a very substantial increase in its budget. At the time they could still use drug addict prisoners as research subjects at the ARC. The ARC could still recruit, and they did. This was 1972, and they continued to send addicted federal prisoners there until 1976 when the use of prisoners in research was no longer permitted.

NC: Did you get any flak for that decision?

JJ: We got flak from people in Congress and people who didn't want to work in a prison. Working in a hospital is more prestigious. But remember, these hospitals were already housing federal prisoners. I got all kinds of flak from people in Texas because the decision affected the hospital at Fort Worth, too.

NC: Would you agree that federal civil commitment under the Narcotic Addict Rehabilitation Act (NARA) was really the undoing of Fort Worth and Lexington?

JJ: Yes and no. Even without civil commitment, once you had treatment in the community, which was often more effective and certainly more convenient, why would anyone drag themselves to Lexington or Fort Worth? Why go from California to Fort Worth, Texas? Why have two federal hospitals when there is treatment in the community? It was treatment in the community that made them obsolete. When the number of addicts was small, no city needed to have treatment. You could send them all to Lexington. Under such a situation it might have been acceptable to have one place.

We only had one leprosarium, in Louisiana. But drug addiction became a wider problem. Also, no matter how you look at it, civil commitment is an expensive process. You have lawyers, and you have judges. When people are banging on the doors of treatment programs trying to get in, why do you have to spend money compelling them into treatment? That was how I saw it at the time.

NC: Do you think it was a wrong decision now?

JJ: No, but who knows what the historians will think. I didn't want everybody to be unemployed by closing down those two hospitals and I thought I could work a deal with the Bureau of Prisons.

NC: So what did you do with the Bureau of Prisons?

JJ: They were willing to take it over rather than build a new prison.

NC: Now, did you go to Norm Carlson? Do you remember how that happened? That has been hard for us historians to figure out.

JJ: I don't remember the details of how we worked it out. Maybe you could ask Paul Perito or Jeff Donfeld. I could make a decision, but I had able people to work the details out.

NC: Did you also go to New York City to speak with Governor Rockefeller when you were still at SAODAP?

JJ: Yes, I was really trying to get him to rethink the draconian penalties that he was proposing under the Rockefeller laws. There were several problems with them. For example, drug dealing was to be punishable by life imprisonment with no possibility of parole. It seemed to me that rather than allow somebody to turn state's evidence against him a drug dealer who could be sentenced to prison for the rest of his life might be inclined to murder the witness. Even murderers are sometimes paroled. Beyond that, I didn't see that the penalties would necessarily accomplish what Governor Rockefeller wanted, but I was unsuccessful in my attempt to convince him of this.

NC: Why was it important, I mean, for someone in your role to have done that? You must have seen it as a pretty serious step that New York State was taking.

JJ: These things tend to have snowball effects. I thought that this would lead to a federal shift so that the federal government would not be softer on drugs than New York. There's a history to this. In the early 1950's, when Harry Anslinger convinced Ohio to pass some of the most draconian drug penalties in the country, it began a competition with each state trying to have the toughest drug laws.

I was also concerned about the total cost of dealing with a drug problem. I didn't believe then and I don't believe now that it makes logical sense to leave the cost of incarceration off the books when you consider the costs of particular drug policies. If you plan to criminalize certain forms of drug use or drug dealing, the cost of that policy option must include the cost of incarceration. When you have more and more minimum penalties and keep people longer and longer, you're raising those costs. Further, what's the evidence that if you increase sentences from 20 years to 40 years you're going to have more deterrence? You will have 20 more years of paying for incarceration. Back then we knew what that costs because we continued to look at the costs of one policy option versus another. We knew what treatment cost; we knew what it cost to control crops,

production, and illegal smuggling; and we knew what prosecution and incarceration cost as well. To me, longer incarceration was a redistribution of the resources available to implement a thoughtful national strategy.

I was hoping that policymakers would at least be honest and keep the cost of incarceration on the books so they could say: this is what the drug problem is costing us. Then you could look at the cost of imprisonment, and if you cut those costs down, you could put more into prevention and other things. At some point, the cost of incarceration was shown as part of the problem, and later policymakers decided to take it off the budget. I really don't know how they're treating it currently, whether or not all of the people that they have in the state and federal prison system on drug charges are actually counted as part of the cost of our drug policy.

NC: Could you also talk a little bit about SAODAP's education and prevention efforts?

JJ: There was a lot of pressure on us to put into schools all kinds of curricula to tell kids about drugs, explain and educate. All the data I had up to that time suggested that

this had no impact. Some of the available data suggested it made them more sophisticated and actually increased drug use. So I thought, the least I can do is not make things worse. Some of the public service ads looked like they were counterproductive. So SAODAP decided we should have a moratorium until we could find out what's effective. My fundamental perspective was that we needed to know what works. A message about drug use is not necessarily useful or effective, no matter how clever it may seem to the people who make the public service ads or choose the school curricula.

So we did not do a lot in the area of direct K-12 prevention or public service ads, but did initiate research to try to start the process of asking, Do they work? What works? One thing we did was to more than just say that medical schools ought to be teaching doctors something about addiction. We put into place the Career Teachers initiative and directed NIMH and NIAAA to fund it. The issue wasn't so much one of trying to keep people from using drugs, but to teach physicians how to deal with people who already had been using drugs.

NC: Did SAODAP actually fund any research?

JJ: We did fund research. We had our own budget, but it was quite small. We funded some research on naltrexone, and LAAM. We paid part of the cost of the Vietnam follow-up study. We leveraged our own money with funds from other sources, as well. We suggested to other agencies such as NIMH, VA, DoD, and OEO what we thought should be research priorities.

NC: Did the tensions between SAODAP and NIMH ever resolve?

