In Celebration of Donald M. Gallant (1929 – 2020) by Thomas A. Ban

On March 11, 2020, Donald Gallant, a distinguished psychiatrist and psychopharmacologist, passed away.

Born and raised in Brooklyn, Don went from Boy's High School to Tulane University in New Orleans where he "fell in love" with physics under the spell of the Department Chair who had worked on the atomic bomb. Discouraged by the devastating effects of atomic weapons and seeing no future in bombs Don applied to Tulane Medical School where he was exposed to the charismatic influence of Robert Heath, founding Chairman of the combined program in psychiatry and neurology. In the summer of his sophomore year (1953) he found a taste of real psychiatry in a summer externship at a New York State hospital. It was during this externship he recognized the known similarity between psychotic amphetamine addicts and patients with paranoid schizophrenia that convinced him that the illness must have "a molecular or metabolic basis" (Blackwell 2011)

After graduation from medical school he joined Heath's Department of Psychiatry where he assisted Heath, first in his pioneer and controversial work using subcortical electrodes to locate and identify EEG patterns in schizophrenia. Then, working with and learning statistics from Mel Bishop, Don became co-principal investigator in one of the first Early Clinical Drug Evaluation Units (ECDEU) in the US National Institute of Mental Health ECDEU programs set up and first directed by Jonathon Cole. Patients for his unit were recruited from four different patient populations: (1) an outpatient service at Charity Hospital in New Orleans; (2) a schizophrenic research unit at East Louisiana State Hospital (a daily five-hour commute over secondary roads); (3) a 32-bed alcohol and drug abuse unit at Southern Louisiana Hospital in Mandeville; and (4) a New Orleans outpatient Alcoholism Clinic. Research on these diverse populations yielded 35 publications in three years (1963-1965) and contributed to Don's acceptance as a member of the American College of Neuropsychopharmacology (ACNP) in 1963. Later he became a Council Member (1973-1975) and during this period he was also Chair

of the Ethics Committee and spent considerable time developing ACNP's first Statement of Ethical Principles..

In addition to his research in psychopharmacology, Don also set up and ran a free general medical clinic in a low income housing project (the Fischer Project) and introduced an innovative method for early identification and reintegration of school dropouts.

Not only a clinician/researcher Don Gallant was also is a respected educator, the recipient of a Career Teacher Grant from National Institute of Alcohol Abuse and Alcoholism (NIAAA) in 1981 and from the National Institute of Drug Abuse (NIDA) in 1983. Also in 1981 he was appointed Tulane's Director of Medical Student Education in the Departmen of Psychiatry and continued teaching after becoming Professor Emeritus in 1991. In recognition of his activities in education, for 20 years he received the Tulane medical students annual "Owl" Award and, in 1991, the Gloria P. Walsh Award for "inspiring teaching, wise counsel and keen interest in the welfare of students."

During his lifetime research Don Gallant contributed to the clinical characteriztion of several psychotropic drugs, including triperidol (Gallant, Bishop, Timmins and Steele 1963; Gallantt and Bishop 1969); thiothixene (Gallant, Bishop and Shelton 1966); cinanserin (Gallant and Bishop 1968); and penfluridol (Gallant, Mielke, Spirtes et al. 1974) among others. He also extended clinical investigations with some of the new drugs, e.g., doxepin in chronic alcoholism (Gallant, Bishop, Guerrer-Figureo et al. 1969). Gallant and his team provided further substantiation of cardiac conduction changes with thiordazine and were first to report on similar changes with mesoridazine (Dillenkoffer, George, Bishop and Gallant 1974). As early as the 1970s, Gallant addressed ethical issues related to psychopharmacology research (Ban 2011a; Gallant and Force 1978). Regardless of research contributions his "main source of pride," as he expressed it himself, was "having been always available to my patients twenty four hours a day, seven days a week, even though it involved thousands and thousands of patient" (Blackwell 2011).

Don was interviewed by me for ACNP's Oral History Series at the annual meeting of the College inNew Orleans, Louisiana, on May 7, 2001 (Ban 2011b).

TB: This will be an interview with Don Gallant, born in Brooklyn, New York in 1929, for the Archives of the American College of Neuropsychopharmacology. We are at the annual meeting

of the American Psychiatric Association in New Orleans. It is May 7, 2001. I'm Thomas Ban. I think we should start from the beginning: where and when were you born and if you could tell us something about your childhood, education and early interests?

DG: Well, I was born in Brooklyn, New York; I spent my first 17 years going to school in Brooklyn. My family situation was very comfortable. We were middle class. I had one sister. I went to Boys High School in Brooklyn, which was, at that time, considered to be a high school in a relatively dangerous area, Bedford-Stuyvesant, but I never had any problems. After graduating, I went to Tulane University. One of the reasons why I went to Tulane was because a doctor who lived on my block went to Tulane Medical School, and we all respected and loved him very much. So, that was my idea at that time. At Tulane, for some reason, I fell into love with physics and, so, instead of taking a pre-med course, I ended up as a physics-major. In fact, one of my idols was the Chairman of the Department of Physics, Joe Morris. He had worked on the atomic bomb and I was fascinated by him. He had lost several fingers from radiation, and I thought he really was in the forefront of research. He was my hero for awhile and I was thinking about making physics as a career. But, later on, my excitement decreased in the area of physics. The bomb had been dropped. The hydrogen bomb was being worked on and I didn't see anything really exciting or rewarding to pursue in that area. The devastating effects of the atom bomb discouraged me. So, I ended up at Tulane Medical School. In my sophomore year, Dr. Robert Heath, who was a former member of the ACNP, talked to the class. He was a charismatic man. He was very impressive, a tall good-looking man. In fact, *Time* magazine had a write-up on him. They called him the Gregory Peck of psychiatry. I thought that psychiatry may be a good specialty for me. When my friends heard about that, they were very aggravated. They accused me of leaving medicine. In those days, psychoanalysis dominated American psychiatry; the Brody-Redlich concept of the schizophrenogenic mother. John Rosen wrote a book on, "Direct Analysis," dealing with psychoanalysis of schizophrenics. I found all of this hard to accept, but this was the predominant influence in American psychiatry at that time. In fact, in order to progress academically in medical school psychiatry departments you had to be an analyst at that time. Anyway, I thought I'd spend a summer as an extern in a psychiatric hospital to see if I really enjoyed psychiatry. So, that summer, in my sophomore year, the summer of 1953, I ended up at Gowanda State Hospital, which is a state hospital about 30 miles south of

Buffalo, New York. It was a fascinating experience, but also a terrifying experience. I enjoyed the patients. In fact, I had tremendous empathy for them. I really started understanding the severe incapacity of schizophrenia and psychotic depressions. At the same time, the treatment methods were unbelievable, particularly at this hospital.

TB: Tell us something about the different treatments used at the time?

DG: Insulin shock therapy was the main treatment modality that was used. One of the psychiatrists, the medical director of the hospital, came from Austria. He was using insulin shock therapy more than any other modality at that time and I, as a medical student just finishing my sophomore year, was given the temporary job of injecting 50 percent glucose in a gigantic syringe, with a "horse" needle, and trying to bring patients out of their insulin coma. It was a nerve wrecking experience, because what we used was like a "horse" syringe and I had to inject them, intravenously, before they started convulsing. Pushing the 50 percent glucose through the syringe was like pushing molasses through a syringe. And, even to this day, I get nervous when I think about it. At the same time, we saw a number of amphetamine addicts and they so closely resembled paranoid schizophrenia that I just couldn't fathom schizophrenia not being on a molecular or metabolic basis. I mean, they were just qualitatively different and seeing how the amphetamine psychosis so closely resembled the paranoid schizophrenic, I felt that psychoanalysis really was way off track. So, when I came back to Tulane, I definitely committed myself to psychiatry since it was one of the few places with an emphasis on the organic cause of schizophrenia. My empathy for the patients was very intense.

TB: Where did you do your residency?

DG: I stayed at Tulane for one special reason and that was that our department has always had a combined neurology and psychiatry department right from the very start when Dr. Heath came down here. I felt that schizophrenia was an organic metabolic problem. I wanted to get my feet on the ground, so I actually started off in neurology residency in the first year. I ended up as Chief Resident, because some of the residents were drafted into the service at that time. So I had a very good experience in neurology and, then, went on to psychiatry. At the same time, Dr.

Heath, understanding my interest in pursuing research, asked me to start interviewing some of his patients with subcortical electrodes. The number of quintuplet electrodes would vary anywhere from 80, up to as many as 120 electrodes, implanted in the hippocampus, the thalamic nuclei, pre-frontal cortex, and the limbic system, of course. I had some fascinating experiences that I can still recall today. I was always interviewing in the blind manner. We had this one-way mirror room and I would sit in the room with a patient. The electrodes were under a cap that covered the patient's head, so when he went out in public all that showed was a little cap and no one could see that he was wearing these electrodes. The wires would go through a little hole in the wall of the one- way mirror room and on the other side they did the stimulation. Even though I was blind as to time of stimulation and location, I was able to tell almost every time they stimulated the hippocampus or the amygdala. Now, this was in 1955 and 1956. Psychoanalysis still dominated academia. I remember one déjà vu experience vividly, a patient saying to me, suddenly, "you know, you look exactly like this priest that was in my church back in Baton Rouge, LA," and described the priest exactly just the way I appeared. This, to me, was unbelievably fascinating. One other person that I knew was doing this type of work was Delgado up at Yale. Every time Heath stimulated the amygdala, the patient would become uncomfortable, fearful, or angry at different times, according to the amygdala section. I thought, my god, this is real and it was a fascinating incredible experience for a young person not even out of residency. And, then, there were fascinating people that used to drop by to visit us, because they had heard of Heath's research which had been mentioned in *Time* magazine. For example, we had this biochemist from Sweden by the name of Ehrensvard, an unbelievably interesting man. He had written a book on the Biochemical Adaptation of Man, along the same lines as Darwin's theory of evolution; the theory of Biochemical Evolution from the early species up to mankind. He was interested in everything. He also drank quite a bit and sometimes he would have occasional alcohol blackouts. He spent about four months with Dr. Heath. Our lab was on the second floor at the medical school. That was our research area which stayed open 20 hours a day. Heath used to be in it about 18 hours a day. It was unbelievable the amount of time he spent there. But, one evening when Heath was not there, Dr. Ehrensvard came by and he had been drinking. Now, there was only one technician on the second floor laboratory area at that time. Ehrensvard started writing formulas and the technician told us about this the next day. He started writing formulas, first on the blackboard. Then, he kept writing the formulas onto the

door, onto the next room, all around the second floor. Well, the next day we came there and we saw these formulas and we didn't know what he really intended to do, but we were scared to erase the formulas, as we thought that he may have developed some new concepts. Meanwhile, he was drying out somewhere. So we kept the formulas on the wall for about a day and a half or two days, trying to figure out what his intentions were. Finally, he shows up about two days later and he looks and he doesn't remember writing them. It was an alcohol blackout. And, not remembering having done this, he said he just didn't understand what he wrote, so we were finally able to erase it and clean up. The Dean was very disturbed about all of this going on in the research area as he suspected what had happened. I don't know how much I should tell you about Ehrenswand.

TB: As much as you wish.

DG: I mean he was really unusual. He was a wonderful man. One day, he was down at the French Market, Café Du Monde, having coffee and doughnuts with two of our lab technicians. And, in those days, Café Du Monde used to have these gigantic sugar bowls that they'd chain to the table to keep people from stealing these sugar bowls. Well, Ehrensvard was insulted. He thought that there should be trust and that this was unacceptable to him. So he picked up the sugar bowl, ripped it off the table and ran away. They chased him and caught him. They called the police and they wanted to lock him up, but Dr. Heath intervened and stopped it from happening. He was attracted to Heath's work on Taraxein.

TB: Am I correct that we are towards the end of the 1950s? Could we go a little bit back in time?

DG: Right, right.

TB: It seems to be that Bob Heath had a major impact on your career.

DG: Oh, yes.

TB: Probably, he was the single most important person for you deciding to become a psychiatrist?

DG: I would say, yes.

TB: You attended his lectures as a medical student and started psychiatry before the introduction of the new psychotropic drugs?

DG: Yes.

TB: You talked about insulin coma therapy and also about seeing amphetamine addicts who resembled paranoid schizophrenics. Is there anything else you would like to tell us about that period?

DG: Well, actually, I remember one incident when I was extremely embarrassed at Gowanda State Hospital. I haven't thought about this in some time, but there are some incidents that always stay with you. One of our assignments was to work up some of the patients and present them to the staff and one of the patients I worked up was a man about 77 or 78 years of age. He had some problems with memory, some problems of orientation and he, in addition to having these memory problems, which were primarily organic, he also told me, about some delusional material about some oil wells he owned in New York State. Having grown up in Brooklyn, New York, I had never heard of any oil wells in New York State. When I presented him to staff, I presented him as a dementia case and, also, mentioned his delusions about the oil wells, and it became part of his diagnosis. Well, about a week later, his son shows up at the hospital and, yes, he had oil wells in New York State. I never got over that. So, I think it was a good lesson. It always made me hold back and not be too impulsive in my evaluations of patients or of people. That memory, of course, has stayed with me all of these years.

TB: Was this in the early 1950s, about '53?

DG: Right.

TB: Didn't you enter the army after medical school?

