PSYCHOPHARMACOTHERAPY
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I guess the easiest thing to begin with is you were the first person in this part of the world to use Chlorpromazine. Do you want to tell me how this came about?

I don't know whether I was the first one to use it but certainly the first one to publish on it and to do a systematic study on it in North America. It all happened because of a drug sales man. You know they make the rounds. I worked in a mental hospital in Montreal and they would come around and leave their literature. I was extremely busy at the time and didn't have much time to see the detail man, the Rhone-Poulenc salesman that was, so my secretary couldn't give him an appointment. So he said "I'll leave this literatures - two or three reprints - with him and it will be so good it will sell itself, I don't have to see him".

She gave me these reprints and she reported what he had said to me, which I thought was pretty arrogant and ridiculous and because of this I read it. I read it the following Sunday in the bathtub, where I do a lot of my reading, and it was very intriguing. It was in French. Now, we were in French Canada, in the province of Quebec; my wife is French Canadian and we speak French at home. So it was not very difficult for me to read it. Nowadays, of course, there isn't any difficulty, even the Anglophones learn French, but at that time Anglophones just wouldn't speak French. Anyway, I read it.

It was very strange, they made statements such as this is a sedative that produces something like a "chemical lobotomy" - somehow it was different from other sedatives. I really didn't believe this. In those days we had the barbiturates of course; they were the reigning sedatives. But we also had morphine and scopolamine injections for extreme agitation. We also had paraldehyde which was the cheapest and the most frequently used sedative. It smells awful, you could smell it when you got into a mental hospital. So I said, well its just another sedative and they are kind of dramatising it.

But it sounded somewhat different. The authors, Deniker and Delay, I didn't know anything about them but from their language and from the way the articles were written I realised they knew what they were talking about. So the next morning which was a Monday, the first resident I met was Dr Hanrahan and I asked him "do you want to start some research with me on a new drug?" and he said "yes". So we did it.

Now at the time, all we knew was this would be some sort of a new sedative. There was nothing specific about an antipsychotic or anti-schizophrenic action or anything like that. We decided we would try it out on about 70 or 75 patients. Nowadays, of course, this would take years but in those days it didn't take very long. We just chose 70, and we did them all, practically simultaneously, within one or two months. Also I didn't have to ask permission from the Director of the Hospital. I didn't have to get permission from the FDA or the Governmen. There were no ethical committees at the time, no guidelines, laws or regimentations. The only thing I had to ask myself was, was the thing reasonable, was it worthwhile and was it responsible? I don't remember - this was in 1953 - whether I even asked the patients. Certainly there was no such thing as informed consent at that time. I might have, but I don't think so. I just ordered it. I might have told the families if they visited. They were always very happy about anything being done because in those days you couldn't do anything for patients that would help them really.

So we did this and in the first two weeks there were two or three very peculiar events in some patients who were acutely psychotic with schizophrenia. We included schizophrenics, depressed patients and we also had some organic dementias; we didn't know who to give it to. We gave it for agitation, not for any nosological entity. And two or three of the acute schizophrenics became symptom free. Now I had never seen that before. I thought it was a fluke - something that would never happen again but anyway there they were. At the end of four or five weeks, there were a lot of symptom free patients. By this I mean that a lot of hallucinations, delusions and thought disorder had disappeared. In 1953 there just wasn't anything that ever produce something like this - a remission from schizophrenia in weeks.

Well, then, okay, so Hanrahan and I decided when we had about 75 patients treated for 4 - 6 weeks to write a paper. Something which is not often mentioned nowadays, but quite a few other investigators had found the same, there were quite a few depressed patients who got better too, quicker than they would ordinarily have done.

I should have said that before we gave chlorpromazine to all human subjects, the 70 plus patients, I wanted to see first of all whether it really was another kind of a sedative. So I asked for volunteers among our nurses for a research project. Nowadays its difficult to get research subjects - its almost a bad word, research, but in those days, Sputnik days, everybody wanted to be in on research, so quite a few nurses volunteered. What I did was give them Chlorpromazine one day, enough to make them quite sleepy

Roughly how much did you give them?

I think I gave them 5omgs - 75mgs orally which was quite a bit. And then a week later, they were given secobarbital, enough to make them sleepy to the same extent. Of course, we didn't expect it, but several of the nurses fainted from orthostatic hypotension. We were scared Hanrahan and I. We didn't know what it was, but since they very soon came to, we realised what it was. Anyway what we then did, before and after giving them Chlorpromazine, we gave them a series of tests, such as reaction time, and digit symbol substitution tests - now they would be called neuropsychological tests. What we saw was that all subjects would get equally sleepy on both drugs and sometimes fall asleep. The ones on the secobarbital were dopey; they didn't do very well on the tests, they could hardly understand what they were supposed to do. But the ones on the Chlorpromazine, once they were awake, they did as well and sometimes even better than they had done before on the tests. That was, of course, unheard of, unthinkable at the time.

There were therefore some indications that this was really an entirely different kind of sedative. So then we started on our patients. We started in May and by August we had written the paper and then we sent it to the publishers of one of the larger psychiatric journals.

Which Journal did you send it to?

The Archives of Neurology and Psychiatry. We were quite ambitious. We didn't hear a thing from August until January. Nowadays a six month wait wouldn't be so unusual but in those days it was, because there wasn't so much to be published. I finally thought there was something a little bit fishy, so I wrote to them and asked them to send me my paper back and I would send it to someone else. They didn't; they published it in the March issue. What

happened, or what I now deduce was probably the reason for this, was that the Americans wanted to have the scoop. Two months later somebody called Winkelman published on Chlorpromazine - but actually as an anti-anxiety agent. And then five or six months later Kinross-Wright in Texas published on its anti- psychotic effects. I think it was only because I pushed them asking them to send the manuscript back to me that they published it in March of 1954.

The first important papers in France had been published in 1952 by Deniker and Delay. Between August 1953 and March 1954, Rhone-Poulenc, the company had sent my manuscript around to quite a few people in the States. One of them was a friend of mine Henry Brill, he was the Director of a large hospital in New York State.

Pilgrim's ?

At Pilgrim's, yes. I think it was the biggest in the world at the time - some 10,000 patients or something. He knew me and trusted me and trusted the paper. On the basis of that alone I think he was the first one to use this new drug officially in the States. At that time, he also had an official position in the New York State Office of Mental Health, where I am now Deputy Commissioner of Research. He arranged on the basis of this paper to give the drug to a lot of patients at Pilgrims and based on the results he published papers on the reduction of restraint and seclusion and on how the admission rate remained the same but the discharge rate went up, and so on.

When the article was published in 1954, I think it was the first English language paper on chlorpromazine. Then there was another one, I wrote in German, which was published in Germany. So that is how it all started; it came about really because I married a French Canadian and we spoke French at home.

Interestingly while this was happening a Professor of psychiatry from Scotland visited the hospital and was introduced to me. I told him about the research I was doing. I was very enthusiastic because I had never heard of hallucinations disappearing in a few weeks with a pill and so I told him about it. He gave me a little pat and a patronising smile. I said "well you are close to it in Scotland, you should fly over to Paris and really look at it first hand" and he said "you know the French!".

Let me go back and just ask you why you ended up over here. You actually come from Germany?