JJ: No. There was never a resolution because every agency resents anybody who tries to tell the head of that agency what to do. A major area of tension was probably that the head of NIMH wanted to put drug abuse treatment money into the community mental health centers. That was their favorite vehicle. I happened to believe, and I must say I had personally witnessed, that when you gave the community mental health centers the money they did not treat addicts. They treated the people that they liked to treat. There was no quid pro quo. Thank you for the money, but we'll treat who we want to treat. And they frequently didn't have the necessary expertise. NIMH was still dominated by people

with a more or less dynamic view of psychiatry and people with similar views ran the community mental health centers. They favored sociologic and analytic approaches and were not using the more cognitive approaches that are now proven to be what works. I felt that if we put the money there, it would get dissipated, and that's where NIMH wanted to put the money. We wanted the money for treating addicts directed toward programs that would deal with addicts.

NC: Was there a basic versus clinical split within SAODAP?

JJ: Not within SAODAP. We would have liked to see a little bit more work on outcomes so that we could look at fine-tuning what was being funded on the treatment side. Some people's funding went more into the basic side. But at the end of the day, it wasn't enough to really fight about. That wasn't the big fight.

NIMH had their own ideas on who was going to do their outcomes research. I had participated in some of it when I was in Illinois. They had a large program that was gathering data, and they already had an N of 55,000, but they wanted to keep funding it. I thought that if you had 55,000 people, you should have enough data to talk about outcomes.

There's a kind of symbiosis, or a kind of illicit liaison, between project directors and the people they fund. These relationships tend to cloud things. There was a little tension over that.

When I was still in Illinois, NIMH was giving us eight-page forms to fill out on everybody we admitted. I didn't really have much confidence in how well the forms were being filled out in the clinics, since we never got any feedback from it. It couldn't be done centrally. It had to be done where you interviewed the people, and it's very difficult to get people to fill out all those little blocks and ask all those questions. The patients don't want to answer them. And it goes away, you don't use it clinically, and it's just an extra burden on the front line clinicians with no feedback. I said it's time we either get some results or fund something else.

There were more than enough areas for tension. I'm sure that they weren't happy with the decision I made about their clinical research facility at Lexington. It displaced some of the doctors there, some of whom are colleagues of mine. They weren't happy with not putting all their money into the community mental health centers. They weren't happy with

methadone, by the way. They would say, oh, the White House is all about a magic bullet. That was a distortion. We never thought methadone was a magic bullet, but we thought it certainly had some utility. There might have been bruised feelings just from overriding their views. It's clear that NIMH had a more sociologic view, that nothing could be done until you right all the wrongs in society, until there's equality and justice and no discrimination. That's both utopian and nihilistic. We thought you could do some things, even though it was not within our power to bring light and justice and fairness to all people. There was more agreement on the need for more basic research.

NC: Is it fair to characterize you as having a more pragmatic approach that could be put into practice, whereas they had a less practical orientation?

JJ: Perhaps. They were somewhat utopian. Maybe they were right. Maybe in a world where everybody is happy and lives in the suburbs you'd never have any heroin addiction.

NC: Or methamphetamine.

JJ: Or methamphetamine. Or cannabis. It was a view that I respectfully declined to accept in toto. I said, there are a few things we probably can do.

NC: In the time that you were there you did accomplish a few things. Had you intended to stay until the sunset?

JJ: No. I had taken a two year leave of absence from the University of Chicago that was up in July of 1973. My mind was made up for me after I had made myself persona non grata in March of 1973. When President Nixon decided that he would have mandatory minimum prison sentences (because, as I had feared, he didn't want to be outflanked by Nelson Rockefeller), I wrote a memo that said I didn't agree with that decision and this was leaked to the press. That was not well received at the White House. I submitted my resignation, although I did not leave until June 1973.

NC: Had you decided beforehand to use the occasion to make clear your views on mandatory minimums?

JJ: No. I wrote what I naively thought was an internal memo and it was leaked to the press. I hadn't decided to leave, although I was ambivalent about staying because

another Goodman and Gilman edition was coming up. I knew I couldn't do that from the White House.

NC: Were you that committed to Goodman and Gilman that you really wanted to do that?

JJ: After '65 and '70, the third and fourth editions, it wasn't a matter of being that committed, but involved.

NC: Did your White House experience change the Goodman and Gilman chapter in any way?

JJ: I think the experience changes the person and the degree to which you can concentrate on academic writing. Compared to the fourth (1970) edition, which I wrote when I was in Illinois and still active in academics, I felt less prepared. Two years of looking at Vietnam, regulations, and testifying before Congressional committees, and making policy decisions is not keeping up with the literature. The literature had not yet felt the impact of the investment we made in research and it was not yet growing logarithmically, so it wasn't impossible to deal with it, but it was difficult.

NC: So the sudden knowledge explosion in the field hadn't yet really trickled into the literature?

JJ: All the grants were made in roughly '71, '72, and '73 so very little was in the literature by '74. There was one exception. The work on the discovery of the opiate receptors was published in 1973. In one of his books Sol Snyder said that his interest in working on the opiates was a result of the increased funding for research that SAODAP made possible.

There were still only a handful of specialty journals, so in that sense it was still doable. I don't know how people do it now. There are just too many specialty journals and too many things coming out. I just don't know how people writing textbooks are able to decide what's important anymore.

NC: When did you bring Bill Martin into the Goodman and Gilman chapters?

JJ: I think it was '75. The reason was that Bill Martin was really the first person to talk about receptors. They hadn't been discovered, but he predicted them. From '71 to

‘73 they began to really talk about the discovery of the opiate receptors. Opiate pharmacology became more molecular, and Bill was working full-time in this area. I felt I’d lost a step or two as a result of taking on administrative responsibilities. I believed I needed him for the opiate chapter, but I still wrote the drug abuse chapter myself.

You make some mistakes in life, but taking on those chapters was not one of them. Maybe I left something out, but you can’t put in everything given the limitations of space. For me they were a lot of work. Maybe I was too obsessive about trying to decide whether to put in this or that fact.