DG: No, after residency, I was drafted into the Air Force and, since I had training in neurology and psychiatry, they decided that I could do both. So they sent me over to Clark Air Force Base in the Philippines, which was a base, at that time, of about 25,000 people, including civilians and families associated with the base. In addition, I had to be responsible for treating a lot of US Government employees living in Southeast Asia. I was the only psychiatrist in that area at that time, from 1959 to '61. That was before Vietnam blew up. I was the only psychiatrist for the United States Air Force, Navy, Marines, CIA, ICA, in Southeast Asia, in addition to Clark Air Force Base and Subic Bay. That's a lot of people that I was responsible for. In fact, I had documented and kept my charts because of the tremendous experience that I was offered. I saw close to 1100 patients in two years. At the same time, I had read some of Paul Wender's work with dextroamphetamine in hyperactive attention deficit disorder. This was published about 1958. We had children in a school on our Air Force base, and we had a number of kids, whom I diagnosed as hyperactive attention deficit disorder. I used dextroamphetamine, 5 or 10 milligrams twice a day, and, in a number of cases, I had rewarding results for the child, school teacher, and parents. I think that was one of the events that steered me towards psychopharmacology because it was almost like magic. In fact, one of my associations, talking about magic, was my first year residency at Charity Hospital in 1955. In those days, in Charity Hospital, we had 1800 patients. Alcoholic withdrawal encephalopathy was not unusual because in those days the patients were left lying out in the street for several days and nobody would bring them in. By the time they came in, they were really deteriorated and in a number of cases, just giving them thiamine, 50 or 100 milligrams intravenously, would eliminate the ataxia and ophthalmoplegia of a Wernicke's Encephalopathy within 30 to 60 minutes, giving me a wonderful sensation of being a doctor, as well as a psychiatrist, and, also, making me feel that the medications had a real definite use. Of course, some of these patients would have residual memory symptoms. It was just amazing how fast the ophthalmoplegia and the ataxia would clear up. It was a tremendous experience. I should mention that we had some excellent faculty. We had Russ Monroe, who went on to become Chairman of the Department of Psychiatry up at the University of Maryland, an excellent clinical person. We had Harold Lief, who went up to

Philadelphia to head up the Family Research Unit there at the University of Pennsylvania and other really outstanding faculty in Neurology. Heath, himself, was boarded both in Neurology and in Psychiatry, as well as having his training in Psychoanalysis at Columbia. Some of our medical students such as Steve Paul and Peter Rabins published their first articles under my supervision and residents such as Chuck O'Brien went on to become outstanding clinical researchers.

TB: Wasn't Heath trained by Sandor Rado?

DG: Yes, yes, he was trained by Rado. In fact, Rado was one of Heath's heroes. At one time, Heath had the fantasy of trying to tie the biochemical concepts of psychiatry with the adaptational theory of psychoanalysis and Rado, of course, was very, very much interested in it. Rado came down a number of times to lecture to us and he was a very impressive man even though he wasn't that biochemically oriented. He was very impressive in the way he did patient interviews. He had a wonderful touch and just watching him was a learning experience.

TB: So, you knew Sandor Rado?

DG: Yes, a wonderful man.

TB: Can you tell us a little bit more about Bob Heath? No one talks about him any longer.

DG: I know. I get sort of disenchanted or disappointed when I look at some of the current articles in the literature that really evolved out of some of the work that he did. His papers are not even referenced in some of these articles. The neurophysiologic and biochemical concepts of dopamine transports and the cerebellum; his organic approach to schizophrenia and just the basic stuff we were able to report as far as the various physiologic functions of the hippocampus, amygdala and the striatum had not been reported prior to 1947-1948.

TB: Didn't he start his research in New York at Columbia?

DG: He had done work on the Columbia Greystone project, trying to find some other way of treating schizophrenia, neurosurgically, instead of doing frontal lobotomies, which were unbelievably damaging. They were trying to do partial temporal lobectomies. He also was well known around Columbia because of his interest both in neurology and psychiatry. He was only about 35 at the time when our Dean at Tulane Medical School was looking to start up the department of psychiatry. We didn't have one until Heath came down, and this was before I started medical school. It was about 1947 or so that Dean Lapham asked some people at Columbia if they would recommend anyone who might make a good chairman and they mentioned Bob's name. In the next day or so, the Dean went to Atlantic City. He was lying on the beach next to somebody and started talking about New Orleans and Tulane and mentioned that he was looking for a chairman. The person he was lying next to was Bob Heath. And, that's how Heath became down as chairman of psychiatry and neurology. He said neurology would be under psychiatry, not medicine, the way it was in other medical schools. So, that was the beginning of our department at that time. I think he was department chairman longer than almost anybody else in the history of medical schools in this country.

TB: When did he die?

DG: He died last year. He had congestive heart failure. In fact, some of the residents went to interview him about a week or two before he died; put him on tape about his experiences, his memories about the past and how he became involved in psychiatry and research. I think he also mentioned one or two funny stories about his subcortical electrode patients. The patients he selected were either drug refractory epileptics that did not respond well to anticonvulsive medication or severely ill chronic schizophrenic patients that were not responding at all to treatment. He would do these subcortical electrode implantations with the idea that eventually he would do a temporal lobectomy if they didn't respond to stimulating treatment. Anyway, one of the patients, a temporal lobe epileptic patient, ran away, left town and went to the University of Chicago. Danny Freedman was Chairman of the Department of Psychiatry at that time. The patient tried to sell himself and his hardware for $5,000 to Danny. Freedman, of course, had no idea what was going on. All he saw was somebody with a cap on his head so he called Dr. Heath and said that he had this patient who was trying to sell himself. He said, "Not only do I not want

to buy him; I don't have the money, either." So he arranged for the patient to be accompanied back to New Orleans. Things were always happening around Heath, he was a very impulsive man at times.

TB: Let me ask you about your early research. Didn't you work in the 1950s with hypoglycemic agents?

DG: Yes, right. The idea was to see if you could lower the blood sugar, gradually, and, at the same time get a therapeutic effect, but that turned out to be totally nil. It was no better than placebo.

TB: Didn't you get involved also in group therapy in those years?

DG: I became involved in group therapy with alcoholics and drug addicts who are much more amenable to the approach. With schizophrenics, I could never really do group therapy. That would be a ward meeting, it wasn't group therapy.

TB: You also did some research in the 1950s with dextroamphetamine, didn't you?

DG: Right. Paul Wender, of course, was the first person who published on its use in ADHD. That was in the *American Journal of Psychiatry* in about 1956 or 1957, right before I went to the Philippines.

TB: Wasn't that your first paper?

DG: That was my first publication and it was in an *Air Force Medical Journal*.

TB: What did you find with dextroamphetamine?

DG: We had excellent results. The attention span increased; hyperactivity decreased. I only took classical cases of ADHD and had about 4 or 5 of them. It wasn't a large study, but it was a

very impressive response that made me realize that psychopharmacology, even on that relatively primitive basis, would be very promising as far as helping patients.

TB: Are you board certified in both, psychiatry and neurology?

DG: I was board-eligible in both specialties, but I only certified in psychiatry. After taking my psychiatry certification, I just didn't want to go through the testing again, memorizing and so forth, but I felt that having neurology first helped get my feet on the ground. When medical students ask me about going into psychiatry, I usually recommend a year in neurology, to deal with the structure, before they get into something more subjective. My experiences in the Air Force, having such a large number of patients, was really good for me. By the time I left the Air Force, I had more clinical experiences in two years than I would have had in a clinical practice in the states in four or five years. I was involved in different things; we had a fellow by the name of McCann, a prisoner of the Communist Chinese. He was supposedly a second hand car salesman in Shanghai, China, and when the Communist Chinese took over in 1949, they couldn't figure out what a Caucasian car salesman was doing in Shanghai. They locked him up when they realized he was a CIA agent and he was still in prison by 1959. His wife heard he had lung cancer, so she appealed to Mao on a hardship basis to see if he would release McCann; and he said yes, if we would come and get him. Well, I was the only psychiatrist in that area, so they sent me and the Colonel of our hospital, Colonel Gehring, to Hong Kong. I had some peculiar experiences there, to give you an idea of the CIA. This was in 1959 and the CIA in Hong Kong wanted to keep what we were doing secret, so they arranged for me to meet them in a bar in some very poor Chinese area. There were no Caucasians in that section of Hong Kong, and I didn't have anything to wear but an Air Force uniform. So I would be wearing my Air Force uniform in supposed secrecy and they'd say McCann is coming tomorrow. The next day the Hong Kong newspapers would say McCann is not due till Thursday. Sure enough, the newspapers were right. The CIA was wrong. This went on for about three days. Finally, he showed up at the border on Thursday. I was sent to see if he was brainwashed. Well, the poor man wasn't brainwashed but he did have lung cancer which had metastasized to the brain. But, his wife felt comfortable since he died on what was considered to be US soil rather than in a

Communist prison cell. I had a number of experiences like that, being the only psychiatrist in that area, as well as the only neurologist. It was very, very interesting.

TB: What did you do when you came back from the Philippines?

DG: I started off with a psychopharmacology research grant. Bob Felix, who was head of NIH, selected Jonathan Cole to head up the psychopharmacology research branch and they had funding for about 19 or 20 ECDEU centers. Jonathan Cole was looking for people to apply and he called up Bob Heath, realizing Bob was interested in the organic aspects of psychiatry. Bob applied with Mel Bishop. Mel Bishop was a psychologist I thought was tremendously talented, really an excellent person. And, Heath wrote up the grant. By the time I came back, the grant had been accepted, so Bob called Jon Cole to see if he would put my name as the principal investigator. Jon Cole really took a chance on me, because I'd only had a couple of publications from the Air Force. I owe a great deal to Jonathan Cole because of his taking a chance on me. He allowed me to be the principal investigator and Mel Bishop the co-principal. If it weren't for Mel I wouldn't have known anything about statistics. I still know nothing, but I know a little bit of nothing, rather than absolutely nothing. Mel was of tremendous help; we worked very well together for many years. In fact, Mel is responsible for one of my most positive memories concerning a compliment made to me at an ACNP meeting. Correct me if I'm wrong, but it was in January of 1964, I think it was January at that time, when they had the first meetings. Joe Zubin was a superior psychologist at NYU, very well respected. At that time we didn't have the psychiatric rating scales standardized yet and were only starting to use the BPRS. Instead we used the Tulane Test Battery, which had a lot of statistical evaluations. So I presented the data from the Tulane Test Battery we used for evaluating schizophrenic patients and their response to medication relying on Mel Bishop's having educated me. At the end of my presentation, Joe Zubin complimented and said, "It's nice to see Tulane has a good psychologist." I explained, "I'm not a psychologist, I'm a psychiatrist." He was surprised! I felt that was the greatest compliment in the world; someone like Joe Zubin thought I was a psychologist! I still remember that.

TB: Can you tell us about the Tulane Test Battery?

DG: Well, it was very primitive and simple. Mel extracted data from one of our organic mental status exams and quantified it. He also added about eight or 10 questions dealing with psychoses that we threw. He took the WAIS scale, the eye-hand coordination and the IQ parts of the test and incorporated them as part of the scoring of the Test Battery.

TB: In what kind of patient populations did you use the Battery?

DG: We had several different patient populations. We had an outpatient population at Charity Hospital in New Orleans, where we ran a psychiatry service with LSU Medical School, but Tulane was more involved with patients in the State program even though we were a private medical school. So, I was running this schizophrenic research unit at East Louisiana State Hospital up in Jackson, Louisiana, a building given to us by the state. Bob Heath was a very good politician when it came to Louisiana politics and we had 120 patients, 60 male and 60 female patients evaluated for transfer to our research unit. These patients were unbelievably severely chronic. Their mean duration of hospitalization was about 22 years, with a standard deviation of only about 4 years. They had not been treated by anyone, just warehoused before they came onto our unit. East Louisiana State Hospital was out in the woods, totally isolated, about 115 miles from New Orleans, with no good roads going up there. I used to go there twice a week and it took me about two and a half hours each way, five hours of travel. So, this population, when we did quantitative organic testing, resembled dementia patients more than acute schizophrenic patients on some tests. We had some data along those lines but never published it. The families had given up on these patients; they would never visit, even though we tried to get telephone consents from the families to include the patients in our studies. In addition, every study had to have judicial consent. So, the test battery had to be a primitive one for that type of population. In fact, when the BPRS was standardized by Overall and Gorham, we could not use it in the way it was standardized for acute schizophrenic populations. We had to modify it, because on a lot of items we could not base the score on a verbal response from our patients. So we changed them to observation items. So we modified the BPRS for our chronic population, while we used the original BPRS for our acute schizophrenic population at the hospital in Mandeville, Louisiana. I also had my alcoholic and drug abuse population at

Mandeville. We had these four different populations for psychopharmacology studies and so I was involved in quite a few studies with Mel Bishop. Without him, I could never have functioned. We were so busy, so preoccupied, so involved that when I looked up one day we had published about 25 papers in just two years! I guess that's what helped me to be admitted to the ACNP in 1963. I don't think I would normally have been admitted but in 1963 there were fewer people applying and I had those 25 publications which was good as far as psychopharmacology was concerned.

TB: Was the research supported by the ECDEU grant your primary activity in those years?