I come from Berlin. I actually am one of these rare people who decided to go into psychiatry before they went into medicine. When I hear about that now, or meet such people, I am very suspicious. Usually they are a little weird. They may be very good but there aren't many of those. My father was a surgeon. He was, of course, aghast when I said I would go into psychiatry and he said "you don't know what you are doing but as you are going into medicine, there will be lots of time to change your mind". But I didn't.

Psychiatry interested me because I had had a depression as an adolescent and rather than leave school as my parents were advised for me to do, because I would never make it, they hired a tutor for me, a student, and he was very interested in psycho- analysis. He gave me

Freud's books and I had read all of what Freud had published at that time, when I was 14 or 15. This bibliotherapy might have helped. Anyway, for a year or two, I was not able to work at all and he had to do my homework for me. That was when I decided psychiatry was a very fascinating kind of thing. I had always wanted to become like my father, a physician, but I decided then to become a psychiatrist and stayed that way all the way through medical school.

You did your training in Germany before you came over.

Yes. In Germany the system was different; there were no medical schools like in the States, for instance. You went to University rather than into philosophy or jurisprudence or medicine. You just registered and in those days you would go from one University to another as often as your parents could afford to let you go. So I went to Freiburg to start with, because my father had studied there, and then I went to Marburg because Kretschmer was there. In those days Kretschmer and Schneider were the big stars in psychiatry.

What was he like ?

I don't remember very much. I mean Kretschmer was just one Professor and I did all kinds of other things as well. I learned Russian. I did a lot of philosophy. I remember more of Heidegger in the seminars discussing existentialism than I remember of my medical lectures. After Marburg, I went back to Freiburg and then to Vienna and I met Wagner-Jauregg there, the only psychiatrist who ever won the Nobel Prize.

Then, finally, I graduated in Berlin. On the way I had always taken extra lectures in psychiatry, which you could do because what you have to take wasn't prescribed like it is in a medical school today. There was a certain minimum that you had to have but then you could take extra lectures in whatever you wanted. So I did a lot of philosophy and psychiatry at the time as well.

So coming from that background, how did you interpret the effects of the drugs? People like Roland Kuhn who were psychotherapists at heart had tremendous problems it seems to me from what I have heard him say, just trying to work out how a drug could be helping a psychological disorder.

That was not difficult for me. While I had a philosophical and existentialist background, I never had many problems with the mind-body problem. Now, of course, you have a biopsychosocial model but I think this is just an aide memoire. I don't believe that anyone really can integrate things into one biopsychosocial concept. If people claim to do it they are either deluded or they are lying. I don't think they can do it. I think what you do is, anyway what I do, is to oscillate, and you have to learn to do that very quickly. I look at somebody completely biologically and a fraction of a second later completely psychologically and a fraction of a second later I look at his social environment, and so on. And that keeps going like in a film, until it seems to become a continuum - but it isn't really a continuum, at least I don't think it is as such.

So how did you explain the effects?

Well, before 1953 I had always thought of psychoses as something essentially different from anything else in psychiatry, because of the loss of contact with reality. I always thought that there must be a "centre" for hallucinations and delusions and I still think so. Seriously. Something that specifically keeps you in contact with reality and if that centre, or whatever function, in the brain, is disturbed and the physiology connected with it, you then begin to lose your contact with reality. Because contact with reality is such a solidly anchored function, no matter how sick you are with depression or with agoraphobia or obsessive compulsive or whatever, you may be extremely sick, you may be paralysed in your home for 10 years, you still are not psychotic. On the other hand, overnight you can suddenly become psychotic, that is lose contact with reality and become hallucinated or delusional or develop a formal thought disorder - the three hallmarks of psychosis.

Because of this I always thought that there must be something physical about the psychoses. I thought of the neuroses and personality disorders as different - they were intra-psychic but there was something extra-psychic in the psychoses. So I did all kinds of things to see if I could make a difference. I gave huge doses of caffeine. At one time I remember being intrigued by the polarity of the manic-depressives. I tried to change their metabolism by giving them ammonium chloride in large doses to make it more acid and at other times more alkaline and so on. None of this worked, of course. But I was always looking and hoping for some physical intervention that might make a difference and at the same time I did a lot of psychobehavioural tests, such as reaction times and so on.

Why did you do those?

Well, for one thing I was working in a mental hospital, during the war, when most of the staff had gone to war. I was an immigrant, a refugee from Germany, and I had my own difficulties with that. I had up to 600 patients alone; there were no residents or interns at the time and only one registered nurse to help me - otherwise untrained personnel. In order to keep up my morale I had to do some research and so I always went around with a little scratch pad and had patients draw on it or did association tests or something or other. I also felt that by doing things like after-image experiments or reaction times I might find some physical, neurophysiological function that was disturbed and would be correlated with hallucinations, for instance. Because an after-image - you know if you look at something bright red and you look at a white ceiling afterwards you see a green image - that's actually like an hallucination. Its a perception without an external stimulus. I did a lot of work with after-images and with critical flicker fusion thresholds.

I was always hoping and looking systematically for psychophysiological and neurophysiological correlates of psychotic disorders and doing all kinds of other things. In those days it was all trial and error. You produced huge skin blisters in order to do something to the reticulo-endothelial system and the immunological system. I injected oil of turpentine into the abdominal fascia - that produced a big sterile abscess, which you then had to open in the operating room. It also produced a great deal of leucocytosis and fever which you wanted. Patients, for a day or two, would get better - enough to keep on doing all this sort of thing.

There was a lot of trial and error going on in those days. The only one who had been somewhat successful was Klasi in Switzerland, with his continuous sleep therapy in the 20's. I did that too but that was both expensive, because you needed a lot of nursing, and risky because quite often the patients would develop pneumonia and we didn't have penicillin yet.

So that was a risky thing. But anyway I did all this constantly in order to keep my morale up while looking after 600 patients. So when finally a pill did work, you can imagine how I felt.

About a year before that or even just a few months before that - we used to take students in the hospital on their clerkships - and one student asked me one day, when he looked at the patients who were looking at the ceiling talking to their hallucinations, "will there ever be any kind of pill that could help these people?". I thought the question was rather ridiculous, but I was quite benign and patronisingly said "well, there would never be a pill but somehow eventually we might be able to help them". But a few months later we had a pill.

Even so I didn't quite believe it and it took almost 2 years until I really would talk fairly freely about "anti-psychotics". I had correspondence with colleagues in the United States, who worked with these drugs and none of us were really talking about antipsychotics - we couldn't believe that there would be something specifically antipsychotic. Some even said anti-schizophrenic, which I have never believed of course - anti- psychotic yes but anti-schizophenic no. I said in a talk to the Canadian Medical Association in the early days, I said it is almost like the antibiotics, one could almost call it anti- psychotic. I was very apologetic about it and made it clear I was talking only metaphorically. Although in our correspondence, we would say that chlorpromazine really did remove delusions and hallucinations, it took two years really until we were comfortable with this idea.

How did I explain what it was doing? Well, we didn't know anything about dopamine at the time. Dopamine, in fact, was not something that we considered to be important. It wasn't even a neurotransmitter. We knew about noradrenaline, and I still remember talking to a pharmacologist once who said "mark my words dopamine eventually will become very important". I thought that wasn't very likely because at the time it just seemed to be a precursor of noradrenaline.

