RADICAL BIOLOGICAL PSYCHIATRY
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Can we start with your contacts with Krebs and Gjessing?

My contact with Krebs to begin with was merely as a medical student who was very enthused by biochemistry and the excitement of Krebs. The psychiatric connection came through Gjessing. Rolv Gjessing, was very famous then but is hardly known now. He came from Norway to see Krebs and he took Krebs to begin with to Middlewood Hospital, the local 2-3 thousand bed mental hospital to see, of course, that there were more patients in beds there than in all the rest of the hospitals put together in that period. He tried to encourage Krebs to study mental illness. Krebs obviously wasn't going to do that because he was still trying decide whether succinic acid fitted in the tricarboxcylic acid cycle or not and he was doing this work with yeast. But they both got the Bishop to complain about the bad conditions of patients of that period and Krebs became enamoured by the scientific attitude of Gjessing, who he thought was taking the sort of clear cut scientific attitude you should take. Krebs was totally opposed to using statistics in science. He said they were used following bad experiments which demonstrated very little. Gjessing, of course, was selecting patients very carefully and showing he could cure people who had long-term periodic catatonia of a clearly defined type - this was very exciting then - by giving them massive doses of Thyroxine. This is almost forgotten now but I can assure you that it worked. He gave them Thyroxine in doses rising daily from 1 to 10 milligrammes. I thought it was like giving people straight Electro-Convulsive Therapy. The pulse rates went up to 180 and everyone in the place was nervous about whether they were going to survive.

Now we know there were a lot of other reasons why they may have got better but at the time Krebs believed, and I certainly believed, that there was a group of patients who went into nitrogen retention periodically and they had a feedback system which caused the nitrogen retention. This was the toxin which was causing their schizophrenia. When it got to a certain level there was a compensatory mechanism, that wasn't working properly, that then poured the nitrogen toxin out and so they got better. At the time thyroid extracts or later thyroxin were the likely first choices to wash nitrogenous products out of the system. Sir Charles Harrington who was to become my supervisor from the Medical Research Council had only just synthesised thyroxin. The voracious reader and polymath Gjessing contacted him as well as Krebs. He was in touch with almost every aspect of science. He, for example, developed a method of presenting meteorological data which was used by the Norwegian meteorological office. His motive was to see if the weather could be shown to influence the course of his patients illnesses. He concluded that it did marginally do so. Gjessing argued and everybody in the world accepted it then, that he had demonstrated that a nitrogen containing toxin must be causally involved in at least some sorts of schizophrenia. Believers included Sir Aubrey Lewis, who was perhaps the world's biggest critic of everything, as you will see in the psychiatric chapter in Price's Text Book of Medicine in which he wrote that the explanation of one type of schizophrenia was now known.

That type of schizophrenia disappeared but also the theory he used had a mistake, which I think I can claim I showed. He claimed that because the phase relationship was different for patients, that is the relation in time between the nitrogen retention and the psychosis, that it could not be a simple relationship secondary to the mental state or the activity of the patient. He failed to notice, which is actually very simple mathematics, that two sine waves put together can produce any phase relationship you like. The phase relationship depends only on the relative amplitude of the two sinusoidal factors ($a \sin wt + b \cos wt = A \cos(wt+d)$ and then $\tan d = b/a$). The mathematics of it are schoolboy work in a way. One of the factors was that lying immobile in a catatonic state, which some of these patients did, led to muscle wasting and so there was nitrogen loss. In addition, they were on an absolutely constant diet, so another factor that could cause nitrogen loss was when they were exercising to levels for which their carbohydrate and fat intake were alone inadequate. Then they had to catabolise proteins and they got into negative nitrogen balance. The two factors were necessarily out of phase if not necessarily in the terms of the illustrative formula - nineties degrees out of phase. The relative height of the two factors produced the phase relationship. I did not realise this point until after translating and editing Gjessing's major work "The Somatiology of Periodic Catatonia." The translation was performed with Leiv Gjessing, Rolv's son and to a great extent by Miss Marshall Sir Aubrey Lewis' very able secretary. She seemed even as polyglottic as did he and he certainly encouraged this endeavour.

This realisation did seem to severely damage the central biochemical argument. However, that doesn't explain why the massive doses of Thyroxin worked nor does it explain why, in that period, Gjessing was able to collect so many periodic catatonics. They were rare patients which is what Krebs liked about it - he liked him showing something clearly in individuals something clearly - but if you look at Kraepelin's "Lehrbuch der Psychiatrie" periodic catatonia at that time accounted for 13% of the psychotic patients. My theory became that it was the ECT like effect of the inordinate doses of Thyroxin which was helping, but that doesn't explain why the condition has disappeared?