DG: No, I had several primary activities. Looking back, I think they should have locked me up. I was unbelievably hyperactive and with someone like Mel, who was also productive, both of us were overdoing things. I would start off my days at about 3:30 in the morning then I would go to Jackson, do my research evaluations. I liked to get there before my daytime and research staff. I never had to criticize them. They just made it their business to come in on time, because I was waiting for them. It was the best method I ever found for dealing with staff. If they came late or started goofing off, it was very apparent to me and to them. Then, after I done my evaluations in the morning, I would drive to my alcohol and drug abuse center at Mandeville for the afternoon to do evaluations there; I was the only psychiatrist treating alcohol and drug abusers in southern Louisiana for the state. At Mandeville, I had a 32-bed unit I ran by myself with a good nursing staff. After my work at that unit, I drove back to Tulane Medical School to catch up on correspondence. Later on, I also became medical director of student education at Tulane as a third job. So, I would get home by about 6:30-7:00 in the evening, write my research papers, spend insufficient time with my family; go to bed by 11:00 o'clock and, then be up again at 3:30 am. This went on for years, and that is why I say they should have locked me up. But, I was enjoying it. I realize I was very fortunate to go into psychiatry and lucky to stumble into it the way I did. To have the opportunity to do patient care at the same time on the alcohol and drug abuse unit was fascinating. I ran the alcohol and drug abuse clinic here in New Orleans and did quite a few clinical studies in that population as well as in schizophrenic patients. I did also clinical studies in outpatients with anxiety and depression in the population at the Charity Hospital.

TB: Those were obviously very productive years. Could you tell us more about your research related to ECDEU?

DG: Jon Cole was the heart and soul of ECDEU; if he had not been given that job, it could have fallen flat. He was so enthusiastic and so pleasant and easy to deal with. We had a bunch of characters, each one of us representing different early clinical drug evaluation units, and we presented our findings, argued, and agreed or disagreed. I'll never forget that one time in 1962 we received a drug from Janssen, called trifluperidol. I should say that our chronic schizophrenic population had almost no placebo response. We did about four or five double blind studies in 1961 and 1962 and we never had more than a ten percent placebo response.

TB: Didn't you publish on that work?

DG: Yes, we did. You have a better memory than I do. I think it was in the Archives. Anyway, I only had 12 patients on trifuluperidol the first time. I presented our data at the ECDU meeting and one or two of the other investigators, who were somewhat older than me, criticized me for sticking my neck out because I was saying this was a new active therapeutic drug and not just one of those "me, too" drugs. Apparently, somebody at NYU had an acute schizophrenic population and couldn't really see a difference between the drug and placebo. So, they also disagreed. I felt pretty shaken about it. We went ahead and did the double blind study and, sure enough, trifluperidol was really a good antipsychotic. I believe that later on it showed some pancreatic problems in mice and it never became commercially available in this country. That was the first butyrophenone we studied and Paul Janssen wrote me a very nice letter, thanking me for sticking to my guns. Obviously, it helped his company. That was just one of the experiences I had with ECDEU. The group functioned very well, I thought. It was a group that traded experiences, information and data so all of us grew from the experience. Again, without Jon Cole, I don't think it would have evolved so well.

TB: Would you like to mention a few people who were involved with the ECDEU program in those years?

DG: George Simpson, you and Heinz Lehmann. Herman Denbar, I remember very well. Sidney Merlis and also Sidney Malitz from Columbia were there. I can't remember anybody from the West Coast. So it was all people from the northeast and our unit in the south.

TB: Was your unit one of the first in the ECDEU network?

DG: Yes, I think it was. Originally, the grant was directed totally towards schizophrenia, but we opened it up with my alcohol and drug abuse population in Mandeville and depression/anxiety studies at Charity Hospital in New Orleans. We dried out our alcoholic inpatient population for four weeks or so and, then, did anxiety studies in those who had residual anxiety. These were double blind studies. In one study in 1969 we compared doxepin vs. diazepam vs. placebo, and doxepin came out looking as good as diazepam for its anxiolytic effect; we published that in the Journal of Psychopharmacology. At first, other investigators didn't pay any attention to these findings. I didn't want to use benzodiazepines in the alcoholic population, who did have a tendency to misuse those drugs. From that point on I was using tricyclics, and later on SSRI's, for decreasing anxiety in generalized anxiety disorders, rather than the benzodiazepines. Later on, I think in 1990, Lancet published an article on the anxiolytic properties of doxepin. So, for many years I was disappointed other people had not started using the tricyclics for anxiety.

TB: We also studied the effect of doxepin in anxiety disorders in the 1970s.

DG: Oh, you did?

TB: We developed with Heinz Lehmann a conflict tolerance test and doxepin increased conflict tolerance just as benzodiazepines did. Can you mention other drugs you studied in those years?

DG: Haloperidol, of course. A lot of people were involved with haloperidol at the same time we were. In fact, there was a separate haloperidol meeting that went on in Miami. I remember we were all reporting about the same results; that it was an excellent new drug and the side-effects were minimal to moderate while sedation was practically lacking. So it was a much more

comfortable drug than chlorpromazine. Molindone was another drug that was interesting to study and, then, butaperazine, but nothing much came of that. That was an antipsychotic.

TB: Didn't you work with mesoridazine?

DG: That was a bad situation. I don't know if you're aware of the story, but about 1970 or 1972, we worked with mesoridazine. Two problems with that; one was granulocytopenia reported in the literature and we were able to confirm that. The other one was the prolongation of the QTc interval, so we reported on the prolongation of QTc interval with mesoridazine which we could detect double blinded. We also double blinded thioridazine after that against thiothixine and placebo using a control group of my attendants on the research unit. Lo and behold, only one of the 13 thiothixine patients had prolongation of the QTc, and one out of 13 control attendants also had prolongation after eight weeks. But 13 out of 13 patients on 800 milligrams a day of thioridazine, and seven of 13 on 400 milligrams a day had prolongation of the Q-T interval. We published that. In fact, my cardiology fellow that read the EKG's could identify thioridazine, blind. He did this with George Simpson's patients, also. George was fascinated. After we published, somebody from Sandoz called and started yelling on the phone at me, criticizing me, saying I was unethical for publishing the data. This was 1972, and I was shocked that someone from a pharmaceutical firm would start telling me I'm unethical for publishing these findings, which were unbelievably solid. They were controlled double blind studies. The EKG's were read blind and the cardiologist reading the EKG's did not know to what drug group the patient belonged. It was solid, solid data and Sandoz Company never made any mention about it. It was published in the American Journal of Psychiatry I think but nobody paid much attention to it in the literature. In the last few years people have been talking about it again, but the data has been out there for 20 or 30 years.

TB: Cardiac conductance changes with thioridazine, structurally closely related to mesoridazine, were first reported in the early 1960s.

DG: Early 1960s?

TB: Three patients died in Kingston, Canada while treated with thioridazine and it was attributed to cardiac conductance changes caused by the drug.

DG: Sudden death.

TB: Sandoz became interested at the time in the effects of thioridazine on the EKG and in a crossover study with trifluperazine and chlorpromazine we showed, and published in 1963, that it produces prolongation of the QT interval. Lou Gottschalk had similar findings a little bit later and he tried to identify the metabolites responsible for the cardiac changes. For many years these findings were dismissed. Now, there is a warning.

DG: That's the only time I ever had somebody call me up and start telling me I was unethical and the first time I had somebody from a pharmaceutical firm try to prevent publication. It's instilled in my memory.

TB: Didn't you subsequently study the effects of several psychotropic drugs on the EKG?

DG: We did it routinely at the very beginning and also with the tricyclics. We saw a few changes. The type of studies that we did would vary from East Louisiana State Hospital in Jackson to Southeast Louisiana State Hospital in Mandeville to Charity Hospital in New Orleans. They were very different types of studies. When it came to selecting patients for the ELSH research unit in Jackson, LA, there was a problem. Patients in the state hospitals were segregated, so we had a choice. Either we were going to transfer Caucasian patients to our research unit or transfer black patients to the research unit. Even though Louisiana was segregated, the federal government wasn't. So with a federal grant, we decided that we better just do our research on white patients. Thus, our schizophrenic research unit was all Caucasian. It was a paradox, in a sense. Here you have a segregated society and we're doing research on the Caucasians, who aren't in favor of integration. We were able to have both groups of patients, blacks and whites, at Charity Hospital but in separate clinics. We saw differences in placebo response which we were very afraid to publish for fear people might think we were prejudiced against blacks. We had no control over that state-imposed system. We had to do it that way or

else be arrested. At one time, we did a double blind studies of a compound called JB8181, (desipramine), vs. placebo in the white clinic and in the black clinic, about 20 to 30 patients in each group. Now, the black and white populations in Charity Hospital, this is 1963 or so, differed significantly, personality-wise. In those days, Charity Hospital was called Mother Charity, and it served the entire population that was poor. The black patients had a positive identification at that time for Charity. The whites at Charity Hospital were there because they couldn't afford private treatment and they were much more negative. These two populations had a significant difference in placebo response. With desipramine vs. placebo in our white population, we saw a significant drug-placebo difference, but in our black population, because of the significant placebo response, it was about 55%, we could see no significant difference between drug and placebo. So, when we added up the data, we saw only a slight difference of drug over placebo. Were it not for segregation, we would have never been positive about the antidepressant properties of desipramine. So we had peculiar clinical drug experiences during that period. It was a very unusual time for New Orleans and the State and we were caught in the middle concerning the way we were doing research.

TB: So, you had been involved in studying antidepressants as well from the very beginning?

DG: Oh yes. We had some compounds that were serotonin antagonists that we tried out in our population, but didn't get any real good positive results using them. At one time we collaborated with Leo Hollister on a protein fractionation serum project. Heath was interested in that area and Leo was interested a little bit in that area, so we collaborated on our patient populations, but, again, we saw no significant difference between our controls and our schizophrenics. We reported those findings because I felt it was important to report on negative results, as well as positive. Except for the Sandoz incident, we never had unusual pressure from pharmaceutical firms. With the present terrible conflict of interest by pharmaceutical firms and investigators, I do believe that bringing back the government financed ECDEU units would help to clear the air. When I reflect back on these different populations, I feel that most interesting was the significant differences between our acute and chronic schizophrenics patients. We called them Type 1 and Type 2 schizophrenics, according to Crowe, and, also, our alcohol and drug abuse populations where we reported on some of the dual diagnosis problems. Depression in our alcoholics, of

course, was much higher than it was in our non-alcoholic population. So, they were very good populations for doing antidepressant studies.

TB: In the early years you collaborated with Heath, didn't you? When did you stop working with him?

DG: I reached a point with Heath when I was not able to really collaborate with him after about 1964 or 1965. We had a difficult incident that was a sort of inappropriate situation. He accused me of stealing his research, in which I had no interest. After that, it was sad, but I told him I could stay at Tulane only if we did separate work. This incident occurred over a patient with diabetes insipidus, of all things. We had a patient in the VA Hospital and one patient at Charity. Both had lesions in the mid brain area. One patient had a temporal lobe tumor associated with diabetes insipidus and the other patient had multiple small infarcts. I was accumulating the data for joint publication with Heath as the lead author when I was still a young faculty person. He had been my teacher in medical school, my mentor in residency, and I took it for granted he would be the lead author on this paper, which wasn't covering a lot of territory on diabetes insipidus. It was not that important to psychiatry, but he blew up at me, thinking I was going to take credit for the paper. It was a very bad scene. He was overly suspicious. Russ Monroe left and Harold Lief left, after some years of difficulty with Bob Heath. I should say that at other times it was fine and easy to get along with him. At other times, it wasn't that easy.

TB: But you did publish several papers with him before you parted?

DG: He believed that the area between the limbic and the prefrontal cortex, was the key to schizophrenia, and he was focused on that. He would see occasional spiking, in the schizophrenic patients with subcortical electrodes from that area, which he didn't obtain from the temporal lobe levels. That was interesting and he published the data. We did the antibody study, the protein fractionation study. But outside of those studies, there was very little. I felt bad about it, but it's amazing the way we both functioned separately after 1965 and were friendly outside of the investigational work. We got along quite well. It was almost as if he forgot about the incident and to him it was over. I never forgot the incident, obviously, and to me, it was never

completely over. It was very hurtful for a long period of time, but I always defended him in public. People either admired and respected him for what he was doing, or else, they thought he was untrustworthy and didn't trust his data. He did have one large research problem, which was "controls." He would not pay enough attention to having good controls for his studies. Talking about controls, there was an incident concerning Heinz Lehmann. Heinz Lehmann, who I considered to be a very gracious, friendly, open person came down as part of a site visit to look at Heath's work. On our research unit Heath grabbed him and before Lehmann knew what was happening, Heath had blood being drawn, using Lehmann as a control. And, he didn't even object. Later on, when it came to evaluating Heath's Taraxein work, almost everybody was totally negative about it. But Lehmann held back for awhile and said, give him a chance. You know Lehmann much better than I ever knew him, but I think that's the way he was. That was a terrible joke, but Heath would do things like this. He was very impulsive.

TB: Could you tell us more about your research related to ECDEU? You were involved with ECDEU for well over a decade.

DG: 16 or 17 years, something like that.

TB: It was a very, very productive period in your life.

DG: There was an interesting thing about being productive and receiving ECDEU support. ECDEU gave us grant support, not for just doing the drug studies, but also to support my base salary at Tulane. So it enabled me to do a lot of work outside of ECDEU. It was of fantastic help in all areas of my clinical experiences, not just in drug studies. The ECDEU formula produced quite a few outstanding people, Arnold Friedhoff, Sam Gershon, Sid Malitz, Max Fink, who were very good. So, it was very rewarding and stimulating, very good people, for the most part. The ECDEU also permitted us to do our anti-anxiety and our antidepressant studies. We even did a study with metronidazole in alcoholics that was a "spin off" from our ECDEU grant.

TB: Tell us something about your findings with metronidazole in alcoholics.