So how did I explain the mechanism of action to myself? I have published on this once or twice. I thought it was the kind of a new sedative; that was something I had established experimentally. It was a new sedative, which did not destructively interfere with wakefulness or arousal. Patients might doze off, but once you had aroused them, you could immediately awaken them and they would be quite normal, not doped any more. I thought that clearly psychotic patients have a lot of trouble, they are terrified by their delusions, hallucinations, experiences, their psychotic anxiety which is different from neurotic anxiety, and if they could be given a sedative which would not interfere with their cognitive functioning, as this obviously didn't, as I had established experimentally, they would not have their disturbing agitation and panicky anxiety and their self-help potential, which is always there, their own healing power would have a chance to get through - if it was not held back by the anxiety and the emotional disturbance. So it was because it was a sedative that did not interfere with cognitive performance, it therefore allowed patients to cure themselves.

I still think there may be a good deal to this even now. I don't think it is just all neurotransmitters and receptors. In the same way I think that depression is a learned illness, and that maybe one of the reasons why antidepressants, which physiologically or pharmacologically should work very quickly don't - this may be because it takes 2 or 3 weeks before you unlearn what you have learnt in your feelings and your perspective of the world. Anyway so that's how I explained it to myself at the time - patients use their own recovery potential because they are sedated without being doped.

How did you view the side-effects that happened. This again must have been virgin territory seeing some of these side-effects for the first time. You can't have known what they meant.

No. At first I remembered the hospital having been visited in the late 1930's by Sakel, the inventor of insulin coma therapy. We were one of the first hospitals to use it. I slept in the same room with him once and he told me that when he developed this treatment a few years before in Vienna, he always had his passport under his pillow at night, because he didn't know what would happen, he might have to leave the country very fast because of a toxic fatality in one of his patients.

What was Sakel like?

I was just starting out as a very junior psychiatrist when I met him - in 1938 or 1939. I had the impression then that he was a bit flaky. He got his idea when he was treating heroin addicts with insulin in Berlin to help them over their withdrawal symptoms. I don't remember what his rationale was but it calmed them down. Once one of his addicts who was also schizophrenic, accidentally slipped into a hypoglycemic coma. Sakel was scared but brought him out of the coma quickly with an injection of glucose. To his amazement, the patient showed a considerable improvement of his schizophrenic symptoms. Sackel then wanted to use hypoglycemic coma as a treatment for schizophrenia. But they would not let him do it in Berlin, so he went to Vienna where they let him set up a clinical trial and he had some success.

When Hitler came, he accepted an invitation to the U.S. He lived in a hotel - I forget its name and it does not exist anymore - in New York City. I thought that he liked the good life and to feel important. He died fairly young - I think in New York City.

Where you surprised when insulin coma was shown not to work?

No. It was an utterly nonspecific therapy. A shock to the brain and to the whole organism - like banging a watch on the table to make it go again when it stopped. I never thought of it as a cure. It was a very risky, cumbersome and messy method of treatment. But it was the first and only game in town then.

So you felt a bit the same way as Sakel in terms of possible problems

Yes. Particularly since we didn't know how to dose Chlorpromazine. The French had gone up to as much as 300mgs or something like this but not much higher. I decided that we had to get some sort of a guideline. So we agreed that we would aim at making patients sleepy. But some people didn't get sleepy even though they got 600 or 700mgs. It was hard to know how high to go because I was never quite sure that the drug wasn't possibly quite toxic in a way that might take several months to become apparent.

This was at the time of Moruzzi and Magoun, so beside my psychological self-healing potential theory, I had the explanation that the drugs worked on the arousal system, the reticular ascending system. It had become clear that people's arousal could definitely be diminished

and re-activated again if they were stimulated so that they could function quite normally under the effects of the drug, until they were not stimulated any more. Given this, I wasn't surprised that they were just not very active and that they remained passive. I felt very much better seeing them passive in this way than seeing them the way they were with barbiturates or paraldhyde because I knew that if necessary they could play chess with me just as well and could beat me in chess although they were sitting there apparently completely passive.

I had a lot of personal interaction with patients at the time playing chess and cards and chatting with them. But then a few months after we started the treatment, a friend of mine, a neurologist, and myself, were both looking at some patients and they were walking like typical Parkinson patients and I said "by the looks of these, it looks like they have Parkinson's". He said "its not possible because there is no way of inducing Parkinsonism". It couldn't be but there it was. Anyway, we coined the term extrapyramidal symptoms, which hadn't been described before. So we wrote a paper on the extrapyramidal symptoms as side effects and how these effects looked just like Parkinson's - but again not daring to say something that was "impossible" at that time, when there was no pharmacological way known to produce Parkinsonism in humans.

Were you concerned that when patients began to walk this way that even when the drugs were halted that they would still remain Parkinsonian?

No. Of course we had tried to stop the drug and we found that in a week or so the patient would be alright. We didn't witness them develop tardive dyskinesia; that came very much later. So, we knew it was easily reversible and we also knew it occurred only in 20% or 30% of patients. We wondered how high we could go with our dosage, but then Kinross-Wright in Texas, like a typical American, went up to 2,000mgs or about that high.

That early?

Yes. The Europeans made fun of this as being so American, Texan even, - you know everything is bigger in Texas. We thought it was fairly high, also, but anyway he went up to over a 1,000mgs and I think up to 2,000 mg within a year or so.

Deniker came over to Canada then and visited us. We had had two or three cases of jaundice, which they hadn't had with chlorpromazine.

What was Deniker like?

I saw him last about a year ago at the 40th anniversary of chlorpromazine. I went over to Paris and saw him. He had a stroke. He is a fairly typical Frenchman; my wife is a French Canadian and he visited us several times. He was like a Frenchman - they are very sure of themselves; Parisians particularly - they tolerate everyone else, they are very nice to them, they are very polite - he was that type, a real Parisian intellectual. He wondered why we had - and he never had - any jaundice. I remember Hanrahan asking me if we really had to mention that there were three cases of jaundice - it wouldn't be very good in our first publication on the drug. I said look we've got to, but I haven't seen one since then. At that time I think Rhone-Poulenc probably had something or other in the drug that they have eliminated since then. Or what is

also possible is that there was a sub-clinical epidemic of hepatitis which hasn't been there since. In France they didn't have any cases of jaundice.

Nothing happened anywhere that caused people to stop and think perhaps we shouldn't go any further with this drug?

No. Look, you can't imagine. You know we saw the unthinkable - hallucinations and delusions eliminated by a pill! I suppose if people had been told well they'll die 2 years later they'd still have said it's worth it. It was so unthinkable and so new and so wonderful. There were all kinds of things happening. Chronic schizophrenics who had been divorced because they had been psychotic for 10 years, now all of a sudden they were symptom free and their husbands or their wives were married again. It was a very strange time.

Anyway, I began to see detail men from the pharmaceutical companies more often and I would tell them "now we can really do something very dramatically about psychotic symptoms, now its up to you to find something for manic-depressive disease" - because we were always fairly sure this was a physical thing, much surer than we were about schizophrenia. Anyway they came up in 1957 or so with the tricyclics - imipramine.

Strangely although neuroscientifically we didn't know what was happening with the antipsychotics, when the antidepressants appeared, it didn't take very long to find something out. By 1959 or so the effects of reserpine on neurotransmitters had been noted and hypotheses about antidepressants and neurotransmitters and reuptake and so on were appearing. So we began to have an understanding of how antidepressants worked but it took until about 1965 when Carlsson and Lindquist came up with their Dopamine theory for neuroleptics. So for more than 10 years I was working only with the kind of theory I had that the organism helps itself once it is freed from agitation.