Anyway, that was one issue. The other issue was that I was tenured at the University of Chicago and had the option of going back, but not if I dragged it out indefinitely. So I was not particularly unhappy about leaving the White House, although I did not go back to Chicago, and I would have liked to have left the White House under better circumstances. I thought the White House reacted to the memo in a heavy handed way. I didn’t leak my disagreement about mandatory minimum sentencing to the papers. Somebody else did. So be it.

NC: Was it, in retrospect, the right issue to have done that with?

JJ: There were people in SAODAP, like Grasty Crews and others, who believed that mandatory minimums were just the wrong thing; that you've got to take a stand. I respected these people a lot. True, they were more concerned with civil liberties than I was, but civil rights and justice are important. If you're asking me if I had known that one of my staff would leak the memo in a way that would cause me to be in great difficulty, would I still have written it? I'd have to think about that.

But I will tell you this. Once the crisis was over, the crisis being Vietnam, heroin deaths, expanding treatment and research, establishing a central focus for oversight and for policy, things started to settle down, it became clear that we had had our one bite at the apple, and the White House was going back to an emphasis on law enforcement. So if your star is declining, it's time to go. I don't know whether or not it had to do with my shortcomings as a bureaucrat. I tried never to think of myself as being the Drug Czar. My father used to tell a story about a suit, the moral of which is that you should never confuse yourself with the role that

people give you to play. I didn't change when they gave me this job. I just had to wear the suit, and when I had to take the suit off I didn't feel I had changed much. I never felt comfortable in all of the trappings that went with a White House position. It just wasn't me. I felt much more comfortable when I was in Illinois. I just didn't like moving in those high profile circles. There are people who feel comfortable walking in and out of the White House, but I never really felt that this was anything more than a temporary task that I had to get done.

NC: Also in terms of Nixonian drug policy, I'd like to ask you about the development of the Controlled Substances Act.

JJ: Yes, I was part of that, but from my position at the University of Chicago. In that instance I was a great antagonist of the Justice Department because what they wanted was the absolute power to determine where drugs went on the proposed new Schedules. They also wanted the power to engage in education and dissemination. These were not activities that I had any confidence that they would undertake with any kind of scientific integrity. They had a history of exaggerating adverse effects and even making things up. There's a certain spin you can put on data, but

at a certain point you're not spinning it, you're lying. When Justice said they were going to do these tasks that were traditionally the responsibility of the Department of Health, Education & Welfare, I said publicly that the legislation as drafted was unacceptable. I might have been more articulate back in those days. Eventually I think they made some kind of modifications.

NC: Were you concerned about who would have decision-making authority under the CSA?

JJ: Absolutely. I liked the idea that there would be several levels of risk associated with different classes of drugs instead of the Manichaean outlook where a drug was either bad or good. It was a step forward to recognize that there are some drugs that are very risky, and there are some that are minimally dangerous. That would make for sensible policy; but I really felt that the decision needed to be made in a rational, coordinated way with health data as well as criminal justice data, and with people looking honestly at those data. The World Health Organization Expert Committee on Drug Dependence usually did that. It was necessary to look at all of the issues, both the consequences of classification as well as some of the

benefits of shifting classification, and I didn't trust a process that was left entirely to law enforcement.

NC: Were you at all concerned about the potential impact of the CSA on research?

JJ: At the time? No. But I suppose I should have been. If you put everything in Schedule I, you can't get access to it unless the DEA says you can have some. And they had not been very cooperative for a long time.

NC: Didn't they have to be very cooperative with some researchers, like Maurice Seevers at University of Michigan?

JJ: I suppose so. I don't know what price those researchers had to pay to be in the good graces of the BNDD. But then, I wasn't concerned about that kind of research. I don't recall being concerned that we wouldn't be able to develop new drugs.

NC: I'm also curious about whether you thought at the time about whether the CSA would have adverse effects on pain management and on how doctors dealt with chronic pain.

JJ: I don't think so. The new CSA would not really do anything different with the opiates. They were going to be where they were, in Schedule II, and that was appropriate. I didn't think that was a big issue. A lot has to do with how these laws are administered, not the way the laws and regulations are written, but how they are actually interpreted and administered.

NC: You wrote an article in 1985 about scheduling. Do you remember that article? It contains a poignant vignette about your father's death.

JJ: Well, my father and my father-in-law. The same thing happened to both of them in terms of under-medication for pain.

NC: So you must have had some concerns.

JJ: Yes, I was concerned about under-treatment of pain. I mentioned it in the G&G chapter in 1965. But I wasn't concerned about the CSA. The issue was the attitude of doctors. You had to try to understand where that attitude comes from. Clearly if you have local law enforcement people who are intimidating doctors who treat pain, then nothing

you do about the CSA is going to change that. Morphine is a drug that causes addiction and also relieves pain. How do we balance those aspects of the drug? It's still an issue and it still has to be dealt with, and it is not an easy thing to do. In every profession, whether it's the DEA agent or the local guy on the state medical board, or the doctor, there are mavericks, and there are bad actors. Some doctors are dumb, some are debilitated, some are dishonest, but they all still can write a prescription for morphine, and they'll still probably be paid for doing so. There are all kinds of people who can over-prescribe, at least potentially, and the response to each of them is a little different. If you have a doctor who's way over the hill and all he's doing is writing prescriptions for opiates, I guess you have to take away his license or get him to retire. But, should you put him in prison? The British at one time said that the way you get the attention of the Navy is from time to time to hang an admiral in the public square. We do that with white-collar crime. Harry Anslinger's technique was to advertise the prosecution of a physician and thereby intimidate all physicians who were exceeding what he thought was appropriate medical care. You need to have some lines you don't want physicians to cross. On the other hand, you

don’t want them so intimidated that patients are really in pain unnecessarily.

My concern with the CSA had to do with other areas of responsibility that the drafters of that bill wanted to assign to the Attorney General, which had more to do with research and education and seemed to me to more properly belonging to HEW. Eventually they crafted a compromise with the help of the Senate. There was a lot of lobbying.

NC: Were you part of the lobbying effort?