DG: This was interesting. We did a double-blind study vs. placebo and found no difference. It was a six month study and at the end we found no difference in the abstinence rate or the number of drinking days. Even though, theoretically, metronidazole might inhibit ethanol to some extent, it didn't perform clinically in the area of alcoholism. Until that time, there weren't too many controlled research studies on the psychopharmacologic aspects of alcoholism. In fact, these protocols led to some comparison clinical studies with "criminal alcoholics," with good results. I was disappointed that our data was not utililized by other substance abuse investigators. Our data is even important for the present time. What we meant by "criminal alcoholics" were patients coming out of our state penitentiary in Angola, which is a pretty bad state penitentiary; back in 1969 it was horrible. These patients had committed major crimes, such as homicide, rape, armed robbery, directly or indirectly associated with alcohol problems. They were randomly distributed into two groups. This was a small study, only about 24 patients. One group had to come regularly every week to the clinic for treatment and take Antabuse (disulfiram). If they got involved in the project, they were given early release. Otherwise, they had to serve their regular time, so it was free choice. If they chose early release, then they were randomly distributed, one group to come to us for at least 6 months in addition to taking Antabuse. We needed the six months to work out their initial anger about the enforcement of treatment. The other group had to come to our clinic only once, in addition to the regular parole, and we had to talk them into coming on a voluntary basis. At the end of one year, even though it was a small study the results were significantly different. In the voluntary group, only one out of 12 patients was doing okay. Of these 12 patients, nine were back in jail at the time of the one year follow-up and two were at large out of state for breaking parole. In the compulsory treatment group, about seven or eight out of the 11 or 12 were doing well. It was small but it really told us something. If you have enough of a hammer to hang over the "criminal alcoholic" or probably the "criminal drug addict" and the compulsory treatment is long enough to work out the anger about being forced to get treatment, it's very worthwhile. This data was published in 1969, but nobody took advantage of that data that I know of until recently. New York State is now involved in that type of project, but it was disappointing that people did not take it up in the 1960s. We did similar projects along these lines. Remember Sam Guze? He was chairman of the Department of Psychiatry at Washington University Medical School in St. Louis. He was an

excellent person who did some research on crime and poverty and was responsible for getting the "revolving door alcoholic" grant. I was running a free clinic at the Fischer Project, which was a low-income housing project in New Orleans, at that time. I worked there Friday afternoons and all day Saturdays.

TB: You became very much involved in the Fischer project, didn't you?

DG: Right. This Fischer Project had no methadone clinic, no medical clinic, nothing, so I started a general medical clinic. Sam Guze published an article; I think it was in the Archives, on people from the poverty area in St. Louis, a black project area, who ended up in jail. What he reported was, if a child missed 10 days in a row at school in the first grade and missed 10 days in a row of the second grade, he or she was twice as likely to end up in jail as somebody who had not had that type of absentee attendance. So, we took those two pieces of data and, having spare time on my hands, we hired an African-American woman, who had a car, and paid her about $30.00 a week. We had contact with the school principal in the Treme area in New Orleans who gave us the attendance records. If a kid missed 10 days of school in a row in the first grade and 10 days of school in a row in the second grade, we had this woman pick up the kids and bring them to school. It was amazing the way their grades were jumping up in six weeks, fascinating just from coming to school regularly. To show you how important that small piece of data is, there were two little girls who missed about 20 days of school in a row in the first grade and about 20 days of school in a row in the second grade, and by the time our lady went to pick them up we learned that their father had committed suicide the previous week. The wife had died from cancer the year before and he was deeply depressed, not paying attention to the children. They were running wild. Missing school in those two pieces of time usually meant that there was a tremendously fragmented home situation that had to be adjusted immediately. That was spin-off from the time that was allowed to me by having my base salary supported by the grant. We were doing our obligated research work, but also doing "spin off" work at the same time. At the Fischer Project, we were paying for our patients' methadone; the heroin addicts had to come over from the west bank on the other side of the Mississippi River, to get their methadone every day.

TB: Could you elaborate further on this?

DG: The clinic was a general medical clinic. We made contact with the population to get them to realize that we were not out just to do research with them. We also used that population to teach my medical students about delivering medical care in poverty areas. If the patients came to the methadone clinic on a regular basis and stayed clean for two months, we would get them a job. If they stayed on the job for three months, which meant that they were clean for five months, we would pay for their expenses to move out of the project. When they reached that point, most of them would not want to leave the project. That was one of the saddest discoveries I ever had. They were too scared to move. It was their comfortable cocoon. They'd been there too long and, thus, they wanted to stay for the rest of their lives. That's where all the IV shooting up was going on and all the selling in the Fischer Project. So I felt a bit disenchanted by my experience after four years or so, and realized we came to these people too late in the course of their illness. They just couldn't make it socially outside that environment. That was a very depressing experience, to say the least. I undertook all of these clinical experiences for my own learning. For teaching, this was very, very important and still is important to me now.

TB: Weren't you the Director of Education in the department?

DG: Yes.

TB: From the early 1960s?

DG: No, later on. I became Director of Medical School teaching in psychiatry in the late 1970s or early 1980s. The previous director had left and they had nobody to take over. I enjoyed medical school teaching and I had some positive reward out of it. I call it "selfish-selflessness." They had what they call The Owl Club at Tulane; if you're an outstanding teacher, you'd get an award every year. I had been getting this award every year for 15 years or so, and, in fact, the graduating class gave me an outstanding award for medical student teaching. That positive feedback is essential if you're going to really keep functioning and enjoying what you're doing. Being the Director of Medical School Education in Psychiatry was a real challenge. To get full time faculty to put the hours of teaching in became more and more difficult, because those

people who did not have grants to support their base salaries would have to either see private patients or do consultation work with outside institutions. It became very difficult getting people to volunteer to do teaching. Sometimes, I felt like a total bully, trying to get them to spend an extra hour or two a week. It was rewarding, but also frustrating. Nowadays, it's still a problem, and the present Director of medical school education in psychiatry still has problems getting people to give lectures since it takes them away from their income, much more than back in the in the 1960s and 1970s, when we had federal grants to back us up and the grants were easy to get. So, when old people like me come down here to do some teaching they're very grateful, because I'm helping them have extra hours to support their income.

TB: So you had grants for teaching. But you also had support for some of your other projects.

DG: We had the grant to do the compulsory treatment study with alcoholics. In fact, we did another one after the criminal alcoholic study. We did a "revolving door" alcoholic municipal study. That was federal grant support.

TB: When was that?

DG: It was about 1972. The criminal alcoholic study was in 1969. We thought compulsory treatment might also work for the "revolving door" alcoholics. Now, there is a difference between these populations. The revolving door alcoholic or skid row alcoholic is less dangerous to the community than the criminal alcoholic, but we found out he's less treatable. This was a large 100 patient study funded by a separate grant. We had 50 patients come to our inpatient program for four or five weeks treatment, then got them a job and paid for a place to stay. That was our compulsory treatment group. If they missed a clinic visit, they went back and served the remainder of their sentence, which was usually between 60 and 90 days in Parish Prison, a totally different type of club to hold over someone's head, compared to Angola State Penitentiary for the "criminal alcoholics." The voluntary group just had to come to us once a week in the clinic and if they decided not to come, they were dropped. We did extensive follow up. Now, doing follow up on "skid row" alcoholics is not easy, because they move around a lot; although, in New Orleans, they liked the climate, so many of them stayed here. But they still moved around.

So, we hired someone who had some detective skills to locate our patients for follow up; looking for them in Arizona prisons, everywhere, using telephone books, school systems or what not. He was able to locate, it sounds impossible, more than 80 percent of our patients in a one year follow up, but it turns out the compulsory treatment for this group was a total waste of time, compared to the "criminal alcoholic" group. There were real differences. This group had nothing to lose. They had already been arrested an average of 50 times or more. We had one person, named "Whitey," who been arrested close to a thousand times. I know that sounds impossible, but it's true. When he needed a place to stay, he would call the police and tell them there's a drunk lying in the street and they should pick him up because he's blocking the store entrance. Then he would lie down in the street. They'd come, pick him up and take him to Parish Prison, where the food was pretty good in those years. We had a psychiatric evaluation profile, the PEP. Jerry Levine standardized this, by the way. It has a factor in it that is "overly optimistic." These "skid row" patients scored significantly higher on the "overly optimistic" scale compared to our "criminal alcoholics." They thought, "I'm going to make it"; "I can do it"; "No problem"; "I'm off alcohol the rest of my life"; this type of unrealistic approach was one factor. Another factor was "self-esteem." They scored very low on "self-esteem." So this combination of very "low self esteem," but "overly optimistic" and not having much of a hammer to hang over their heads if they skipped treatment resulted in significant failure. *Time* magazine had an article on various city jails and parish prisons in the United States, and New Orleans Parish Prison was voted among the top places to stay for food at that time. So, that study was a total flop, and it told me I'd better put my efforts elsewhere, that these patients were too far gone for us to step in and do treatment after they had been in jail 50 times or more.

TB: You mentioned the name of Jerry Levine, the successor of Jonathan Cole.

DG: We might have written something together for the Psychopharmacology Research Branch, but it slips my memory right now. Jerry was a good influence following Jon Cole. That was a very good selection and he continued doing some of the things Jon did, so he was a definite help to us.

TB: You also did some work with Leo Hollister.

DG: Yes, and with George Simpson we did some collaborative studies, and also with Arthur Sugerman. I think we also did one study with Sam Gershon, but I'm not sure.

TB: What did you do with Art Sugerman?

DG: I think it was one of the drug studies, because I know we became very friendly. It just slips my memory.

TB: Is there any other drug you worked with that you would like to mention?

DG: Thiothixene was a relatively good drug, because the sedation was much less than it was with some of the other antipsychotic drugs and its' EKG effects were practically nil. It was very clean in that area compared to Mellaril.

TB: You got interested in side effects very early and published extensively on them.

DG: Yes, I published also on the hematologic side effects of Serentil.

TB: Then you became involved with alcoholism and substance abuse. Didn't you direct a substance abuse center?

DG: I was Program Director for the southern part of the state from 1962 until I retired in1990. During that period of time, I was the only psychiatrist from Mandeville, on the other side of the lake from New Orleans. This sounds rather absurd, but I was responsible during those 30 years or so for almost 10,000 patients who came through our inpatient and outpatient program. And, one thing I'm proud of, probably the thing I should be most proud of; every one of those patients had my home phone number and my work number. I had a card I would give to every patient that came through our clinic or our inpatient program and to every patient that came through my VA program. I started that in 1985. Every patient knew they could call me anytime they wanted to.

TB: So, you were always available to your patients.

DG: The schizophrenic families or the patients themselves. This was very, very rewarding and it was very rare when a patient took advantage of it. Rather than tell a patient I was going to see them in a week, I would have them call me in two days and let me know what was happening. We might even increase their dosage in three or four days, not having to wait a week. So I was able to escalate the dosages more rapidly. It was very rewarding for me and the patients. The patients sometimes would almost go into shock when you gave them a card with your home phone and your work number, so it made for a very good automatic doctor-patient relationship to start off with. In fact, when I was doing the Fischer Project and running the free medical clinic, I was doing house calls. I wouldn't do that nowadays. This is back in 1969 through 1975 or so. Then, there were a lot of knives and only a few guns. Now, there are a lot of guns and a few knives. I would make house calls with the president of the TCA organization, an African-American. One time, the president came to see me and said, "My aunt just came down from Connecticut and she was in a mental hospital up there. Now, she's flipped out again. Can you come and see her? She's got a butcher knife in her hand." So, I said, "Yeah, but you're coming with me." So, we went into the apartment and there she is, holding up a huge butcher knife in her hand saying, "I'm going to be killing someone or kill myself because the voices are telling me to." She looked at me, I had my white coat on, and said, "Who are you?" and I said, "I'm a doctor." She answered, "You're no doctor; doctors don't make house calls." I cracked up and started laughing. Keith, the president, started laughing and she started laughing and put the knife down. That was a wonderful episode, a wonderful scene. I'll probably remember that on my deathbed. All of these things were very, very rewarding and that was the most fun part of what I was doing, so it was well worth it.

TB: It seems that you had numerous activities simultaneously.

DG: Yes, we were too far spread out. I sometimes think I should have devoted almost all of my time to psychopharm, and yet when I look back on my memories, I'm very happy I did all of that crazy stuff.

TB: Didn't you also study sexual behavior?

DG: That was a joke. I really didn't study that. What happened was that there was a magazine called *Medical Aspects of Human Sexuality*. I don't know why they called me to write two articles; I thought it was a joke, but they were offering to pay me $250.00 for an article on something I didn't know about so I said, great. One article was on Sexual Positions during Pregnancy. I thought I would play a joke. I went ahead and wrote that the best position is the pes caelum position, which meant "foot in the sky." The editor corrected it and changed it to "foot on the ceiling" and published it. They give me $250.00 for that. I thought, hey, this is a great occupation. That was a good deal of money for an academic back in the 1970s. I mean, you can publish anything you want in this magazine and get revenue. Then there was some other article, I've forgot what the title was, but I had one reference as something about "how to grow aphrodisiacs in your garden" by William Shakespeare. They published that in the *Medical Aspects of Sexuality*. I also worked with pipotiazine palmitate. Do you remember that?

TB: Yes, of course.

DG: A drug with a very long half-life. One injection lasted almost four weeks. I don't know why it never became commercialized in this country, but it worked very well in the chronic schizophrenic population. It was the first significantly long-acting neuroleptic. It was an ideal drug, once every three to four weeks. In my introduction to the study that we did, I wrote that this compound had such a long duration of action it must be slowly metabolized by the liver and would work well in people who had the "Simpson syndrome." In the next sentence I added in reference to the "Simpson syndrome" that the drug is only slowly metabolized in the liver when drinking "First Growth" wines. When I mentioned the "Simpson syndrome" in my presentation at the ECDEU meeting, everybody got a good chuckle out of it. I used that presentation to cite that as a reference in the article. It was a joke, but, it was fun at that time. I was somewhat of a mischievous character at times.