In 1957 there was a meeting of the World Psychiatric Association in Zurich. I remember it. Jung, I think, was still there and one thing I remember was that for several days, until late at night, people, at the congress, would discuss existential psychiatry which only Europeans could understand. I flew back from there with the Chairman of the Department of Psychiatry at Toronto University. He had come from England.

Who was it?

Alwyn Stokes. He asked me, since we were sitting side by side, he said: "now look there was so much fuss about existentialism, everybody talked about it until late at night, what is this whole thing". So I thought, well here is a captive audience, he wants to hear about it, he is a Professor, he is obviously quite bright so I'll start. Then for about two hours I talked about it, Husserl and Heidegger and so on and so on. He listened carefully and very attentively and at the end he said "well the whole thing is really just a symphony of words isn't it?". So ever since I have given up trying to explain existentialism to anyone outside Europe, although South Americans take well to it. Indians from India take well to it too. I developed a whole theory actually and I gave a few talks on the question of why people who have had an Anglo-Saxon education until the age of 12 or 13, will never be able to understand existentialism unless, like anthropologists, for years they do nothing else but study it and immerse themselves in it. And that is because the English, you know, they had the Magna Carta; they had the celebration of

commonsense with Locke and Hume, they had the tremendous scientific breakthrough at the same time as they had Locke and Hume - at the same time, they had Harvey discovering the circulation which must have been like our nuclear developments. Anyway because of this and because of their moral and political maturity, anyone who had an Anglo-Saxon eduction would be commonsensable and would view people who would say that suffering is good for something as just ridiculous. My son went to an Irish Anglophone school where we lived until he was about 11 or 12, then he went to a French High School and College then he went to McGill finally and graduated in medicine and I don't think he could understand it.

When I wasn't speaking to this Professor about existentialism on the plane, I read what I had brought with me and that was a paper by Kuhn on Imipramine. He had given a paper there with about 12 or 14 people in the audience. I wasn't there. But I did get (in German), the Schweizer Medizinische Wochenshrift, where he had published it, and I read it there. Immediately when I arrived back in Montreal I asked Geigy, I phoned them and said I would like to have some of this Imipramine stuff. They said what's that? I said well its an antidepressant apparently, you have worked on it for years, but people at Geigy in Canada had never heard of it. They were quite embarrassed. But, within a week, I had the stuff and we did the first study of antidepressants of the tricyclic type in North America, I think - Nathan Kline had already done the first trial of the monoamine oxidase inhibitors.

How did the first trial go?

Fine. There again, they had said about 65% - 70% would respond. We opted to inject it - it was injectable at the time. We didn't quite know whether that really helped. What we did establish was that if you went over 300mgs it didn't help any more; you just got more side-effects. We also tried the MAOIs and we unfortunately had one fatality, that was with iproniazid. So for a while we laid-off those but kept on with the tricyclics. Because of all that, I was kind of character cast as as a psychopharmacologist; we kept on working with the various derivatives of the antidepressants and the anti-psychotics - Tom Ban joined me then and we did a lot of this work. But that still didn't really change my philosophy - to me drugs are only adjuncts, very helpful practical adjuncts, but psychiatry is not psychopharmacology. I don't think it ever will be.

Please tell me more.

Well psychiatry is a medical specialty. Now all medical specialties are there to help sick people to get better. It so happens that in most of the other medical specialties, there is a great deal of scientific background, which is quite solid, evidence based. But I really don't think that internists or surgeons or whatever are great scientists. They use what the scientists produce for them. They have to understand a little, just like somebody who is a pilot of an aeroplane, for instance - he has to know quite a bit about the plane but he doesn't have to know very much about aerodynamics or how to build a plane or how to fix it even, but he has to know how to fly - and flying is quite different. Somebody who is the world's expert in aerodynamics - I wouldn't want to fly with him. Now a psychiatrist ought to be somebody who can use whatever is available to help his patients, but what is most important, as far as I am concerned, is the contact-intensive training that you have to have. You have to have 1,000's of hours of contact with patients, regardless of how much molecular biology is behind it when you finally give them a pill. Nor do you have to know too much about the molecular biology of genetics, when you

have to tell relatives of patients when they come for consultation but you have to know enough about it to know what to say.

I have often wondered whether it was a good thing that I was instrumental in getting the drugs into psychiatry because now people are increasingly using DSM.111-R, or things like that, as a laundry list and psychopharmacology as a cook book. I actually know some colleagues to whom I used to send patients but I don't do it any more, because doing the laundry list they are no longer using much of their empathy - they think it is not very scientific. So they aren't as good.

I still think that you have to help as a psychiatrist, not as a neuroscientist. You have to help individual patients and to help individual patients you have to know when to smile and when not to smile, what kind of tone to use and what not to and so on. That comes only after thousands of hours of contact with patients. Now what our residents do is they get, I think, much more than they can digest of neuroscience. They don't have Phd's, which they really would need to understand molecular biology nowadays. I have a hell of a time of just keeping abreast of the headlines, you know, and that's having done it all along. How anyone new coming into it can understand it without being, as I say, a Phd, I don't know. But they are being taught this, hours and hours and hours. There are also still many many hours of psychodynamics and the theories behind that and the supervision of longterm psychotherapy. Now all these many, many hours take away the time for contact with patients.

I never had postgraduate training - it was during the war and there was no way of getting away because they didn't have enough staff to let me go anywhere. But I think I got the best training by just from 8.30 in the morning, until midnight, I was making my rounds and seeing hundreds of patients for many, many hours everyday over many years. So I learned the kind of idiosyncratic, individualised flexibility that you have to get through, I suppose, empathy and the expertise you get through the experience of being able to relate to individuals rather than to statistical numbers or biological facts.

Now to me a psychiatrist ought to be the ideal mixture of a science-minded physician; still a healer. Lets say a painting has been discovered somewhere in an attic and it's supposed to be a Rembrandt. Somebody says, how would they ever establish it? Eventually it goes to one or two of the experts in the museums. They don't get the answer from books - anyone can get from books what you can learn about the infrared and the X-ray qualities of the paints and so on and so on - but they have seen thousands of paintings and therefore they know whether it is or isn't. That sort of thing just has to come through personal contact and that to me is still the most important thing because the rest you can learn fairly quickly. I can teach somebody basic psychopharmacology in two weeks, if he's really well motivated. And my students, who have never seen a psychiatric patient before, within a week of looking at DSM III.R for the first time, they argue with me about diagnoses. So that can be taught very quickly too.

The fine tuning of it takes years and that has been short-changed now because of our progress in the neurosciences. We clinicians have been responsible for all the new treatments from the moral treatments of the 19th century, which was very effective, to psychoanalysis and then the unspecific shock treatments and then the psychopharmacological treatments, all of this came from the clinicians. Finally we asked the neuroscientists to help us to find out how they work. They did and eventually after 10 years they came up with some explanations. Now they are going ahead, intoxicated by their own successes but its research for research-sake, its not research for psychiatry any more.

I am as excited as everybody else looking at these sexy pictures of the thinking brain - when you think of lifting a finger, this lights up and then that lights up, you know. You can even see what happens in the brains of the obsessive compulsives when they follow their compulsions or obsessions or what ever; but you don't need to do this because you could see it before in their behaviour. It doesn't tell us any more. Anyway, my fear now is that with that tremendous and really exciting progress, we will get away from the patients and become therefore less competent psychiatrists but great prescribers of MRIs and SPECTs.