JJ: I was invited to meetings sponsored by the Justice Department where they presented their views and I told them my views. I don’t know why I was invited, (it must have been early in 1970, or even in ’69). Maybe it was because I was working in the area; maybe because of the chapters I wrote for Goodman and Gilman, I don’t know.

NC: After you left SAODAP and went to Columbia, what research paths did you embark on there?

JJ: I wanted to get more involved with smoking. I was consciously doing penance for focusing so much on heroin,

which was a very visible problem that affected a very small population. Tobacco had been largely ignored, and it affected millions. To the distress of many in the Washington world of politics, I mentioned tobacco use as well as alcohol use in the first Federal Drug Strategy, which I largely wrote. I didn't think enough attention had been given to the addictiveness of tobacco. Nicotine addiction wasn't a diagnosis, and many people, even in the medical/psychiatry community, argued that tobacco use was not, under any circumstances, an addictive disorder.

At Columbia I immediately got involved in tobacco-related research, although I couldn't drum up much enthusiasm for it in the department. I was first trying to understand how effective smoking cessation treatment was, trying to learn more about how to treat people who want to stop smoking.

Also, a colleague and I did some interesting abuse potential studies on loperamide, a drug that everybody uses now.

NC: Yes, it's got a little bit of an opiate in it?

JJ: It is an opiate. Except it's a peripheral opiate. It doesn't get absorbed, and it's totally insoluble. So it's

not a drug that has very much abuse potential. The only thing it does is give you constipation. Loperamide started off as a Schedule V drug when it was first marketed, and then it was dropped from the Schedule entirely.

But my research focus was on tobacco and I published several studies, including one in 1975 on Smokenders treatment program. During that period, I convinced Bill Pollin, who was then Director of NIDA, that tobacco dependence existed and was important enough to be a concern for NIDA. Bill initiated some NIDA conferences, and I wrote and delivered several papers on tobacco use as an addiction.

NC: How did you get the project to develop diagnostic criteria for tobacco dependence?

JJ: Bob Spitzer and I were on the committee that developed the criteria for drug dependence disorders, and I convinced him that tobacco had to be included among the drug dependencies. We included tobacco addiction in the DSM-III draft that was released in ’78. There was a great deal of concern about whether we were creating a new psychiatric disorder, tobacco dependence. Critics argued: Doesn’t that

say 50 million people have a psychiatric disorder that they didn't have yesterday?

NC: How did you reply to that question?

JJ: Awkwardly at first. I wrote a paper with Murray Jarvik titled, "Tobacco Use and Tobacco Use Disorder," in which we said that you can use tobacco but not be considered dependent on it -- unless you want treatment or want to stop and can't, and then you have the disorder. Our view was that dependence exists on a continuum. The question was, Do people who are regular smokers have Tobacco Use Disorder if they say they could stop any time, but they don't want to stop? I wrote a couple of papers on that. Eventually it sorted itself out, not in an entirely satisfactory way, if you ask me, but I felt some satisfaction that at least we got tobacco use disorder into DSM-III for the first time. It was a diagnosis. We defended it against many attacks, particularly from people and institutions that had major funding from the tobacco industry. Also, smokers just didn't like the idea of being called drug-dependent.

I was less successful in persuading the American Cancer Society (I was a member of their research advisory council) to view smoking as an addictive disorder.

I also got myself into a research dead end, a major mistake that I didn't think through enough. I allowed myself to be misled by the tobacco companies' advertising of low tar cigarettes, and by a paper in *Science*, by Gori and Lynch. That paper postulated that there is a dose-response relationship for smoking-related toxicity (which is true), and if the average cigarette contains 15 milligrams of tar and you reduce the tar to one milligram, you may drop below the toxicity threshold. What's the implication of that? Gori and Lynch dealt with the dose-response relationship and lung cancer. They concluded that since there was no statistically significant increase in incidence of cancer among those who smoked two or three "full strength" cigarettes delivering a total of 45 milligrams of tar a day, if they smoked 15 cigarettes that contained one milligram of tar, it would be below threshold. Leaving the logic of this argument aside, it seemed to me that smokers would want to switch over once low tar cigarettes became available. But they were not doing so, and I wondered why. As it turned out, I was naïve about exactly how tar was measured. The

data published by the FTC and advertised by the tobacco companies were not measures of what a smoker takes in. They were measuring what a smoking machine takes in. I knew that, but I didn't fully appreciate the significance of the parameters used by the FTC smoking machine. I thought the question was: Why isn't everybody switching over? Now that there are light, very light, and even ultra light cigarettes, what does it take to get people to switch over?

I was able to win a NIDA grant to explore the question, and I started a study using economic incentives. I asked, if I paid smokers to switch would they do it? We got people to switch, but only later did we realize the full extent of the capacity of smokers to compensate in various ways for the lower tar and nicotine levels. This was the reality that I learned about too late: the tobacco in the regular, light, and ultra-light cigarettes was the same. What the manufacturers were doing was making little holes in the filters so when the cigarette machine smoked, the tobacco smoke would be diluted with air. The tar and the nicotine levels as measured on the machine would go down. The cigarette machine always smoked in the exact same way: a two-second puff and an interval of maybe 30 seconds. Everything was always the same. But a person doesn't do

that. Smokers can immediately sense the amount of nicotine they're taking in and take deeper puffs. Although we recognized that they could take deeper puffs and/or could smoke more cigarettes a day, we didn't see them smoking more cigarettes a day when they switched. What we didn't realize is how things change when you take a deeper puff, let's say 40-seconds instead of 30-seconds, or what happens when you hold the smoke in your lungs just a few seconds longer – a longer dwell time. In other words, all you have to do is just hold it a little bit longer and you can double the amount of toxins you absorb.