TB: When did you get involved with medical ethics?

DG: I must have had some guilt about my mischievous behavior. It was when Phil May was president of the ACNP back in 1973 or 1974, around that time. Phil was a fantastic person. He was the British version of Heinz Lehmann, very gracious, and if I remember correctly, he was the first investigator to perform a comparison study of psychotherapy with drugs in schizophrenia. It was an early study and the design was chlorpromazine plus psychotherapy, vs. CPZ alone vs. psychotherapy alone.

TB: What did he find?

DG: Psychotherapy by itself was like placebo in those schizophrenic patients. In the early studies, chlorpromazine by itself is significantly better than psychotherapy in schizophrenics and psychotherapy added nothing to the efficacy of CPZ. That was really the conclusive study showing that all this nonsense by Brody and Redlich, the schizophrenogenic mother, and John Rosen, direct analysis, was useless. I was Chairman of the ACNP Ethics Committee, at that time and Phil asked me, since I was Chairman of the Ethics Committee, to start drawing up guidelines, stating the principles of Ethical Conduct for the ACNP. It took a lot of my time but I really got involved. In fact I asked Dave Mielke to take over as principal investigator of our ECDEU grant because I was spread too thin. I was overly involved in developing the Statement of Principles but I think it was the first Ethical Code developed for any medical organization. Psychology had their own, but not any medical or research organization I'm aware of. I spent about two years on that, working with Bob Force, a lawyer at the Tulane Law School, and the entire membership of the ACNP. I sent out several drafts to get input from every member and reviewed all of the inputs. Most of the members returned it so reviewing their comments and trying to incorporate them was a much greater job than I realized it would be. Since Phil May had asked me, I was obligated because I felt indebted to him as a person. He was just a fantastic individual. We finished it after about two years and it came to a vote by the membership. We had about 162 members at that time and only one member voted against it. I thought to myself, out of all these eccentric people who belonged to the ACNP, to have only one person vote against it was a fantastic accomplishment, so I was well satisfied. We modified it again in 1984-85, and it has been modified and changed quite a bit since that, as the years pass. That was a very

intense piece of work and it made me examine a lot of what I was doing. It was very educational for me and I learned a lot. Bob Force and I later edited a book on Legal and Ethical Issues in Human Research and Treatment that had the imprimatur of the ACNP. We had Albert Jonsen, Angela Holden from Yale, Alan Stone, Karen Lebacqz, Robert Levine and other contributors. Also Father McCormick at Georgetown was another person who was very sharp in this area. So, it exposed me to quite a few people outside our field and was something I can thank our organization for, because I never would have got involved in that area, were it not for the ACNP.

TB: You also wrote several chapters in that book.

DG: It was a book on *Ethical Informed Consent for Research and Treatment*. I felt that was a complicated area for schizophrenics and still is.

TB: It was a very successful publication.

DG: Yes, we had a good number of reprint requests. In fact, there was a spin off. I was invited to a number of places to give lectures. It was unusual at that time to have the Principles of Research, which had some legal implications. I think the one person that voted against it was afraid of legal implications. I received invitations to participate in symposia in Michigan and in Boston. It exposed me to different areas and I became quite knowledgeable.

TB: You also wrote a book on alcoholism, didn't you?

DG: I wrote a book on alcoholism by myself. That was probably one of the things I did that required more time than anything else. It was *Early Diagnosis, Intervention and Treatment*; describing, not only diagnosis, but the various types of interventions and treatment modalities. Treating alcoholism is a difficult problem; the non-compliance rate is quite high but no different from asthma or high blood pressure. The idea that you have a genetic susceptibility for alcoholism as well as for high blood pressure, asthma, and diabetes, and comparing it in these terms makes it more acceptable as a chronic medical illness. I've also written on the relapse or non-compliance rate, showing that the rate in alcohol and drug addiction is not that much more

significant than non-compliance in diabetes. In one survey of diabetics, where medication was prescribed by their physician, approximately 50% did not take their medication as prescribed. Pretty shocking, that it was almost as high as the non-compliance rate with alcohol and drug addicts. So, one relapse should not be regarded as an end-point any different than with diabetes or high blood pressure. About 50% of hypertensives don't take their medications, as prescribed, at one time or another. The worst case of non-compliance I use in teaching medical students was published in the *Lancet* 12 or 14 years ago on congenital hypothyroidism. Almost half the mothers were not giving thyroid as prescribed for the children. That is incredible. By trying to get people to understand non-compliance not being that different in alcoholics and drug addicts from other medical illnesses, I felt like I could make it more acceptable to the public and encourage less negative reactions which I always felt was very important. Psychiatry has always had a problem of acceptance in one way or another, lesser nowadays than years ago. When I first told my friends I was going into psychiatry, my best friend stopped talking to me for two days. That's a true story. He said he was going into Internal Medicine and went up to Mt. Sinai and became an outstanding internist. He said to me, "You're leaving medicine; you're deserting us." So, psychiatry has had a tough row to hoe, as far as being accepted.

TB: North American psychiatry was almost entirely psychodynamic at the time you entered the field, but it seems that New Orleans with Heath were biologically oriented.

DG: Right.

TB: So, in a way, you were lucky.

DG: I was very lucky. He gave me a chance to get my feet on the ground and I can thank him for that.

TB: Were you involved in psychodynamic psychiatry at all?

DG: I wrote a little paper one time for GPs on how to treat primary care patients psychodynamically, but I'm a bigger fan of the systems they worked out in recent decades;

interpersonal therapy, cognitive behavior therapy, and more recently, the cognitive behavior systematic analysis model for depression. I'm much more impressed by their results as they're easier to evaluate. I've read the *Interpersonal Psychotherapy* book by Weissman and Klerman, because I have to teach part of that to my residents and discuss it with them when they're seeing patients. When I retired in 1994, Tulane was very nice to me. Since I put in 40 years of teaching they made me Professor Emeritus and let me have an office across the hall and a secretary. I don't get paid. This way, I'm in control of my life, which was never true before. I come in three days a week, on Mondays, Tuesdays and Wednesdays, teaching medical students, the freshman, sophomores and juniors and the first year and second year psychiatry residents. The areas I cover with them are the psychopharmacology of anxiety, affective disorders and schizophrenia. I also teach group therapy techniques, and alcohol and drug abuse. Those are the main subjects. It keeps me feeling young working with young people, which is very, very important, so my marbles don't get too rusty!

TB: So, you do that three days a week?

DG: Yes.

TB: And, the rest?

DG: The rest of time I'm reading. I was accumulating books that had nothing to do with medicine for many years, looking forward to the day when I retired to read them. Nowadays I read more medical journals than I did before, because I didn't have time, but I also read more novels, both fiction and non-fiction. I have much more time to do that, because reading is a major part of my life. The only problem I have is in my compulsiveness. When new books or magazines come out I don't have the ability to turn them down, whether they're medical, non-fiction or fiction books that have nothing to do with medicine. So I keep on buying books and magazines; for example, *The New Yorker, Scientific American, The N.Y. Review of Books,* etc. I've never caught up with all the books I've been saving to read in retirement. But I hope I never catch up. It's fun!

TB: So, you read a lot.

DG: I read an awful lot. I read more than ever before. I'm always calling up George Simpson to tell him about something I've read. He does that with me. He reads a lot, also.

TB: Do you keep close contact with George?

DG: Yes, I've always felt George and I had a lot in common in spite of our tremendously different backgrounds. We've always had fun with each other and I like to think that attracted us to each other as well as our interest in food.

TB: Good food.

DG: Yes, we both enjoy good food. That's one of the reasons I stayed in New Orleans. When I look around the country and think of different opportunities, such as Kansas City, oh my God! I couldn't handle that food, and my wife also enjoys good food, so we had no choice but to stay. And, it's a very interesting town. We live in an area that's only twelve minutes from downtown, and yet it's totally different from the French Quarter and the French Market.

TB: I remember when we had an ECDEU meeting here. I also remember the hospital you worked in those years. We were traveling for two and a half hours to get there.

DG: Yes, but we have an interstate now. At the time we had the ECDEU meeting down here back in the 1960s we had a meal at Antoine's. Were you there?

TB: Yes. That was in 1967 or 1968.

DG: I asked each ECDEU member to chip in about $10.00 or $12.00. The wine we were served was a Chateau Pontet Canet. I remember because I had to pay for part of that wine out of my own pocket. Arnold Friedhoff got up at the end of the meal and toasted me, saying something

about how fantastic. They got their money's worth and I was thinking to myself part of that was my money!

TB: Let me switch. Could you tell us something more about your involvement in group therapy?

DG: I enjoyed group therapy very much. I'm doing group therapy right now. We had a group therapy association, Louisiana Group Therapy Association, as part of the National American Group Psychotherapy Association, and I was very involved in it. We had no one to teach us that in 1962 and 1963, so I used to meet with four or five prominent psychiatrists in town, one night a week. We'd supervise each other, criticizing each other and reporting on our groups. That was the only way we could learn because we had nobody to teach us. There was somebody named Hugh Mullan up in New York who worked with group therapy. He was very good so we had him down as a guest. I was enjoying it so much; working with alcohol and drug addicts, the group phenomenon naturally takes place. That's what some of us call the pseudo-cohesiveness, false feelings of togetherness when we first meet somebody who has the same background we do. Example: when I was overseas in the Air Force, in the Philippines, I hadn't met anybody from Brooklyn, New York in a couple of months, which is unusual, because we had a couple of million people living there. Then, one day, somebody came over to our Air Force base and I met him, another doctor. He was from Brooklyn and he went to the same high school I went to, Boy's High School. Before you know it, you start getting close to a person, even though you know nothing about them. That's what I call pseudo-cohesiveness, false feelings of togetherness. Of course, later on, about two or three months later, I found out he was a real terrible person and I didn't like him at all. So, sometimes our first impressions are not the right ones. But, in the beginning, alcoholics have this automatically happening to them. "Oh, you're an alcoholic; you go to AA. What AA group do you go to"? So, they fall, naturally, into this group phenomenon, and they're easy to treat with group therapy, much easier than other types of patients. Nowadays, most of them are mixed drug addicts and alcoholics, but they still have this AA approach or NA (Narcotics Anonymous) approach. I started to really enjoy group therapy because it flowed so smoothly and so evenly and we worked out some techniques that worked quite well. And, at the same time, I started realizing, early in treatment programs, that if you didn't deal with the spouse, you're missing out on a gigantic part of the patient's life and each one of us sees the world

through our own eyes. The more eyes you have to help you see the world, the more valid the observations become. So, I started putting this practice into married-couples group therapy, treating six or seven couples at one time. I used to do this on Wednesday afternoons, two groups, one from 1:15 to 3:15 or 3:30 and the other one from 3:30 to about 5:30 or 5:45. They were coming from Baton Rouge, because I was the only one doing this for the state. They also came from Lafayette, Louisiana, Cajun country, and they were people who were fun to deal with, especially the Cajun population. They love to drink and they love to talk, so they are very good patients. And, I found out that when we did a follow-up on the first 160 couples that we treated, back in 1969 or 1970, only two of them had separated in the 18-month follow-up, which was probably below the national average. My success rate, as far as abstinence and decreased days of drinking, was much higher because that was a select population I was dealing with. Of course, if they're still married, that means they have a support system, and if they're still married, they're more likely to have a job, so you have more positive predictors working for you by just treating married couples. You have a much better chance of success. This added to my enjoyment, because I might have been finding failure in one area, but I was successful in this area; that made it much more fun. You need to have some positive reinforcement with a group that's difficult to treat.

TB: What about drug therapy?

DG: One of our former residents, Chuck O'Brien, showed naltrexone efficacy very nicely, along with Stephanie O'Malley from Yale. In two separate double-blind studies, they showed how naltrexone decreased the number of drinking days and decreases craving to some extent. The data are solid; two separate studies done without each one aware of each other, coming up with the same results was impressive proof of the drug's efficacy. So, we now use naltrexone frequently. One other thing I forgot; I used to consult on college campus two half days a week treating Tulane uptown college students. I started going on Tuesday afternoon and Friday afternoon, it must have been about 1973, which was a nice change of pace from drug abusers and schizophrenics. It was like ice cream, soda and candy, to consult and treat college students. They had a number of eating disorders. In bulimia cases, I used naltrexone, not in a research study design, but case by case, and had some good results with a few cases, decreasing the binge eating

and euphoric responses to carbohydrates. At the same time, I had the opportunity to participate in a naltrexone nationwide VA study with Leo Hollister, who was on the committee to look at the data for using naltrexone with heroin. Of course, those were very disappointing data. The patients would stop taking it. With naltrexone in bulimics I had patients, when they graduated, who were afraid to leave town because they might not get the naltrexone elsewhere. So, it did help a sub-group of bulimics. But, I wasn't running a controlled study.

TB: So you were using naltrexone I bulimia. What about Antabuse (disulfiram) in alcoholics?