Let me take you back, after introducing Chlorpromazine, the first series of meetings began and you went to the Val-de-Grace meeting in Paris. What was that like?

That was two years later in 1955. I don't recall very much of it except it was a very great celebration. Everybody was very happy. You know what I remember most was flying over from Montreal; Rhone-Poulenc paid for it. In those days, it was before the big jets, you still had real beds. You could really lie down on a bed in a plane. There was a thunderstorm outside and I was lying on the bed and falling asleep and it was just wonderful. I remember a festive atmosphere at the meeting but they weren't going overboard.

I've just come from a luncheon now of previous organisers and past-presidents of the International College and Lewis Judd was saying how well everything was going ahead, and Neurobiology is our basic science now and in another 10 years we'll have done this or that. Well it wasn't that way then. It was very much like having won the lottery, I suppose, I never did but I think I would be that way if I had suddenly won $500,000 in a lottery. I would be very happy I wouldn't think of the future or anything else. I would just be very happy about it and talk about it a lot. So that's what we did. We didn't think, like we always do now, of the future and what's coming next and what's the cutting edge, or this sort of thing. That frantic kind of thing which I think is counter-productive, we didn't have it then. We were happy and said Okay. Now we have much more to work with, lets go on working without making projections of what we should be doing.

Who were the people who stood out for you at that meeting?

Obviously Deniker and Delay because they had called it and they had laid the foundation for our euphoria.

What was Delay like; he has a reputation as a card-playing, novel-writing, flamboyant person.

He was patronising really. You were happy if you were allowed to talk to him, kind of. Pierre Deniker was much more down to earth although still Parisian. Delay, for instance, became a member of the French Academy and all his students had to heavily contribute to the jewel-encrusted sword that he got. This sort of thing.

Pichot was there as well.

Pichot is very nice. He is also down to earth. You can talk with him and he is concerned with what one has to be concerned with. And he has a good sense of humour, not particularly French. You know I come from Quebec and we aren't very happy about the French; the French let us down in the time of Louis the 15th, with their "who are these people, its just a lot of desert with snow in it; we won't send any soldiers". Voltaire even celebrated Wolfe's victory as the beginning of the liberation of all America. So that's how Wolfe and the British could get us and now you know all the troubles we have about autonomy and so on. Politically today it is just all horrible. Anyway we were talking about the French, Pichot is in that respect not very French. The way we look at it in Quebec, Churchill wasn't very English - he was not like an Oxford Englishman. Pichot is not like a Sorbonne Frenchman.

One of the other early figures was Wolfgang Da Boor. You called him later to find out why he later lost interest in psychopharmacology - you also reviewed his book on psychopharmacology, which you saw very much as a watershed book?

Yes, the interesting thing about it was that when he wrote that book, we actually already had Chlorpromazine but he was not particularly enthusiastic about it. It was a watershed without him knowing it. He kept saying in the book, there isn't really any possibility of having a physical foundation of mental illness. At the same time, he was looking at the physical effects of drugs on dimensions of mental functioning. But he wasn't thinking of actually curing anyone. In the whole book, there isn't anything about curing or even about being significantly therapeutic with any of the drugs. All he proposed we do was to study the phenomenology of the brain being affected by these drugs.

When Da Boor vanished, everybody had the impression that there might have been a clash of personalities between himself and Rothlin

He wasn't easy to get on with, Rothlin

In an article in this book Thirty Years CINP, Pierre Deniker suggests that at the meeting when Rothlin was proposed as the first President of CINP, there was silence because he had been hostile to the idea of CINP in the first place and then he emerged as the first president.

You know in the early days I wasn't there. I wasn't invited and Freyhan and I, we were quite angry because we hadn't been invited although we.....

Why do you suppose that was?

I don't know. For one thing Ewan Cameron, who was a very important person at the time, Scottish American but teaching at McGill University - he had all kinds of scandalous troubles afterwards with the CIA - well, anyway, he and I we were both in Montreal. He was up the hill in the University Clinic and I was in the mental hospital. He didn't like mental hospitals. And I, since I was 5 years old had aspired to become a Professor eventually but there didn't seem to be much chance for it where I was. I had asked him for a job at the Allan Institute where he was but he made some excuse and didn't give me a job. He may have been a little jealous.

Chlorpromazine had worked out for me while he was struggling with all kinds of other things. He wasn't much of a scientist. Not a good researcher but very ambitious. I think he was a good clinician. He was a tremendous administrator and at the time one of the world's outstanding psychiatrists. So I think it may have been personal that he didn't want me to get in there. He was one of the movers in the first meeting.

Anything else you can tell me about Cameron - you're probably sick and tired being asked about the man

He was tall - most of his medical staff were too and I am still convinced that one of the reasons he did not accept me at the Allan, when I once asked him for a job there, was that I was not tall enough. I was angry and frustrated then and actually made the tacky 'decision' that when the mountain wouldn't come to Mahomet, Mahomet would have to come to the mountain, meaning that I would have to outprestige the Allan and him, through my work at the little mental hospital on which he looked down so much. That, in 1950, seemed to be as likely as the David and Goliath story. Of course, I was jealous of Cameron but he became jealous of me a few years later.

As a person, he impressed as rather cold, aloof, distant, dry and patronising - he called everybody 'Doc' but nobody dared to call him that or by his first name - not much of a sense of humour in short. Rather arrogant. His general rule was never to leave a meeting without having spoken there publicly at least once. I did not like that he thought that accepting the French-Canadian language and culture was unnecessary and expensive. However, he made it a point to devalue politically accepted issues - and that sometimes impressed me favourable. For instance, he publicly ridiculed the concept of 'nice girls' and was scolded in the newspapers for that by the Anglican bishop.

What did you think of his methods of depatterning and psychic driving

I thought they were original - but ludicrously simplistic. You know in those days, I never thought that what he did was not ethical! I was one of those who thought that Cameron was a very ambitious but incompetent researcher. However, I am still convinced that he was a fair man, of good moral integrity and primarily motivated by clinical concern for his patients - at least consciously. My wife and I never liked him but I would always stand up and defend his integrity. Quite recently, I have heard that he was involved with the CIA. The information is quite credible but I am still convinced of his integrity as a clinician.

There seem to have been problems for the first few years of the CINP - people like Frank Ayd, who had been there at the start all of a sudden found that they weren't listed as members any more.

Without knowing why! Well, you know it was international and as I told them today at the luncheon, before I came here, I was the oldest past-president there and they asked me to reminisce. I couldn't remember very much of it except that to me it was amusing what went on before I was on the Councils. Who should become president? And in those days, iron curtain and so on, political considerations were very important. The French were too arrogant, the English didn't do this or that, the Americans wanted to do it all, the Germans - oh for heavens

no don't lets get them involved and the Italians were not quite right. So eventually it had to be Canada because, as usual, that's the role we have been playing successfully in the world - being accepted as nice people. And we are more tolerant than everybody else. We don't have any nationalism to speak of because we haven't much in the way to be particularly proud of. That's why I like it very much and I'm afraid of what's going to happen now with Quebec nationalism. But because we didn't have nationalism and heroism, that's why they wanted me in at the time.

After CINP began there seemed to be a point around 1960/62 where it could have fallen apart.