Later, when we analyzed blood nicotine levels from those smokers we induced to switch, we found that the nicotine levels did decrease when using ultra light cigarettes, but not to the degree suggested by the differences between one milligram of nicotine in a regular cigarette and a tenth of a milligram of nicotine advertised for the ultra light. The very large difference does not occur because of the change in behavior of the smoker. So, we spent a lot of time trying to get people to switch to low tar low nicotine cigarettes. Actually, in our study, which we never got around to publishing for a variety of reasons, we did see smokers cut down and their blood nicotine levels did drop.

Another thing that began to emerge was that the dose-response curves are not the same for lung cancer as they are for cardiovascular disease. Even a cigarette or two can give you heart disease, even if it doesn't give you lung cancer. The idea that you can cut down and smoke safely was not a great idea. I was, in a sense, misled by the tobacco companies. They never really said that low-tar cigarettes would cut cancer risk, but clearly that was the implication. It took a while to discover the tobacco was the same. That was a closely held secret of the companies.

Other work that I did during the time at Columbia included a study of the effect of baclofen on opiate withdrawal, which was published in a NIDA Monograph. My collaborator was Ronald Brady, who headed a methadone program. We persuaded patients to skip their morning dose of methadone and then observed them over the next 24 hours. For that study, I developed and used a new subjective effects check list that was significantly more sensitive than the Himmelsbach Scale that was still being used at the ARC. I later used the new scale when I was at the ARC in Baltimore and designed the pivotal studies of chronic buprenorphine.

Looking back on it, the change from Washington back into an academic setting was harder than I expected. My colleagues at Columbia did not think the smoking research I was interested in pursuing was particularly worthwhile. I should probably have been more aggressive in seeking grants when I got there, but it was awkward because the grant makers were people I had supervised, albeit indirectly, and there was some ambiguity about how much time needed to pass before I could seek funding from a federal agency. I spent a lot of the first year reviewing the literature for updating and revising the opiate and drug addiction chapters for the next edition of Goodman & Gilman, and then producing those revisions. Getting back into clinical work was also somewhat difficult. The heroin treatment turf was already staked out in New York, so going back to running a program like I had in Chicago wasn't an option. However, I did begin to see some private patients and had a special interest in addicted physicians. Altogether, I was busy but not very happy during the 5 or 6 years I spent at Columbia, and when a new chairman came in I took advantage of an opportunity offered to me by Roger Meyer to leave.

Roger had become chairman of psychiatry at the University of Connecticut. He had a new alcohol center and colleagues

there who were interested in tobacco as well. Roger was a good friend, and that personal/professional relationship was very important to me at the time. I went to Connecticut where I worked mainly on alcohol and continued some work on smoking, this time collaborating with Ovid Pomerleau. And again there was the matter of revising the G&G chapters. Overall, the years at the University of Connecticut were interesting and the relationships were good, but Faith never wanted to move there because she felt the kids had moved more than enough and she didn't want them to go through it again before they finished high school. It was a strain going back and forth between Westchester County in New York, where we were living, and Farmington, Connecticut, but I did it for several years.

Then Bill Pollin, who was director of NIDA at the time, asked me if I would be interested in applying to head the ARC. I said no at first for two reasons. First, my research interests had shifted more to treatment than to the kinds of work typically done at the ARC; second, it would mean taking a substantial salary cut to go back to a government position. But Bill was persuasive, and the ARC had a certain appeal for me since that was where I had wanted to go so many years earlier when I finished medical school. So I

applied and was appointed Director of the Addiction Research Center.

NC: By then had the ARC changed?

JJ: It had changed a lot and it was in the process of reconstituting itself in Baltimore, in a renovated building on what is now the Johns Hopkins Bayview campus. It had made the transition from Lexington a few years earlier. There was some abuse potential research with volunteers going on, but it was not, in my view, the kind of research than an intramural program ought to do.

NC: Would it be fair to characterize the abuse potential stuff as somewhat routine by then?

JJ: That's probably fair in part. I had demonstrated at Columbia and at the University of Connecticut that abuse potential studies could be conducted with volunteers, did not require an intramural program, and could be supported by the companies interested in a particular drug. I thought the ARC's intramural research ought to be more cutting-edge. It's not that abuse potential studies didn't have value, but I didn't think they should continue to be the major focus.

I consulted with colleagues, like Avram Goldstein, who said that the ARC should be pursuing molecular and genetic studies. These were areas I didn't know much about, but that's where the cutting edge was in 1984. We had known about receptors for almost 14 years. People were talking about intracellular and molecular changes produced when receptors were occupied. There was a whole new world emerging in terms of understanding drug actions.

The problem as I saw it was, how do you take an essentially a stable budget, which is very unusual in the research field, and make sure you deserve it? Why shouldn't you be competing for resources like the extramural people do? I thought there ought to be a connection between what they were doing in the lab and what they were doing in the intramural clinical program. If you had an outpatient treatment program, why not do some definitive studies of treatment? The ARC was just moving into a clinical research facility again in '84.

Another issue was one of how do you recruit people? How do you make it attractive to people? At the time, the salaries were not as good as the salaries in academia. Certainly the constraints were greater. Frankly, I might not have been

the best person to lead the transition. I didn't have the kinds of research technologies that would allow me to personally set up a molecular lab or an imaging lab or a lab looking at receptors. I knew that that was important work and I thought it ought to be done in a way that linked it with the clinical research.

NC: did you try to formalize treatment research at the ARC?

JJ: At the time they weren't doing real treatment outcome research. They weren't taking the products of what they were doing and testing them to see if they would have relevance to the field. They were letting other people do that. Lexington never did any treatment outcome stuff because they couldn't. The ARC was strictly intramural. Since the new ARC in Baltimore was recruiting people from the community to participate in research there was an opportunity to see how well these new agents did in treating addicts in the community.

NC: Were they receptive to that idea?

JJ: It was not easy. You had to find people who were interested. You had to recruit people. All in all, we

recruited a couple of very, very able people in Mike Kuhar and George Uhl. Mike brought some people with him, Errol De Souza and others, who have gone on to do very well developing new products in the pharmaceutical industry. Edythe London had already been recruited by Don Jasinski and was doing great work with brain imaging. I got a little bit involved in talking to her about research design in ways that might have been useful to her. Also, we were looking at the function of the sigma receptor.