DG: I always used Antabuse, but I tried to get a support system to supply it to the patient. If there was a wife, I'd have the wife give the Antabuse to the patient. I would tell a patient, you're not taking it just for yourself; you're taking it for the family. If they didn't have a family and they were working, and had an employer, I'd have the employer give them the Antabuse. So, I tried to bring in support systems to administer the drug. Of course, the original Antabuse study with controls was done by Fuller, at Cleveland Clinic, before he joined the NIAAA. He did that in 1973 or 1974 and found no higher abstinence rate at the end of nine months, but he did find a significantly higher number of days of abstinence on the Antabuse. But the abstinence rate was only 20% or less in nine months. Just seeing that data, you automatically realize if you have a support system, use them to give the Antabuse with the patient's permission.

TB: What else were you involved with?

DG: Teaching and supervising. You have to be careful with Antabuse therapy in schizophrenics, because Antabuse does inhibit dopamine ß-hydroxylase, and you don't want to increase their dopamine too much. So, in schizophrenics, I would use a placebo dose of Antabuse, 125 mg/d. We did have a problem with our schizophrenics who were crack abusers; they had a little bit more of a response problem. I think George Simpson made some similar observations. Our patients would become more outgoing, their thought dissociations might decrease, but sometimes the paranoid delusions and hallucinations persisted. I found myself getting desperate and would add a small dose of a dopamine-D_2 blocker on top of the atypical antipsychotic, and, in some cases, the voices would go away. I haven't done a double-blind study, but I have a suspicion that

the reason is that cocaine causes renal vasoconstriction with a decrease in blood flow velocity and vasoconstriction in the brain vessels resulting in decreased perfusion in the brain. Perhaps not all of our drugs are getting through to the right areas and these patients may need more of a kick with a dopamine-D_2 blocker in addition to atypicals. That's been an observation of mine and I think George agrees with me. The dual diagnosis patients we wrote about back in 1979 have increased, dramatically as the years go by. There are several good chemical reasons for that. I had an interesting experience about this problem. The original report on the incidence of alcohol and drug abuse in primary psychiatric patients came in 1975, or maybe a bit earlier, from the Bronx VA Hospital. The incidence was almost sixty percent in an urban psychiatric hospital. At Charity, we were running about the same percentage. In Wisconsin, they asked me to come up there for a three day seminar on alcohol and drug abuse, about twelve or fourteen years ago. I was giving them the data about the incidence of dual diagnosis problems in our primary psychiatric patients and I made the statement, "I guess, here in Madison, Wisconsin, you're not going to have the high incidence, because you don't have that much of an intense urban situation." They were too polite to disagree with me but two weeks or a month later, I received a manuscript in the mail from a PhD psychiatric social worker with data showing me that the incidence of dual diagnosis in their study was about 40-50% in primary psychiatric patients. So, it didn't seem to matter: wherever you live, you're going to have an increasingly high incidence. It's a real problem because it contaminates drug studies, particularly in outpatients it's a disaster. It was much easier to do studies years ago. There was a study done in New York State, I reported on for the Journal of Clinical and Experimental Research in Alcoholism about 30 years ago, on the incidence of depression, anxiety and schizophrenia among primary alcoholics. It was done in sixteen or seventeen different units in New York State. The incidence was only about 10%. Nowadays, we see a much higher incidence of substance abuse problems in our primary psychiatric patients and vice versa. It's gotten much worse.

TB: Besides naltrexone and Antabuse did you use or study any other drug in alcoholics?

DG: We tried lithium, which turned out to be of no value. We did a lot of antidepressant studies in primary substance abusers, but we didn't do long-term follow-up to see if it had an effect on abstinence.

TB: You mentioned using SSRI's in alcoholics.

DG: The Toronto Addiction Research Center, which is one of the best on this continent, reported a short term study using one of the SSRI's in a one month study to decrease alcohol intake. This was some years ago, and because of their findings we tried it, but we didn't do an adequate double-blind controlled study. So I can't be objective. When you're doing double-blind studies in this population, you have to sort out those with major depressions and eliminate them just to see if a pure SSRI can reduce the alcohol intake.

TB: Do you have a preference for any antidepressant in alcoholics?

DG: I like short-acting compounds, so I would choose sertraline, something that's a little bit more pure as far as affecting the liver.

TB: What about doxepin?

DG: I would not use doxepin now with the SSRI's available. Cardiac wise, you're better off with SSRI drugs. Our alcoholic population seems to be more sensitive (now this is subjective), to anticholinergic side effects than our non-alcoholic population. I have no idea why; it just seems that way. I have more complaints in that area. So, I tend toward the SSRI's for depression in these patients.

TB: Do you remember which antipsychotic drug you studied first?

DG: We might have done work with Stelazine, trifluoperazine. Then we used that as a comparison to a newer line of drugs in our studies.

TB: Which was the last antipsychotic you studied? Did you work with any of the atypical antipsychotics?

DG: No, I handed over the unit to Dave Melke before the atypicals came out.

TB: When did you hand things over to Melke?

DG: 1977, somewhere around that time, when I became overly involved in the ethics code and substance abuse.

TB: Didn't you work with penfluridol about that time?

DG: Penfluridol was one of the last the compounds I worked with. It was a beautiful medication in our patient population. We gave it orally once a week and it worked, as far as antipsychotic activity was concerned.

TB: There was a paper published recently on the effect of penfluridol on the EKG.

DG: Is it used in Europe?

TB: At a certain point in time, it was used very extensively, because it is a selective dopamine-D_2 blocker.

DG: The side effects were minimal. I had fantasies of using that compound and training a high school graduate to make house calls once a week on my schizophrenics, who were living at home or in a residential home, giving them the medication orally. Another compound, I worked with was sulpiride. That was a clean compound. We didn't find any side- effects to speak of in our chronic schizophrenic population, and it's quite an active antipsychotic compound. In Europe there were some controlled studies showing that it was also an antidepressant.

TB: When did you study sulpiride?

DG: That was about 1976. That might have been the last compound I did and thought was clean. I have no idea why that didn't go any further here.

TB: The benzamides are very successful in Europe, but they are not used in North America.

DG: I don't know why. They are cleaner than the atypicals.

TB: Did you work with clozapine?

DG: No, we didn't have that opportunity. Herb Meltzer was doing it in about 1980. But, sulpiride was another ideal drug. Sulpiride and penfluridol were two of the nicest drugs that came down the line but they didn't become commercial in this country. You mentioned the possibility of EKG problems with penfluridol, which I'm not aware of, but that would be something to think about.

TB: Are you still using haloperidol extensively?

DG: No. I may use it every now and then; sometimes intravenously. For example, in a mentally retarded patient who had chewed off her fingers, while putting on a cast to protect her from further damage, we used intravenous haloperidol. Otherwise, I tend to use more atypicals. One reason for not using antipsychotics too quickly is that someone might come in with a PCP psychosis, which can look like schizophrenia, so the patient clears up after an antipsychotic and you will not find out what the basic problem was.

TB: You also studied withdrawal effects of neuroleptics, didn't you? You had a paper on that some time ago.

DG: I think that was one of the mistakes that we made. In one of our drug studies, with a phenothiazine derivative, for some reason we stopped the compound and had nausea as a result. I regarded that as a rebound phenomenon, but that was a mistake. George Simpson corrected me on that, because we had also suddenly stopped our antiparkinson drug. So it was really a rebound from the anticholinergic, a cholinergic response with nausea. I was a little embarrassed about that.

TB: What about antidepressants? Which was the first antidepressant you studied? Wasn't it desipramine?

DG: Yes, that was the first. We might have studied one or two other antidepressants that had a number, but were no better than placebo. In the beginning, when we arranged to do double blind studies on the Charity Hospital outpatients we were trying to concentrate on depression. We might have used one or two compounds in the beginning. Desipramine was the first real active compound we tested that had definite activity compared with placebo. It still is a very nice compound.

TB: Which was the last antidepressant you studied?

DG: I'm not sure.

TB: You have also been very involved with the benzodiazepines. Weren't you on a committee on the benzodiazepines?

DG: Yes, it was the APA committee.

TB: Would you like to comment on that?

DG: Carl Salzman was also on that committee and some others, of course. It was to evaluate the addiction and dependency problems of the benzodiazepines. The recommendation was that there were some people possibly more susceptible to addiction, but the overall opinion was these drugs could be handled comfortably if the patient had the correct diagnosis of Generalized Anxiety Disorder. Otherwise, there was an addiction potential. But the data on the benzodiazepines was interesting in a sense; I had a run in with the manufacturers of alprazolam. When I was a member of the FDA advisory psychopharmacology committee, alprazolam (Xanax) had already been approved to treat Generalized Anxiety Disorder. I was given the assignment, by the FDA committee, to evaluate its antidepressant activity because the company wanted to market the

drug and list it in the PDR as an antidepressant as well as an anti-anxiety agent. There was a suggestion to compare it with placebo. But, I had a great deal of concern, which I expressed in my report to the FDA psychopharmacology committee, that if alprazolam was listed in the PDR as a primary antidepressant, patients with depression were going to get addicted to the compound. They could be more susceptible due to the anxiety that accompanies depression. The data showed that it didn't seem to be as effective as our primary antidepressant medications. So, the company did give me somewhat of a hard time. Depressed patients with low self-esteem could be more liable to the potential addictive properties of benzos if there was an inadequate antidepressant response. It never did get approved as a primary antidepressant and I was happy to see that. I didn't think it was needed in that area. In our alcoholics, while we don't have any objective evidence, we do have some data that suggests alcoholics may be more susceptible to benzodiazepine addiction. Cirillo, in Boston, has published data on adult male children of adult male alcoholics using a euphoria rating scale, comparing 1 milligram of alprazolam vs. placebo. They scored significantly higher on the euphoria rating scale on the drug than they did on placebo. So, those suggestions make me a bit worried. I've seen benzodiazepines spread out too much across the world.

TB: During the years you were involved in clinical trials with psychotropic drugs major changes were taking place in the methodology of clinical investigations.

DG: When I was at NY Gowanda State Hospital in 1954, not only did they use insulin shock, they were also using ice cold baths. When I saw that I just went into shock myself. They had the patients tied who were agitated or sullen tied in restraints surrounded by ice bags. It was like a horror movie, I saw things that I shouldn't have seen. I was too young. I never did see a well-controlled study of insulin shock therapy.

TB: Do you remember when the BLIPS system was introduced?

DG: It was a tremendous addition to psychopharmacology evaluations, getting into objectivity. I used to brag that psychiatrists in the sixties were more objective in their drug evaluations as the BPRS and the BLIPS came along than internists and other medical specialists, even though they

gave psychiatrists a hard time for being so subjective. We were actually, more objective in what we were doing. And I felt good about that.

TB: At the time you started there were no ethics committees, no patient consent forms.

DG: We had to get patient consent at the East Louisiana State Hospital to move them onto the research unit. On the consent form it said that the patient would be receiving investigational drugs, that the families would be notified and if they had any objections, the family should tell us and we would give them the results. But, of course, these families were, for the most part, emotionally if not physically separated from the patients. A part of that, you might say, was hypocritical because we knew the families would not respond. And over ninety percent did not; ten percent did, I would say. We also had to obtain judicial consent from the local judge. So it was paperwork consent in a way. That's one of the things I questioned myself on, when I was doing the ACNP statement of principles. I questioned my own approach to research and that was a very healthy thing to do. With our alcoholics and drug addicts we always obtained consent from the patient, it was something we did without thinking, part of our routine. But it wasn't as good as informed consent is now. It wasn't as detailed, but the patients were told that at one time or another they might not be receiving active medication. We also told them about the potential risks and benefits. I don't ever remember doing a study without a written consent.

TB: So, you did have signed written consents from the beginning?

DG: What happened was that because of the type of research Heath was doing, which had some real dangers, putting electrodes into the brain, he had to develop a consent sheet at the medical school's insistence. That might have had some spin-off effect in psychopharm so that, automatically, consent sheets were expected, not by the department but by the medical school.

TB: Were you the one who implemented the program in alcoholism in Tulane. You ran it for well over 25 or 30 years?

DG: I didn't implement it, but somebody who was vice president of a prominent clothing store. He had a brother, who was an alcoholic.

TB: When was that?

DG: This was 1961, and he had a lot of influence in the State legislature so there was a building built for student nurses at the psychiatric hospital in Mandeville on the very large beautiful grounds; this building was to house the student nurses while they rotated through their training. The contract fell through at the nursing school leaving this beautiful building empty. It was the perfect place for treating alcoholics and drug addicts. There were thirty-two beds, males on one side, females on the other side, no more than two patients to a room, private bathrooms. It was one-story, when they walked out of their rooms, there were the piney woods, and it was just unbelievable. But it all happened by accident; nothing like this is ever planned. Often you plan something and it turns out horrible. He was able to get that building and get the funding to run the program; his name was Simon Marx. I remember him well, a big, hefty man who was very outgoing and knew what he wanted. So, I had this building and state funding; it was a part-time civil service job, which helped a lot. I was only making $12,000 a year with my psychopharmacology grant, and I needed extra money for my family. This was a part-time job that helped me financially, because I never did enter private practice.

TB: So, you were never in private practice?

DG: No.

TB: But, then, you ran this program until when?

DG: 1998; when I retired from this area.

TB: Can you tell us something about the program?

DG: The Alcohol and Drug Treatment Center?

TB: Yes.