I didn't even realise it at the time, although later I became president. I was kind of drafted into this but I had never been very much interested in politics and I let things pass. It amused me to see this - for me - ridiculous puttering around of who should get what and why and so on. I did realise it wasn't very easy what was going on there. But it could have fallen apart you say?

Yes, it seems around 60 to 62, things weren't good. Denber and Rothlin didn't get on.

That was very true. They almost had fist fights. I never was interested enough in the why's and wherefore's. Denber was a little tough and Rothlin was very insensitive. So I think it was almost purely personal in an international thing like that, which was troubling. Later, the issues had to do with the various nationalities but in the early stages it was mostly personal.

You also were involved in the founding of the ACNP.

No. There again they drafted me into it. Several people told me I had to come along and I said I have too much to do, I can't get bothered with another college, another meeting and so on. But they said well, you know what drafting means, we draft you. So that's how I got into it. But again a lot of it was due to the fact that I was the - what do they call these things the opposite of a catalyst? - something that holds things down, this is what being a Canadian is. Like graphite rods in a nuclear reactor, to slow things down. This was a very hectic thing to start in the States and they wanted a Canadian in to temper it down.

It was quite a powerful group of people, quite a few of them ended up in court on opposite sides of the fence like Saunders and Kline. If ever a group of people looked like it needed a few graphite rods in there....

Nathan Kline was one of my best friends. I went to some of the court meetings with Saunders. He was not always easy to get along with - a very determined man. Brill didn't like Nate because he was a bit of a clown but that's why I liked him. But he was very determined and very powerful. He had all kinds of political connections. I had never been interested in and never really been impressed by the political importance of anything. To me only persons matter. But the personal interactions and difficulties were always very interesting and if somebody wanted me to get in to make peace or to keep the passions down - Okay for that reason I would always be available. I certainly never wanted power and I still think it is an ugly or dangerous thing to have power.

Do you recall much of the two day ACNP foundation meeting?

No. Only I still have the photograph of a very long dinner table. No, not of the meeting. The Council meetings I do remember. I don't think anything earthshaking ever happens at meetings. At least that's my perception.

I am thinking more in terms of the people. The two people who seemed to have talked the most at this meeting were Frank Ayd and Bernard Brodie.

Frank Ayd, of course, had a legal training; that's why we wanted him always to be there because that was very important from the beginning. You know he was not very much liked by some of the more orthodox neuroscientists. But on the other hand, and I think this is much more important, we wanted him because he is a pragmatic fellow who is very bright and has legal training. Now Brodie, he was also important then. Incidentally I became president of the American College of Neuro-psychopharmacology almost by default.

When was that?

It must have been 1966/67. What happened was that I was president elect. Brodie was the president but he didn't show up in San Juan where the meeting was and nobody knew where he was. He hadn't said he wouldn't come. I think there were rumours that he wasn't well. Something had to be done right away so I had to become president overnight, not being prepared for it and not knowing much about how to run meetings and all this sort of thing. I chaired the meetings with people telling me all the time what to do and that was my presidency. From then on Brodie disappeared altogether.

In 1960, though, he was still very much the commanding figure. Axelrod worked in his lab, Costa worked in his lab.

Carlsson too worked in his lab. But Brodie just disappeared really. We didn't know and as I said he wouldn't let anyone know either that he wouldn't show up. He was very much there and all of a sudden.....

ACNP began life as a largely clinical grouping. People who were giving these drugs for the first time and wanting to share what they were doing and what was happening and what was the best way to actually look at the new compounds etc. Its moved a long way from that now almost to the point where clinical people feel excluded and someone like Don Klein has gone and formed an American Society for Clinical Psychopharmacology. How do you read all of that? Is it as you say the neuroscientists have gone into research for research's sake and they have lost sight of the goal.

I really see them as charging ahead, intoxicated by their own successes and forgetting completely their roots, where they started and why they started. Okay, fine for them but then we must question their role with psychiatry. Today at the luncheon, Lewis Judd was very proud that this was the first time we arranged a CINP meeting 50/50 between basic scientists

and clinicians. He said well I don't know whether we took risks but then he said there were 2,500 people at Eric Kandell's lecture so he said "okay, we know that neuroscience is the thing everybody wants to go to. Well yes everybody want's to go there but what is it going to do to psychiatry?

I think that everybody would want to go and hear Kandell but not for the neuroscience necessarily but because of who he is and to be able to say they had heard a lecture from someone who is probably going to win a Nobel prize.

That's it. Kandell is an excellent presenter. But you see how you can misunderstand the significance of this 2,500. So now Judd thinks that proves it, so now they will have 50/50 and then it will become 70/30 and so on. A year and a half ago in San Juan, we had a meeting with previous presidents of the ACNP, on just this problem. They said that they would mend their ways and get more clinical but Don Klein who was there wasn't very convinced apparently and soon afterwards he started his own group. But I think even that group, if it is called psychopharmacology, will have to go into molecular science and so on. It means it will go away from psychiatry.

Its not politically correct to even say that today. So I don't know what is going to happen. Recently, I think in the Lancet, there was an editorial suggesting that we dissect psychiatry into neuroscience and psychosocial. I don't know where I would go - I'm not neuroscience and I'm not psychosocial. The psychosocial problem is that they don't want any drugs.

Aubrey Lewis apparently at the first CINP meeting said that if we had a choice between the new drugs and the social treatments, such as industrial rehabilitation units, that have been introduced we would pick social treatments. And its interesting that in the UK under Lewis' influence the Maudsley remained very aloof from the new drugs and in the UK psychopharmacology is something that has happened outside Oxford, Cambridge and the Maudsley.

That's not so good either. Somehow we ought to get some sort of an understanding of how to integrate it. You hear people talking about the death of psychiatry and perhaps there will be a death. There will be psychologists and, you know, some clinical psychologists can do psychotherapy as well or better than some psychiatrists. And then, psychopharmacologists and neuroscientists they are not physicians at all. Its very strange almost paradoxical that the more progress we make in psychiatry the more we seem to be heading to our own destruction. I'm quite pessimistic about it. Although I am optimistic about the possibility of helping mentally sick people much more nowadays.

That also came up today at the luncheon. Paul Jannsen asked whether there was anything that we know now better than 10 or 20 years ago. I said yes we know we don't have to give such high doses, low doses will do. The discussion then developed around the table and the others said well we understand so much more about the brain's pathophysiology and neurophysiology - but that's not psychiatry. The fact that we can explain more is not understanding. The understanding part is the personal part, the interpersonal part, and that isn't even seen.

Can I take you back and explore a further issue. Talking to someone like Frank Ayd, when the new drugs were introduced in the US at least people who advocated drug treatment were seen as being in league with Satan - this was the wrong way to treat mental illness. The analysts held the field. I have the impression from the reaction down here was probably a little more vehement than it was up North in Canada.

That's true of everything but actually its a strange thing I know more psychiatrists here and I am much more in touch with American psychiatry than I am with Canadian although I was Chairman at McGill in 1970. You see we had three paradigms in the 19th century. In the first phase, psychiatry couldn't develop before Pinel because the philosophy was that the whole cosmos was a clockwork, it was all material and therefore there probably wasn't any soul or psyche or whatever. But even if there is it couldn't possibly be sick, so to speak of a mental disorder was philosophical nonsense - it was not logical. Then Pinel, who at the time of the French Revolution, was a courageous young activist and a great philanthropist, he said to hell with all the philosophy, as far as I am concerned I want to get these people out of the dungeons. That's how it started. Psychiatry was philanthropy not science or philosophy, not even clinical. He wanted to get them out of the dungeons.