NC: Did you ever discuss the ethical issues of using animals in research?

JJ: I don't think it had to be discussed. We were all very well aware of people's attitudes toward using animals. This was when the plight of the "Silver Spring monkeys" was much talked about, and that was good research. Joe Brady was fighting that fight. Well, at the ARC we had monkeys self-administering addicting drugs. I thought that particular research was worth defending, but some of the animal research did have to shift a bit.

Within less than a year of my coming to the ARC Bill Pollin resigned and there was a change in leadership at NIDA. Ian

Macdonald, who headed ADAMHA, asked me to serve as acting director of NIDA, so then I had two jobs again. I was driving back and forth between Baltimore and the Parklawn Building, trying to run both NIDA and the ARC. The traffic wasn't as bad in those days, but it was exhausting and I was relieved when Bob Schuster was recruited to head NIDA and took over that job.

NC: Did you do your acting director stint out of a sense of obligation? Didn't you see it as an opportunity to change the research priorities of NIDA?

JJ: No, I really did not. I did not want that visibility again. I did not want to be testifying before Congress again. That's a place where to misspeak allows you to be misinterpreted. The media loved to ask "gotcha" questions. If the administration did not like what an agency head said, the worst thing they could do to that individual was ask them to resign. But they could also punish the agency and cut the budget. So you had an obligation to everybody who was getting a grant to try to make sure that your agency was not seen as one that wasn't being supportive of the administration.

Also, it is a strange role to be involved with drugs in this country, given the different views of what should be done. A question such as, "What do you think of marijuana?", can posed in such a way that you can't possibly answer it without creating a headline for the reporter.

NC: Let's go back to the ARC for just a moment. You talked a bit about animal ethical issues. Were there also clinical ethical issues that were problematic for you? Were there clinical studies being done that you didn't want to be done in the way that they were being done?

JJ: Yes, there were some. It wasn't that the studies weren't ethical, but that they were not being done as meticulously as they should have been. Occasionally, a researcher would propose giving doses of drugs that I thought were too risky. Even if they were approved by our IRB I still had responsibility. So, there were always questions - is this safe, is this worth doing, should we be doing this. There were still people around who said you shouldn't give drugs to addicts. If we'd accepted that, then we would have had to close down all the clinical research, because essentially that was what was being done.

There were also some studies where we didn't give drugs to addicts. For example, we looked at cocaine withdrawal. We also did a time-consuming buprenorphine study that was pivotal to making buprenorphine an approved drug. We did that in spite of the reluctance of the drug company concerned, which thought that if buprenorphine became identified with the treatment of addiction, it would cut into its sales as an analgesic. But they finally gave us the material, and we did the study. We were working at that time with Charles O'Keeffe to get supplies of buprenorphine. After much cajoling, they gave us what we needed, and we did the study. That study, with Ed (R.E.) Johnson as senior author, became one of the pivotal studies that led to the approval of buprenorphine for the treatment of opiate addiction.

NC: How long were you the director of the ARC?

JJ: I came in 1984 and left in 1990.

NC: How did you come to go to the Office of Treatment Improvement (OTI)?

JJ: As director of NIDA it was Bob Schuster's prerogative to appoint a new director of the ARC, which he did. I went to NIDA for a few months and then to OTI, a new office that was created and which Beny Primm headed. OTI eventually morphed into CSAT. I stayed there until I retired from government service in 1997 and returned to teaching and consulting.

NC: At OTI and CSAT, what kinds of things were you doing? Did you get to do the kind of research that was about what works best for whom, the kind of research to which you were committed?

JJ: There was actually a conflict between NIDA and CSAT about research. The way it resolved, much to my distress, is that CSAT wasn't allowed to fund research that had a control group.

NC: That's how they put it?

JJ: Yes. You can't do research. You can give out money, but you can't have a control group. You can't do anything that has a control. NIDA does the research. CSAT just gives out money for demonstrations. In one sense you had to

say, what good is a demonstration if you don't know whether it was effective? How do you know if it's effective if you don't have a control group?

NC: Yes. I see that time as a time when the research side separated from the treatment side, and treatment became more about service delivery than research. Is that accurate?

JJ: That's correct. Within a year or so after OTI was formed, ADAMHA (Alcohol, Drug Abuse and Mental Health Administration) split up. NIDA became part of NIH. OTI became part of SAMHSA. That was the big fight, but I wasn't the fighter. In some instances we managed to fund some service studies that did contribute to knowledge.

NC: How did you do it?

JJ: You could have short treatment or long treatment. You could show differences between two weeks and eight weeks. We designed and funded the first studies on marijuana treatment. For the most part the investigator initiated study (RO1) is a terrific mechanism, but if no researcher applies to study a particular treatment and the government still sees a need for it you have to use alternative

mechanisms. So at CSAT we initiated some contracts to study marijuana dependence.

NC: Could you explain what an RO1 is?

JJ: An RO1 is a grant made when an investigator initiates the research with a specific research proposal. The bureaucracy doesn't initiate the idea or put out a request for proposal, or say we'll write a cooperative agreement or contract if you'll agree to do this research. For example, there was a time where nobody was doing research on methamphetamine, but CSAT had data on methamphetamine use and people seeking treatment. Since the lag time between an investigator seeking an RO1 grant and starting the research can be as long as a couple of years, CSAT funded Walter Ling and Rick Rawson in California to do the first methamphetamine treatment research. And we funded the cannabis treatment research, with Tom Babor as the coordinator, for the same reason.