DG: I had a lot of emotional investment in that; because of the staff and the patients I did a number of things with the program that nobody else does. Because of my time limitations, I was there just two days a week. On Monday from twelve noon until five o'clock, and on Thursday morning about five-thirty a.m. We would wake up the poor patients early. It was a terrible thing for me to do to them. I then left at 1 p.m. for Tulane. Within that limited period of time, I did a lot of things. I had to run ward meetings twice a week. But I'd have the nurse call me every day at home in the evening and make ward rounds on the phone, one patient at a time. If the patient had to talk to me, I'd talk on the phone. I used the phone an awful lot. Either the patients would call me or the hospital would call to do ward rounds. When I went to the unit, I would do group staffing. Instead of staffing patients, one at a time, I'd staff three patients and have them interact with each other. I called it a group staffing procedure, which I published. That meeting would last for about two and a half hours, three patients at one time for three hours, rather than on one patient for an hour and a half each. I got them to interact with each other and it became an accepted routine. Patients are interesting, very fascinating. You'd think they'd be too embarrassed to talk about their problems. We would talk before hand about any sexual or personal problems they wanted to discuss in private. The medical students, four or five at a time, would sit in with me as it was an excellent learning experience. We did a group family session, later on that day, with three or four families at one time, going over the history, getting the information, and so forth. It was a very intense treatment and the patients made good progress through this group process. The follow-up would be in married couples' or regular group therapy. Then we had the ward meetings, which I would do Mondays and Thursdays, plus the phone rounds. I had some excellent counselors I trained, who did a lot of behavioral work, including implosion therapy, and desensitization for social phobia problems, like fear of heights. When you do surveys the incidence of phobias is sky high in this population, but you don't find out unless you ask specifically. We used an eighty-two items Fear survey questionnaire, trying to make sure we didn't miss any phobias. We also standardized a relapse prevention inventory (RPI), that I read about in *Lancet* to prevent relapse. And, we did a lot of role-playing, in addition to the group therapy and individual counseling. I'd have my social worker take the patient up to the top of the

Trademart tower, step by step, for desensitization or all at once, implosion therapy, keep them looking over the side of the building until panic subsided, and then feed them a candy bar or ice cream as positive reinforcement. It worked but you had to keep them trusting you and stay with them, looking down from the top of the building. So we did a lot of behavioral work, in addition to individual and group work. I also used Antabuse in my alcoholics about ninety percent of the time. If they were married, the spouse gave it to them. If they were crack addicts, I automatically talked to them about Antabuse, because there was a study done in Arizona that even if those crack addicts who were not alcoholics drink socially, they were about seven times more likely to relapse compared to other crack addicts who didn't drink at all. Because of that, I tried to give it to crack addicts, and would say, "Antabuse might be an additional safeguard here." It's even more interesting since Chuck O'Brien published on the ability of Antabuse to elevate the cocaine level and cause uncomfortable symptoms in crack addicts. Some crack addicts were really highly motivated to take Antabuse, even though they weren't alcoholics. Very interesting! We had our dual diagnosis patients at VA, when I was running that unit from 1985 to 1998. In that program, we had a separate dual diagnosis group going on. In group therapy with dual diagnosis patients, we were a little bit more didactic, a little bit more educational and less confrontational. You don't hit the denial mechanism as hard. You've got to provide some social outlets for them to want to give up their alcohol and drug abuse. You have to go slowly and tolerate more relapses with them, particularly in the beginning of therapy.

TB: During the years you worked with many people. Could you tell us something more about Mel Bishop?

DG: Mel Bishop, of all the people I worked with, was the most helpful and the most valuable, because he taught me a lot. He was a very retiring person who didn't come on assertively and he was very intelligent.

TB: Didn't he move to the pharmaceutical industry?

DG: Lederle pharmaceutical gave him a very good offer financially. The salary of a psychologist just wasn't high enough and Mel had four children. Some of them were already

grown. They were going to college and he needed the money so he went to Lederle. Harriet Kiltie is the one who hired him away from us. Bill Swanson took his place, later on. Swanson was a sociologist-psychologist who could handle statistics.

TB: I have not heard about Mel for a long time.

DG: Shortly after he retired from Lederle, he developed bladder cancer. In fact, I wrote Mel's obituary for the Neuropsychopharmacology Journal. He died about a year and a half ago in Texas. He retired there to play golf. His son is a professional golfer, teaches golf at the course in Austin.

TB: So Mel was replaced by?

DG: Bill Swanson came along. But, Dave Mielke took over. Bill Davis is the other person who's extremely helpful. Dave ended up doing more of the evaluations in Jackson; I was doing less, having become involved in the alcohol, drug abuse and poverty areas while also teaching medical students.

TB: When you say you got involved in the poverty area, what do you mean? What did you do?

DG: It was a free clinic I started in the Fisher project, working with children in the Treme' area, getting those with the high absentee rate to school. The Fisher project work went on for about four or five years and we provided a free general medical service there. When they ended up getting a medical clinic established we were able to leave. I felt we had done our job and now they had a State clinic that could take over the medical responsibility for the area. I had seen about two or three thousand patients in the Veterans Administration program and over ten thousand in the State program, over the course of about 30 years. I'd been the only the psychiatrist in the Southern state program in Substance Abuse and I was the only doctor for awhile, until we got an internist to help out in the clinic. But, I had no physician on my unit at the hospital in Mandeville, except myself, part-time, so, we saw this tremendous number of patients that I had the opportunity to treat and get involved with. I'm always running into them

wherever I go. Without naming restaurants, there are one or two I go to, where about one-third of the staff were patients of mine at one time or another. It's a very interesting experience, to say the least. I have a cute story to tell you. This involves Dave Mielke and me. Dave and I, while we were working together, running the psychopharmacology research unit, would occasionally have dinner at Antoine's Restaurant in New Orleans. Also, when we had guests in town, we sometimes would go there. Sometimes Dave would take a guest; sometimes I would take a guest, sort of share the responsibilities. One waiter knew us very, very well, and had been a patient of mine, in the alcohol and drug unit. And, he'd done well. He had been sober now for about ten years or so, and working at Antoine's for about forty-five or fifty years. One Monday, we found he'd had a stroke and they put him in Tulane Medical Center, so Dave and I went to visit him. I thought he was comatose and I stuck him with a pin but he didn't respond and his reflexes were very hypoactive. I thought, oh, my God, this doesn't look good. Dave was standing on one side of the bed and I was standing on the other when I said to Dave, "Gee, who are we going to get as a waiter tonight?" Suddenly, now this is the truth, this fellow sat up and said, "What are you talking about. I'm still your waiter." It must have been an ischemic episode, not a stroke. So, I've had some fun with patients like that. Want another good story?

TB: Yes, please.

DG: OK, this is a true story. It doesn't sound true, but I had this bipolar patient, a shoe salesman who was an alcoholic. This was at an ACNP meeting in the seventies. I had a paper to present at one of the group meetings on the day after I arrived by plane. My wife was going to come with me. The day we were supposed to leave, our kitchen burns down from a grease fire, so she had to stay behind to take care of the insurance, but I had to go because I had this paper to present. When I was in the hotel, taking a shower that night the phone rings. My wife gets on the phone. She says, "Where are the insurance papers? I can't find them." So, I got out of the shower thinking, about where the insurance papers are; the operator gets on the phone and she says, Dr. Gallant, you have an emergency phone call from the States. It's one of your patients. So, my wife, being a good doctor's wife, said, "I'll hang up and call me back later." So, she hangs up. This bipolar patient, who relapsed, is drinking and he's drunk. He says, in his slurred speech, "Dr. Gallant how can I thank you? You've cured me and I've been able to sell three thousand

pairs of shoes this week." I got so upset, so aggravated that I hung up the phone. I had a bar of soap with me and I flushed it down the toilet instead of putting it back in the shower, I was so turned around. Sometimes, these situations are not that funny at the time; there's nothing worse than a bipolar patient who relapses on alcohol. They can drive you nuts, unbelievable. If you live long enough; you've had a good number of unusual as well as humorous experiences.

TB: You served on many committees during your career.

DG: The FDA Psychopharmacology Drug Advisory Committee was an excellent learning experience. One of our former residents, Linda Kessle asked me if I would join their committee. She's now in private practice in Washington, and her daughter came to be one of my Tulane medical students. That's when you know you're getting old! Being on the FDA committee was really educational. I had one time when I really got stuck, beside that aprazolam incident. I was one of the advisors serving as the Chairman of that meeting that day to evaluate LSD to be approved by FDA as an investigational drug. Without mentioning any names a former ECDEU person brought patients from his alcoholism unit in Maryland. And he had them parade before our committee, each one who had taken LSD, raving about how wonderful it was and how the drug cured them of their alcoholism. I had to sit there and moderate this meeting. He had no control data, no objective evaluations, and he wanted us to consider it as an investigational drug for controlled research. That was a learning experience, but it wasn't a very happy one. It was a day long session that was totally a waste. There's also another story I can tell you about LSD when I almost got thrown in jail. This took place here in New Orleans. There was a fellow in the French Quarter who was caught with about 3,000 tablets of LSD on him and about $5,000 in cash. He claimed that he was a spiritual leader for a religion called, the "League for Spiritual Discovery" using LSD. That, believe it or not, made it a constitutional case, due to religion. So, it ended up in the Federal courtroom, which is directly across the street from our alcoholism clinic. At that time our alcohol and drug clinic was located in the middle of the French Quarter, near the Royal Orleans Hotel. The Federal Courts were located directly across the street from us. The assistant district attorney, a woman, whom I dislike to this day, called me up and wanted me to testify, because we had done some research with LSD, which I didn't mention to you. I'll go back a little bit. We had what was called, a "head clinic" in New Orleans. This was during the

“hippie days” and the Vietnam War. The kids were transients. So we had a “head clinic,” which was a free clinic in the French Quarter. We had our medical students working there to treat the kids’ medical and drug problems. So, we had a good supply of kids who had taken LSD to do research with. Roberto Guerrero-Figueroa, and I took 40 kids who had experimented with LSD 20 times or more. One group had flashbacks. It was two to three months after they stopped taking LSD, as far as we could tell. The other group had no flashbacks at all. Their use of other drugs seemed to be about the same, so the only difference between the two groups was that one had flashbacks and one didn't. We did all-night sleep EEG's on our research unit at Mandeville and Roberto, who was an excellent electro-physiologist, did the all night EEG readings. We didn't have computers set up then, so this was a time consuming experiment. In the end, we saw a much higher incidence of temporal lobe spiking from the scalp in the kids who had flashbacks, compared to those that didn't. It was about 60% vs. 15%, something like that. We published the data in one of the EEG laboratory journals. Anyway, this assistant federal DA found out about it and wanted me to testify in the case of the “League for Spiritual Discovery.” So, I said, "Look, I can't do it right now. I have to go to Washington." Probably, it was an ECDEU meeting. I said, "We'll meet with you, now, in our office. I'll give you a couple of hours and tell you all I know about LSD.” She wanted to prove LSD caused fetal abnormalities. I couldn't give her that data, because we didn't have any. I did tell her about the flashbacks on our EEG data and I spent a couple of hours with her. When I reached Washington, my wife calls me on the phone and she says, "You have a subpoena to be in Federal Court on Wednesday." I said, "Wednesday, after I spent time with her. She promised I wouldn't have to go to court and Wednesday afternoon is my married couples group therapy at the clinic and I have two groups to run from 1:00 till about 5:30 or quarter till 6:00." So, I said, "You know, I'm going to ignore the subpoena, because I gave her the time." So, that Wednesday afternoon, I was in the clinic. My 14-year-old daughter wanted to sit with me in married couples group therapy to see what it was like, because she was thinking about becoming a psychiatric social worker. I asked the patients if it was alright and they said, fine. She was really a sharp kid. So she was sitting with me in the married-couples therapy and handling herself fairly comfortably. In the middle of group therapy, my secretary gets a phone call from the Federal Court, across the street, saying Dr. Gallant is supposed to be in the courtroom right now to testify in this case. The court case is written up in these red brochures, US Court of Appeals for the Fifth Circuit No. 72-2464. I said to the secretary, "No, you tell

them that I've got this group and that I gave the deposition." She said, "The judge says he's going to send six Federal marshals across the street and carry you into the courtroom." I said, "Come on, now, you're kidding," hung up and went back to doing group therapy. About 20 minutes later, my secretary calls me again, she says, "They said if you're not there in five minutes, they're coming over here." I said, "Now, you're pulling my leg; don't joke around; come on, they're not coming here." Five minutes later, these six big hulking Federal marshals, and they were big, came in and picked me up, physically. My daughter is yelling, "Put my daddy down," and they carried me across the street to the Federal courtroom to testify in this case. I said, "Look, put me down, please, I've got to write some prescriptions for my patients." Some of them came from Lafayette, Louisiana. They did me put down and I wrote some prescriptions and, then, they escorted me, three on each side, across the street, my daughter following behind, yelling, "Let my daddy go." I walk into the courtroom. There are 50 hippies, probably hadn't bathed in five years. The courtroom was very smelly. They're sitting on the floor. They refused to sit on the benches, because that would be recognizing the Federal government. The spiritual leader, with this big long, red beard and shaggy red hair, was sitting with his attorney and the judge is yelling at me, not at the kids sitting on the floor, for not being there on time. When I try to explain that I gave a deposition, he wouldn't listen to a word I said. He said, "You just answer the questions. One more word out of you, I'll throw you in jail." OK. I'm sitting there testifying. Every time I testified with something that may be good for the defense, these kids started clapping. Every time I testified something negative, they'd boo me. The judge is banging away. It's like a B movie, comedy. The judge is banging away on the desk. My daughter is making faces at the judge for going after daddy, and I'm sitting there scared that I'm going to get thrown in jail. I've had so many things like that happen over the years. If you live long enough, you see a lot! Anyway, he didn't throw me in jail. The assistant DA won the case, but I never forgave her; she wrote it up in such a way that she doesn't mention my testimony as far as what happened in the courtroom. I was trying to tell the judge how she lied and betrayed my confidence in her, and the time that I gave her.