Then he and Esquirol wrote the first text books on psychiatry and within the 19th century, the three paradigms developed; first the psychosocial with its emphasis on checks and balances of a moral kind. Then Griesinger around 1850 put forward the idea that there are no mental diseases, these are only brain diseases - this was the organic paradigm. And then finally with Kraepelin the, what I call, the agnostic paradigm - "I don't care whether its mind or whether its organic - its clinical". And, of course, DSM III.R is also agnostic - its operational and a-theoretical and so on.

Now the psychosocial bedfellows of Heinroth from the nineteenth century are the behaviourists, Freud and the anti-psychiatrists - they are all shaped by the psychosocial model. And the Griesinger model was picked up by Meynert, Leonhard and Kleist and so on, and then, of course, also the neurosciences. The agnostic one is DSM III and IV. Then there is what I call the integrative imperative - its not really a paradigm - Engels bio-psychosocial model. I don't think anyone can think of all this together, but anyway.

So then in the 20th century, particularly here in the States between 1930 and 1950, there was an absolute reign of the psychosocial model. Everything else had disappeared and you just were anachronistic and simplistic and you just didn't know anything if you thought that there might be physical causes or a physical substrate or that anything physical could ever help - I mean that was seen as ridiculous. You just wouldn't do that. It was politically incorrect for anyone who had academic aspirations. I had those but I wasn't very close to academia in those days, so I could carry on my own work.

This was never the case in Europe, in Germany, for instance; they were much more temperate. As you say, though, in England there are almost the two parties still. I hope somehow psychiatry can be saved by having both and saying okay the neurosciences are there to help us to find the tools for diagnosis and the tools for treatment but the treatment itself is not neuroscience.

But in terms of the reaction though to the introduction of the drugs during the 1950' and 1960's, it seems clear that even people like Nathan Kline and Roland Kuhn, even as they introduced the drugs were still thinking very much in analytic terms.

Yes, it was so but not for very long. I think Kline became much more absolute about drugs doing everything eventually. As I said, I myself thought of the potential self-help powers of the organism and freeing it rather than doing something physically to it. But, yes, in the States, it became hostile almost. That was very understandable because the psychoanalysts had reigned for two decades without anyone even in the shadows threatening them. All of a sudden they were threatened. Their livelihood was threatened. Their academic reputation, their whole ideology, everything. And of course they fought a rearguard action. But they gave up fairly gracefully within 3 or 4 years or so. It was interesting to see this rearguard action because they were threatened; they were completely surprised - completely. It took them about a year before they began to believe it and another 2 or 3 years before they could accept it.

Which years were these?

Actually after our paper in '54. Brill by '55 had already shown how the rate of the inmates in the mental hospitals went down, how seclusional restraints went down and so on and so on. So by '57 I think, they began to really become convinced.

It took 20 years after that though for US Psychiatry to change to a more as you would say a more agnostic condition.

Yes, that's because for the Americans it was almost a status symbol that you have your analyst and it is still to a certain extent.

Well you can take Prozac now. This book actually, Listening to Prozac, it does mark at a street level a change in culture. Whether it is a good change or not is another issue

That's true. But still if you go to Hollywood, the well known writers, they all have to have their own analyst. And in a way this is what Freud really wanted, I think - an educational, guiding kind of technique, not a therapeutic one. So that also influenced people who were not definite psychoanalysts, orthodoxly trained; they still had private practices and then they had to face the fact that private and solo practising would disappear gradually and there would be teams and so on, it was very difficult to take for most psychiatrists because their whole culture had been one of private office practice.

Can I jump and put it to you that before World War II, indeed from the turn of the century until 1950, German psychiatry was world psychiatry but since 1950 German influence has been almost minimal. Why?

I was sitting beside Hans Hippius at the luncheon today. He said that after World War II, there were two stars of psychiatry from the generation born before 1920 and they were De Boor and Matussek - there were two brothers Matussek, one is a psychopharmacologist the other one

was the star, he was an analyst. But neither of them became really anything great. De Boor disappeared and Matussek remained as an analyst but didn't get very far, for political reasons probably. So the leading teachers before them had been Kurt Schneider and Kretschmer but apparently that was all the psychiatry there was after them it only gradually built up around Hippius and so on, later on.

Is there a sense in which the psychotropic drugs have led since the 1950's to an Anglo-American psychiatric culture. Perhaps because these formulations have been drug friendly, they just happened to fit what the drugs can offer.

What also stopped Germany was the complete economic collapse after the War. The universities had to be rebuilt according to American models. No, the Germans accepted drugs. I think German psychiatry has always been more integrated than probably any other psychiatry in the world. They were tolerant to both.

But where Germany clearly led the field before the war, they haven't since.

No they haven't since. Well now you know there is the American money. You can't produce PET scans without a lot of money and that's what is leading us on now or luring us on. The PET scans and the large trials which are very expensive. The drug companies at first paid for it and then Government grants, NIMH and so on. I think that's where America has stolen the edge

Did the fact that so many people emigrated from Germany because of the war take the intellectual wind out of German sails.

It took away the intellectual impetus, yes, and the self-assuredness. German medicine was an impressive thing and there is no doubt about it. When we all went away, I don't think that this tradition was transferred to America but it was abolished in Germany.

I just read the biography of Einstein, the latest one that came out, and it is interesting that when he finally came to Princeton, he had got a telegram before he left Germany in 1933 asking him to enter the US very quietly and not give any interviews to anyone. Because the FBI and the CIA didn't like him at all. He was very suspect and the more interviews he gave to journals and newspapers the more suspect he became. So when he finally got him into Princeton, it had to be done very discreetly and against the opposition of people like Planck, for instance, in Germany. They stood on their heads to prevent him leaving. I think Planck talked personally to Hitler. But I don't think this brain drain transferred things so much as it did stop an impetus and it also was a blow to their self-assurance.

You wrote a fascinating piece in Thirty Years CIMP, looking back at the Prague meeting about how things have changed. What you say is that we couldn't have foreseen, how much would have changed so quickly of PET scans, chronobiology all sorts things that weren't just there. On the other hand as you say when it comes to the actual practice of psychiatry very little has changed.

That's what Paul Janssen implied today, I think. Its funny he is not a clinician but he meant it and it seemed that none of the other clinicians there understood what he meant. Not much has changed in practice. We know how to do it faster and a little better but the modus of doing it really hasn't changed.

Can I ask you about some of the people, who have been involved in the past 30 or 40 years people like Freedman, Klerman, Kline etc etc. Who have been the key people do you think that have helped shape the period if one can say that anyone has been important enough.

Well Kline was very important politically. He had a great deal of expertise and instinct and skill and he actually went to the Congress and after he had talked to them, giving a very dramatic and I think very exaggerated view of what was going to happen, Congress gave a lot of money - it was almost forced upon psychopharmacology. Now I was in the study section of the NIMH for 10 years, starting in '56 or so with the psychopharmacology service centre with Jonathan Cole. He was a very important person. He was very young at the time and I still don't know why they picked him, but he did well in heading the psychopharmacology section. In the early days we had two or three days meetings and there weren't enough grant applications. We met three times a year, I think, and at the end of one of these sessions, I think there was about a million dollars left over, which was a lot of money in those days. We didn't know what to do with it. We said what can we do with it, is there anyone who wants seed money? How can we give the money away? This sounds absurd today, bizarre. But eventually I think we had to give half of it back and for half of it we just practically forced it down peoples throats. I had never asked for a grant but they phoned me and asked would I please apply for a grant - you will get it but you have to apply. Which again is bizarre as now only 3% of people are getting grants.