Scientific considerations were not always primary in giving out demonstration grants. Getting the money out to different areas and constituencies had to be considered, too. To the extent we could and within the limits of what

could be done, I think OTI / CSAT did a few things that left a mark. Beny had already started the $50 million Target Cities program by the time I got to OTI, but we tried to shape it in such a way that maybe we would learn something. I don't think we learned as much as we hoped to, but we funded a lot of treatment service. In the process of funding service we were also trying to fund the technological infrastructure for service. We were trying to get people at the city level who accepted the money to understand that they ought to know how many people are getting treatment in their community and there ought to be somebody looking over all their programs. For example, Baltimore had more than 30 programs, but no one knew what they were doing. Are they duplicating each other? Are there major gaps? Is all the capacity fully utilized? We tried to frame the Target Cities in a way that would generate an ethos of treatment improvement. Given the idea of the Office of Treatment Improvement, the way you improve is to see who has the magic, who gets people better. You can't always do random assignment studies, but if you look at outcomes you can say that certain programs or methods get a much better outcome for the same kinds of patients. Then if you can identify what you think is the active ingredient in doing better, it's your job to try to communicate that to the people who

aren't doing so well. If some people who aren't doing so well don't really get the idea, and they're still getting bad results, don't you have an ethical responsibility to say to them, Why don't you find other work to do?

That's what the Target Cities program was about --training staff to be able to deal with those concepts, and then getting people in the cities receiving the money to try to understand how to set up systems of looking at outcomes and motivating improvement. It's a lot tougher than just giving money and saying, get the program started. That's all SAODAP did. There was not enough time to say we're going to be back in three years to look and see how well you're doing, and we're going to compare you to somebody else. You have to have a longer time frame than SAODAP had to be able to do that.

We have some data that says some treatment programs are better than others. But often no one is in charge of saying to the not so good programs, What are you going to do about that? That should have been OTI's mission, but it was not easy to follow through because it proved difficult to close even a program that failed to see a single patient.

NC: Were there also problems with jurisdiction in terms of the state versus federal responsibilities? What was the relationship between CSAT and the state agencies?

JJ: Beny's view was that the cities were not getting a fair shake from the states. That's why he called it "Target Cities." He wanted to bypass the states. He also targeted "special populations." He knew where he wanted to direct the money and the job of the OTI / CSAT staff was to see that something decent happened and to figure out how to evaluate it.

NC: Have you run into these blending initiatives? NIDA has these blending initiatives now for researchers to try to talk more directly to treatment providers? For a while in the '90s there was a perceived split between treatment and research.

JJ: Well, NIDA has the Clinical Trials Network, and NIDA was perceived as using its money more wisely or more in keeping with what General McCaffrey liked. They got more and more budget increases, while CSAT's budgets remained flat. I think that it had more to do with making the case of what you're doing with your money. NIDA had more money than it

could really sink into research, and they came up with this idea to link the researchers with the treatment. It's hard to say what impact it has had.

I would feel more enthusiasm about it if I could really be certain that the research findings were robust enough to say to those on the front line of treatment, if you would do this, you'll get a better outcome. I think it's more nuanced than that. I think that what makes for good outcome is not just following a rigid procedure that came out of research. I can't tell you exactly what it is, but I don't think it's follow the dots or paint by numbers, although that's probably better than doing nothing and just letting clinicians do whatever they do. Clearly, there is natural variability. If you can look at differences in outcome, you can then use the natural variability to say, if others can get this level of achievement, you should be able to do so as well. At some point you have to put in incentives so that if they get to a certain level, they will be given more resources. You reward results. That's one way to get people to pay attention, not just to the mechanics of getting people into treatment, but asking, How do we get a good outcome?

NC: Now there seems to be quite a bit of emphasis on getting treatment providers to adopt evidence-based practices.

JJ: That's probably useful. But as I said, even people using evidence-based practices don't get the same outcomes. You still want to look at the outcomes. Methadone programs are more uniform than other kinds of programs, and you still see significant differences among those programs. What are those differences? What are they due to? It's not all just patient selection. How do you bring them all up to the highest quality you can? It's an iterative issue of finding what works best and trying to adopt those practices.

NC: Are we any closer to knowing what works best for whom than we were when you started out trying to figure that out?

JJ: I think we are, but just a bit. Some states are actually rewarding results. I'm surprised it's taken people 40 years, because were doing it in Illinois in 1968. I was looking at the opiate positives from the different clinics, trying to understand why there were differences. There were dosage issues, there were issues of counselors throwing people out of treatment, discharging them.

Baltimore is now planning to give some programs that achieve certain goals a 5% bonus. The net effect of that is that at some point they're going to do even better. Then you can raise the standard. The people who don't start to come up to the standard or have the initiative to find out how they could do better will ultimately have to decide to opt out of the delivery system. It'll probably take a long time.

NC: When you look back at the trajectory of your government jobs, are you pleased with the direction that drug policy has come in the last 30 years?

JJ: Do we have more treatment? The answer is yes. Is it more balanced than it was under Anslinger, where you threw people out of your emergency room, you never treated anybody, doctors didn't know anything about addiction, and they thought drug addicts were scum? Has that changed? Absolutely.

People can now say, I was an addict, I recovered. There are lots of people, prominent people, who are no longer ashamed to say, I recovered, I'm better. The stigma of having had an episode of drug problems has been substantially

attenuated. It will be for somebody else's lifetime to take the next step. I played some role in changing that, in getting treatment out so people meet people and see that they do recover. My objective was to get out the whole idea that this is a recoverable condition, not a permanent relapsing brain disease. Sometimes people take what I wrote in 1965 in that first Goodman and Gilman chapter out of context and turn it into, "Addiction is a chronic relapsing brain disorder."

But you asked about policy. I had hoped that by explicitly articulating policy, and emphasizing reduction of harm to society by putting into place the research and information systems that would cast light on the degree to which the goals of policy were being met, and by trying to summarize the costs of policy (at least at the federal level), that our drug policies would be build on such data. In that respect, I have been quite disappointed. There has been a return to escalating criminal penalties for use and possession and a progressive shift of policy resources to methods of supply control that are not as cost effective as others.

NC: I usually ask my interviewees what they think of the redefinition of addiction as a chronic relapsing brain disorder.