TB: Did you do any other research with LSD?

DG: That was the only time. I didn't work with LSD but I knew those who did. Heath and Russ Monroe worked with LSD when I was a medical student. Some government agency was concerned, in 1952 or 1953. They were concerned about the Chinese Communists brainwashing our prisoners of war and they had Ewen Cameron of Canada evaluating LSD, without informed consent, by the way. Do you remember that?

TB: I was working with Cameron on that project.

DG: Heath worked with a person down here, evaluating LSD and Russ Monroe did some of that work. They were also doing it without real informed consent, because that was part of the idea. The whole thing was crazy. Part of the idea was you're not supposed to know you're getting LSD if you're going to be brainwashed. So, they were only told they were getting some type of new drug, but there was no informed consent to speak of. This girl in my class volunteered for it. She ended up flipping out and had to be hospitalized for a couple of days. That was a terrible situation as far as LSD was concerned.

TB: During those years, people used LSD in the treatment of alcoholics. Did you use LSD in treatment?

DG: No, no. After I oversaw this FDA episode with the patient's parading in front of me, that they were into a religion with LSD, I thought this would not make for a good double-blind controlled study.

TB: Let us recapitulate briefly some of the events in your life. You moved to Tulane when you were eighteen.

DG: Seventeen going on eighteen.

TB: Started studying physics. Got a BS, right?

DG: I got a BS. in physics, and, actually, at that point, I applied to medical school and was accepted. They thought my physics major was a little bit unusual. They liked to take people who had biology and chemistry, and they were leaning to accepting applicants with majors in chemistry, biology and psychology but not physics. But I was a made a member of Sigma Pi Sigma, the Honorary Physics Society. I think that helped get into Tulane; otherwise, I don't know if they would have evaluated my application in a favorable way, because they were really very hesitant to take Physics majors. They felt that physics majors were not that interested in medicine, per se. That was a feeling I had and I was concerned when I applied, but it worked out okay.

TB: Your first paper was on dextroamphetamine if I remember well. When did you publish your last paper so far?

DG: This year. Well, not a paper, a chapter. Marc Galanter at NYU asked me to do this chapter on "Treatment of Substance Abuse Disorders" for the textbook that the APA puts out, and, then, Gabbard in Texas asked me to modify the chapter I did for this 2001 book on the APA *Treatment of Psychiatric Disorders*. So, I'm still, occasionally, writing.

TB: What else did you publish recently?

DG: That's about it. I presented on Dual Diagnosis and that kind of material at USC, George Simpson's place, but I do very little of that now, because I feel like I'm out of the research mainstream. I do keep up with the reading, so I'm okay for doing reviews, but I'm certainly not doing any clinical investigations.

TB: Did you do any research after you retired?

DG: In the VA Hospital, we were still doing various types of studies, for example the study on urine drug screens, immediate feedback vs. delayed feedback, as far as treatment results are concerned. We published that data. So, I did a little bit later on, but not much.

TB: We talked about some of your collaborators. You trained many people during those years. Would you like to mention some by name?

DG: Steve Paul wrote his first paper with my supervision.

TB: Was Steve Paul your resident?

DG: No, he was a medical student. Steve wrote his first paper when he was a Tulane medical student and he started residency in Chicago with Danny Freedman. He wrote two papers with us. He asked if he could try one of the atropine-like compounds in our schizophrenic population and I told him to do a protocol for review and go ahead. Then, Earl Usdin asked me to do a chapter on cardiac effects of various psychopharmacological compounds and I asked him, "Look, I've got this medical student, who is very, very sharp. How about his doing the paper and being the first author? Of course, he's just a medical student and if he gets a lead authorship, even on a chapter, it'd be a nice start for him." So Earl Usdin said okay. The first time I think they had said okay for a medical student to do a chapter in a book like that. I told Steve, "Look, why don't you write the chapter? You'll be lead author. I'll go over the paper with you to correct anything, but this is your baby and that's it." So, he did it, did a good job.

TB: Have you kept contact with him?

DG: I was best man at his wedding. It was just Steve, his bride and me, my wife, daughter and the rabbi. They got married here in New Orleans. And, then Chuck O'Brien was one of our residents. He got his MD and PhD in Pharmacology down here and then started a residency down here. Peter Rabins, head of Geriatric Psychiatry and Vice-Chair at Johns Hopkins also wrote his first paper with me when he was a medical student.

TB: Chuck was President of ACNP.

DG: Right, so Chuck, Peter Rabins and Steve Paul were three of our successes, not bad, very good. We had some other good people come out of here. I was just thinking of a person the other

day. One of them is on the faculty over at Minnesota. We had a couple of people ended up as academic successes.

TB: What would you consider your most important contribution to the field?

DG: I don't think I really made any significant contributions, when you really add things up. The things I hold as my contribution really didn't make any impact, like the idea of doxepin, in a controlled evaluation, being an anxiolytic back in 1969. I feel the times I served on committees, writing the Code of Principles, even though it's no longer applicable, was a very important thing. It was important for the ACNP to have one and important for me to do it; and it was very valuable at the time, particularly in being able to get the entire membership, with the exception of one member, to vote for it. That was a worthwhile accomplishment. Our controlled research with criminal alcoholics, first to report on trifluperidol, my teaching awards and the outstanding researchers I mentored. Steve Paul just wrote that I am on the "top of his list for making his career possible."

TB: Any other contribution you would like to mention?

DG: I think my contribution to all of my patients was probably more important in the long run, the idea of being accessible by home phone and cell phone throughout all these years. My main source of pride is having been always available to my patients twenty-four hours a day, seven days a week, even though it involved thousands and thousands of patients. When I think about anything important, I always think about that.

TB: I think those clinics in the poverty areas that you established were a major contribution.

DG: I felt good about that, although, there were some depressing episodes, you know. I mentioned the methadone patients, who didn't want to move out of the area, despite the fact that we would pay for their expenses. I think that the job we did with the children, taking them to school; things like that affected me more in the long run than some of the drug studies I did, which didn't take any great intelligence. Although, I should emphasize that we designed our own

protocols, did our own evaluations and statistics. The only thing the pharmaceutical firm did was give us the drug. Quite different at the present time!

TB: Have you ever been involved with geriatrics?

DG: Only in my getting old, as far as my own particular aging process is concerned. It's kind of funny, I haven't thought about this in years. I did do some, back in about 1974 or 1975. An internist in town started a small geriatric clinic on Tulane Avenue, right by the Broad Street Police Station, and he asked me to consult with him, which I did for one year, to help him get it started. I did do it for a year, a half a day a week, just to get things started; evaluating the patients as far as organicity was concerned. That's why I was so happy I started off with neurology, that gave me an appreciation for the organic aspects, not just the psychiatric model. After one year, I told him that was it. So, I did have that contact, which I'd forgotten.

TB: You received several awards and recognitions. You are a recipient of the Gold Achievement Award from the American Psychiatric Association.

DG: That was the best one, because it was 25 of work. We had to submit all the research data we accomplished on the unit. We had to submit the number of patients we'd treated, the way we treated them, the whole program. I even enclosed my card with my home phone number and my work number to show how we did it. The only time I ever cried at an academic award was during that particular presentation, because it was twenty-five years of work.

TB: Then you also received the Gloria P. Walsh award.

DG: That's a very nice award to get from the medical school because it’s for the entire medical school, not just the department of psychiatry and neurology, for teacher of the year.

TB: You received that for teaching.

DG: Right.

TB: You also had an award from the Association of Medical Educators. It was on substance abuse.

DG: Yeah, that also was a nice award, because the year before they gave it to Charles Leiber. Charles Leiber is a man I look up to tremendously, as far as research is concerned. He got the award one year and I got it the next year. So, in my acceptance speech, which they published in their journal, I talked more about Charles Leiber than myself.

TB: Then, you had the Robert Lancaster award.

DG: The Lancaster award had a lot to do with my community work. They recognized some of the clinical research but they had heard about the work I did in the poverty and alcohol and drug abuse areas with the State programs and so forth. So that was more of a community situation. That was gratifying.

TB: Then, for 15 years, every year, you had awards for outstanding teaching.

DG: That's for outstanding teaching within the particular year so it's not as nice as the Gloria P. Walsh award. But it's a nice award because they have a banquet, you get up and they applaud. Very rewarding, the students and residents sort of keep me on the young side, because I know what's going on with the younger generation. And, I know any teaching time I put in is appreciated, particularly nowadays, because of the way medical school problems have developed as far as not getting paid for teaching hours. Volunteers like me are more valuable than they were twenty or thirty years ago. In fact, I think we should end this interview with something Heinz Lehmann said. Before Heinz died, he was interviewed. I've forgot what journal it was; maybe it was mentioned in his obituary for the Neuropsychopharmacology Journal. He was working with New York State and involved in teaching until very recently. He said something like, I'm paraphrasing, not quoting; people over the age of 65 should pay to teach; not only should we teach for nothing but we should pay to teach. I agree, because I get much more out of teaching than the students or the residents.

TB: And these days you are reading a lot?

DG: I read anything that has a half way decent review. I use the *New York Review of Books* and the *New York Times* book section, the reader's section, and I listen to my friends, listen to George Simpson. He's turned me on to a few good books; I buy the books and, after I read them, I give them away. My wife reads them; I read them; we give them away, because I don't want to accumulate them. We pass them on to the residents or sometimes the medical students appreciate them. And, I read fiction and non-fiction. It doesn't matter. Books like *Regeneration,* Pat Barker's trilogy, are fascinating, a combination of psychiatry and a novelistic approach. In *Regeneration,* she talked about the poets, Siegfried Sassoon and Wilfred Owen, the greatest war poet who ever lived, in my opinion. They had a type of post-traumatic stress disorder and were treated by a psychologist who wrote the essentials up in *Lancet* in 1919. Pat Barker found out about this article and turned it into a novel. Fantastic, the best of the three books in the trilogy. I recommend it.

TB: What do you think about the progress made in the field?

DG: I think psychopharmacology is going in a beautiful direction. It's getting more and more specific with its drugs and they're hitting their targets much more accurately and the methods they have nowadays make the old time animal behavioral models looks so gross, it's pitiful. Yeah, I think it's fantastic.

TB: Are you pleased with developments on the clinical side?

DG: No, clinically, no. Of course, that goes for all of us, the HMO's, the PPO's, the whole thing is one big disgusting situation. I used to give out my home phone number to everyone. But, when a doctor gives out his phone number nowadays, the patients almost die from shock. Because of time restrictions psychiatrists who work at the hospitals, most of what they do is dispensing drugs. There's very little relating going on now between the patient and doctor and you can't afford it, financially. Let me tell you one story. Then I should let you go. One of our

outstanding residents, an excellent resident; he went to Seattle, Washington, after residency. He really enjoyed psychiatry. He really enjoyed people. He enjoyed relating, was very good with psychotherapy and psychiatric drugs and going to be an outstanding psychiatrist. Well, he didn't have much money, so he signed up to work in an HMO. The HMO decided he'll be, mainly, a drug dispenser. If the patient needs psychotherapy, he'll be seen by a social worker or a psychologist, which is less expensive. He had signed a three-year contract. For three years he was nothing but a medication dispenser. That kind of story really is terrible. In medicine, in general, things are not the way they were years ago. You do find many good doctors, but as a group, they don't have time to spend with their patients.

TB: What would you like to see happen in the future in the practice of medicine?

DG: I would like to see that physicians, in general, have the final say as to how the patient should be treated and not have so many administrators make medical decisions. I signed up with an Insurance evaluation advisory group to find out the way they assess how long a patient should stay in the hospital. This group works for companies that insure patients for hospitalization and so forth. The insurance companies use groups like these to say if the patient is kept in the hospital too long. I joined up, not to do the work, but I was interested in learning. I spent my own money, making them think I was interested in joining, and went to Madison, Wisconsin where this particular group has its headquarters. I sat in on their two day meeting and realized their purpose is never to go ahead and extend hospitalization for somebody with severe depression, who might need it, and has too many residual symptoms for discharge, but the whole purpose is to cut down the hospitalization. Some of the ways they did this were obvious. Although the patients required additional hospitalization, they found reasons to cut it down or not to approve the days. Having experienced that was a good experience, I learned a little. This is not the way medicine should go. To evaluate patient stay, first, there is the local person, who is not a physician, but a nurse practitioner or someone like that. Then, it goes to this peer review, which has never even seen the patient. They're in Madison or in LA, but they're not with the patient yet they're giving second opinions, if there's an appeal. That is totally unacceptable.

TB: Is there anything you would like to see in psychopharmacology and especially pharmacotherapy with psychotropics?

DG: Treatments are becoming more specific. Researchers are getting down to receptors and genotype structures. For example, with the benzodiazepines, they're down to the alpha sub units of the receptor. That seems to be more specific. In substance abuse, they are down to the behavioral trait of impulsivity associated with dopamine-D_2 deficiency receptors in the nucleus accumbens and even the possible IAI Taq gene for treatment choices. This is the type of research that will be extending to all areas of psychiatric illness.

More importantly, I would like to see the next presidential administration re-establish something along the ECDEU units with federal funding of independent academic, medical school institutions to do well-designed, reliable, honest, well-controlled drug studies with no impact or pressure from the pharmaceutical industry, whose only role should be innovating and supplying the new medications.

Most importantly, like all of us, I would like to see everyone have free access to good medical care if they are unable to afford it.

TB: I think on this note we should conclude this interview with Dr. Donald Gallant. . I would like to thank you Don for sharing all this information with us.

DG: You're welcome. It's been most enjoyable.

TB: Thank you.

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