So we got a lot of money and we got Congress convinced, almost single handedly by Nate Kline, convinced that we now could cure mental diseases and really have a handle on it. Then there was the Camelot Times with Kennedy, Felix was the NIMH Director then and he unfortunately was over enthusiastic. They created the CMHC, the community mental health centres. They were thinking of prevention and treating the worried well so that they wouldn't get seriously ill. Not thinking at all of where the emphasis now is on the seriously ill and rehabilitation - that's why we have people on the streets, homeless and so on. Felix, himself, a very enthusiastic fellow, was convinced, not like Aubrey Lewis at all, that it was just a question of time before we could close all mental hospitals - now that we had a handle on it with the drugs and these new community centres. That's it, so forget about it; its no longer a problem. Well it was a bit too much and that's why we now have the de-institutionalisation problems.

Now Freedman was an editorial restraining influence. He made the Archives the journal. Who else, Klerman, we were good friends. He was in charge of what was it called - ADAMHA or something anyway even over-ruling the NIMH - and he felt very strongly, and rightly I would think, that what was necessary here was to establish, if at all possible, the efficacy of psycho-therapy. That was a very clever idea but not political, and still isn't political and therefore I don't think he ever got very far. Too bad. He went to Cornell. His wife is the star now, Myrna Weissman. Elkes, has anyone mentioned him to you?

I have a range of views about him, some saying that he was important but important by virtue of his charm and enthusiasm rather than because of anything tangible actually done.

His rhetoric. Yes he is a very good speaker. That's it. I don't think he influences people because of anything he has written or really done very much, but wherever he goes and talks people are very much taken by him. He has known, particularly in England of course, but globally also most of the pharmacologists and psychopharmacologists. If you can, you should really try to interview him. If you had asked me who to pick I would have picked Joel Elkes

What about people from Europe Paul Jannsen?

Yes, he is a very bright fellow. He always hits the right note somehow. He was the first one, of course, who had Haloperidol and who got away from the phenothiazines. I asked him several times about this.. some sort of instinct - one of his young ladies, technicians, had found something in mice which intrigued him. He got on to it and that's how haloperidol came about and now again they have Risperidone. But he has always impressed me as somebody who is really interested in how do the drugs work clinically, which is very nice for somebody who is a pharmacologist and so successful. He has not been lured away. I think he knows a lot about psychopharmacology.

Are you at all concerned about the fact that we don't seem to have any new drugs.

Well we do have the atypical ones now like Clozapine which we had since 1965 but didn't really know we had. I suppose there are a lot on the shelves. My feeling has been, this is again not very politically correct and perhaps not even right, but somehow I feel if we would only stop a while and try to make the best of what we have. We don't even know all the drugs that we have on the shelves. The Benzodiazepines were on the shelves for 20 years or so before we discovered their use. We knew Clozapine worked and we knew that it didn't cause extrapyramidal symptoms but we didn't know that it was better than the other neuroleptics. We should have done what Kane did eventually to show that it was better than the other anti-psychotics. We could have done that in 1970.

What I see in the future of psychiatry are two things, one will be that there will not be any breakthrough with any new drugs but a breakthrough, through the media, in public education - something like the way the media brought about the sexual revolution in the 60's. So there will be a mental health revolution sometime in the year 2000, or whatever, how to treat and how not to treat children and this sort of thing. And if we could do that, I think we could probably do away with about 25 to 30% of serious mental disorders. If we just knew how to bring up children. But that has to be done in every household not just in an Institute. Genes do a lot but their expression depends greatly on the environment in the developmental stages. Only the media can do that and it might take 50-100 years once it starts.

The other is, because of my age I am particularly interested in successful aging. I have a notion that we can't do anything about neurons that are gone irreversibly but we can probably do a lot more than we are doing now preventing them from going. Now we have done amazing things in preventive medicine with reducing smoking, exercise, high fibre, low fat diet and

vitamins to the point where strokes have gone down and heart disease has gone down people live much longer. I think we can do the same probably with mental health or with aging if we had a different value system.

Again that comes back to the media. Our value system now is such that even two and three year olds are learning about money and power. You have to be successful, you have to be competitive, you have to beat the others and money and power are the manifestations of that success. So when people get to be 65 and they can't get much more money or much more power but they lose some of it, they are completely devastated. What we now need is a new psychotherapy for the generation from 65 to 95. Before this, there weren't any in this group apart from a few who lived to be 80 and so there was no specific psychotherapy for them. The first thing this geropsychotherapy would have to do is to dismantle these life long primary values of money and power and replace them with autonomy, creativity, knowledge, learning - what have you. All kinds of other real values. That would take 2 or 3 years before you could do anything else psychotherapeutically.

But what I immediately would like to see is what we can do about preventing stress. I think there is a lot of what I call latent stress. People are seldom fully aware of being stressed. For instance in the elderly - today when I came up on the street here I met a black fellow, a worker, I don't know what he was but he was wearing work clothes and he smiled at me, greeted me saying "Hi young fellow". Now, ridiculous as it sounds, it gave me a boost. On the other hand to say to yourself, well I'm over the hill and what can you expect, is a constant latent stress unless you actually can counteract it.

Now we know through the work of McEwen and others, that stress, not only in the elderly, increases corticosteroid hormone output and this produces a cascade of excitatory amino acids. In young people, this can be cut off because there is enough homeostasis but not in the elderly. They don't cut it off and they have shown in rats and in primates as well, that this cascade definitely produces atrophy of the hippocampal cells. That we can't afford. So we have to avoid stress by all means. Elderly people have usually a higher corticosteroid level than younger people. I think it is because they are constantly more stressed and they don't realise it. So we have to discover these latent stresses and then see how to counteract them and in that way prevent the loss of neurones, which can be shown to lead to loss of cognitive function.

We learnt about stress from Selye but what we don't know yet is where stress becomes distress. Stress is not only tolerable but it is necessary to activate us, but distress becomes immediately destructive, particularly if it is chronic, and more so if it is latent because then we don't even know it is there. So we have to do something about chronic stress, finding out what it does if anything, what the latent stresses are and how to counteract them. So I'm looking towards prevention rather than cure. As regards treatment, I think we have probably enough on the shelves to serve us for some time if we learn how to use it.

REFERENCES

Lehmann H E, Hanrahan G E (1954). Chlorpromazine, new inhibiting agent for psychomotor excitment and manic states. Archives of Neurology and Psychiatry 71, 227-237.

Lehmann H E, Kahn C H, de Verteuil R L (1958). Canadian Psychiatric Association Journal 3, 155.

Lehmann H E (1980). Reflections on a career in psychiatry. The Canadian Psychiatric Association Bulletin 12, 4, 14-16.

Lehmann H E (1986). The future of psychiatry: Progress - mutation - or self-destruct? Canadian Journal of Psychiatry 31, 362-367.

Lehmann H E (1993). Before they called it psychopharmacology. Neuropsychopharmacology 8, 291-303.