JJ: Well, I think it is both inaccurate and in some ways redundant. What I said when I first tried to define the syndrome was that the loss of flexibility with respect to a drug exists on a continuum. At one extreme, you have addiction, a compulsive drug-using disorder which begins to resemble a chronic relapsing disorder. At one extreme! There are milder forms of drug dependence that may or may not be chronic, but might still at some point meet our current criteria for dependence. People do recover.

NC: Yes. You also didn't say "brain disorder." You just said "chronic relapsing disorder."

JJ: Griffith Edwards and I have fought this issue of "relapsing brain disorder" since somebody decided that this is the current mantra. The model of dependence that we favor is a complex interplay between environment and what may very well be a long-lasting sense of greater sensitivity to drug-related stimuli. Obviously, to the extent that aspects of drug dependence involve learning and changed responsivity to

stimuli there are changes in the brain. There are people who have quit smoking who say, I want a cigarette every time I see somebody lighting up. But if you ask them how much they want a cigarette if they don't see somebody lighting up, you get a different answer. There may be long-lasting effects. There may even be effects that, if you start to use, you find it difficult to stop. But in an environment where you don't start again, it's a disorder that has no known disabilities. It's a funny kind of brain disease that doesn't impair you in any way except when you start using drugs. The idea that it's a brain disease means if you could just fix that part of the brain, there'd be no problem. Sometimes I wish I could make certain memories go away, but mostly I am not impaired by them. But that is not where the major inaccuracy resides. It resides in defining the entire continuum by samples taken from the extreme – those who seek treatment. As Lee Robins' Vietnam study showed, even heroin dependence is not always a chronic relapsing disorder if you look at it in the general population. Dawson & Grant, looking at large scale survey databases, observe the same thing with respect to alcoholism in the general population.

NC: Why do you think the definition of addiction as a chronic relapsing brain disorder was taken up? Why did it become the mantra that it did?

JJ: I think that it was a useful way for particular agencies to convince Congress to raise the budgets so they could fund people doing research on an interesting area.

NC: Do you mean specifically NIDA under Alan Leshner?

JJ: Whoever came up with the idea that addiction is a relapsing brain disease, it has been very successful. The budget has grown. From that perspective, you can't argue with success. But it's a Faustian bargain. The price that one pays is that you don't see all the other factors that interact. You minimize all the other major factors that interact. I just know too many people in complete recovery for me to view them as having a permanent brain disease. I know that people who've been alcoholic probably shouldn't drink again, that they may have a vulnerability. But I'm not sure that vulnerability was induced by the drug. It may have been that they had some vulnerability that they had even when they were children, and they just didn't get around to showing it in childhood.

There are lots and lots of complexities here that I think are glossed over and minimized when you say it's a chronic relapsing brain disease, and I think it's wrong to label every case of drug disorder as a chronic relapsing disorder. It doesn't characterize everybody. In fact, spontaneous remission may be more typical than a pattern of chronic repeated relapse. One can relapse, but not everybody does. In describing anything in a truncated, simplistic way you really distort what it is. I think you do the people who have had these episodes a disservice. If I'm an employer and have to bear all the costs, I don't want to hire somebody who I believe has a chronic relapsing disorder if I can help it, not because I don't wish them well, but because the cost of their care would raise the cost of the health care benefits for all the people I employ to the point where I might not be able to be competitive. But if the law requires me to hire them, I would have to.

Clearly, if you say something is a chronic relapsing disorder, you mischaracterize people who've had an episode only once in their lives. A very substantial number of young men in their 20s meet the criteria for alcoholism or alcohol abuse, but they go on, and do well, and do not meet

the criteria for these disorders later in life. Many people have stopped smoking, and by our criteria they had an addiction, a chronic relapsing disorder. Concepts have to fit the observations and the facts. Just like habituation and addiction didn't fit the facts, the "chronic relapsing brain disorder" definition doesn't fit the facts, either.

NC: Did you come up with the phrase "chronic relapsing disorder" itself? You used it in the first Goodman and Gilman chapter in 1965? Was it in the air? You say, "In extreme forms, the behavior exhibits the characteristics of a chronic relapsing disease."

JJ: I don't know. It may have been in the air, or I may have made it up. I really can't recall.

NC: I was planning to ask you whether you thought that that redefinition had been good for science, for people who are trying to talk to the public, or for clinical practice. But I think you are implying that this definition was adopted because it is the extreme form rather than the usual form or the less extreme form.

JJ: The people who went to Lexington voluntarily for treatment probably had the extreme form. Why would anybody have gone to Lexington if they had an alternative? Lee Robin's Vietnam study bears out the notion that if we had done a population survey of addicts not seeking treatment, we would have seen that those who did come for treatment had a more extreme form of dependence. However, as treatment becomes more available, people with less extreme forms of dependence will be routinely seen by clinicians and the pattern of repetitive relapse will be less common.

The way I put it in the fourth edition of Goodman and Gilman (1970) was as follows: "The intensity of this dependence may vary from a mild desire to a 'craving' or 'compulsion' to use the drug. This need … may then give rise to behavior (compulsive drug use) characterized by a preoccupation with the use and procurement of the drug. In extreme forms, the behavior exhibits the characteristics of a chronic relapsing disease."

NC: Again, "In extreme forms the behavior exhibits the characteristics of a chronic relapsing disease." Is it fair to say that you never intended the "chronic relapsing

disease" definition to be used in the way that it is now used?

JJ: I intended it to be used only for extreme forms, not as a definition of all varieties of dependence. Dependence can exist even in those who have milder forms, and who don't relapse. You have to account for those people who, despite everything, relapse over and over again. But that doesn't mean everybody exhibits that behavior. To me, the idea that addiction is a chronic, relapsing brain disorder runs counter to a number of observations. There are just many too many people who have an episode of dependence at some time in their lives who recover even without any treatment. What the motivation was for coining a new definition -- whether it was to force us to focus on the extreme case or a willingness to oversimplify for purposes of slogan -- I don't know.