The enthusiastic medical student's admiration for Krebs was a part of my background. He, it seemed to me, was showing the way in which everything would be explained, and of course the mind was the real thing. His support of Gjessing was a stimulating introduction to psychiatry. But I had to do all sorts of other things first, for example to qualify before I could get involved in the study of periodic psychoses. During my house jobs I actually got involved in what was to be the first machine to automatically and repeatedly monitor diastolic blood pressure. It did not work very well but it led me to join the research group of Professor David Smyth, a close colleague and supporter of Krebs. We worked on biological transport mechanisms, and at the time aldosterone was fairly newly discovered. I suspected wrongly that it could play a role in manic depressive and other periodic psychotic illensses.

The feed back circuit in which I was mistakenly placing it would also explain the sodium and water retention described in the literature of the period expecially by Coppen and Shaw. At that time Mayer-Gross in Birmingham was very interested in this issue. John Crammer, who became editor of the British Journal of Psychiatry, had also published some papers in the area. It was thought that manic depressive illness was due to the retention of sodium. Coppen and Shaw of course wrote famous papers in that period on residual sodium but the truth is that they were achieving levels of precision which were almost beyond what the technique could actually do I did a study for the Medical Research Council on this and I had to report my failure to replicate their work to Black, the great expert in Manchester on Body Spaces. I remember going there in trepidation to tell him I couldn't repeat the work and he said to me if I had repeated it that he didn't think he would have believed me. Which was quite interesting really.

Gjessing thought that periodic catatonia was a combination of manic depressive illness and schizophrenia. His book ends by saying what it really is, of course, only the Gods know but he goes on to try to show in genetic studies and family studies that there is very strong evidence of manic depressive as well as schizophrenic histories in the families. I tried to repeat the work but that type of patient disappeared by the time I got very involved in the problem. I think that what happened in the mental hospital was part of the story. I use the analogy of a violin string. If you pull it tight then it has a certain frequency to which it vibrates but the tension is due to you outside. I wrote a paper in a Japanese book which goes into that theory and that sort of explanation. Krebs who had so encouraged me, continued to be interested and we met very intermittently in Oxford, Sheffield and Germany where he would so kindly enquire about my scientific struggles. I particularly recall him saying "In science you must hope, as I did, to have luck".

The situtation in Norway at the time was rather odd wasn't it with patients having to travel down thousands of kilometres to hospital in Oslo

Yes Gjessing's hopital, Dikemark Sykehus in Asker, was just outside Oslo. It was peculiar that both of the two main Norwegian hospitals were related to Oslo in terms of geography but one served Oslo and the other served the rest of the country. I don't think there was any other significant mental hospital in Norway. It had quite a small population. Patients from the north of the country were sometimes brought down in the guard's van of trains, a thousand mile journey, in a drunken stupor.

You must as I have said also remember that a large amount of this research was done during the Nazi occupation of Norway. Gjessing was not exactly a leader of the resistance but he was a very brave man. They were encouraged to kill the demented people. Gjessing refused to do this. But in order to do his studies he was meticulously scientific, unbelievable really, and he wanted the patients on fluid diets for months. If they didn't take the diet one day, he put a tube down them and gave it to them. Something ethical committees would not exactly allow you to do now. But everybody worshipped him because he was the ardent worker. It meant however that these patients were subject to what we would see now as the most stringent conditions in the big mental hospital. They had what I would call the Scandinavian pseudoinformality about the place. Everyone talked about how informal they were but everybody obeyed the hierarchy and there were very clear rules on that. Nevertheless, it was a very impressive place. Some of the patients were artists and some were playing Beethoven sonatas at night. One was corresponding with Pastermak, in Russia, by writing in English and exchanging Shakespearean types of acrostic poetry. The place was beautiful and enchanted - in the winter covered with snow and the moonlight shining on it in the long nights. All periodically awakened and covered with flowers in spring. The experience was indescribable for a young Anglo-Saxon from Sheffield.

Can you tell me anything about his personal background?
Well he worked with the Laplanders in the North of Norway and wrote a study of the anthropology of the Lapland people. He then became the Director of this mental hospital in Dikemark - a move south. The climate was an issue - as you get older the North of Norway in those days was a pretty powerful factor. He bought the equipment for the laboratory. When he retired, people wanted to know where it was and he said "I've taken it". He'd paid for it. His son, Leiv, took over actually in the end.

It has been said that he wrote incomprehensibly and this is the reason his work languished
Oh no that's not true at all. I mean firstly he did write in German and that's why it was translated. I couldn't have translated it from Norwegian. But he was accepted - the good example is Price's Textbook and I think you'll find his work in Henderson and Gillespie's Textbook - they were the textbooks of the period. He never got a Nobel prize but he must have been pretty near it. He got a number of recognitions. I think what really went wrong was that firstly he did a great deal of work during the war. It was partly possible to do it during the war because there wasn't much to eat anyway so his meticulously made-up fluid diet was perhaps more acceptable to the people - they were being looked after after all. Then of course the war ended in 1945 and everything changed in 52, when the phenothiazines came along. I think the phenothiazines disrupt periodic catatonias but it would be very difficult to show that now.

Gjessing had the idea that if you could predict when a patient was going to become psychotic and when he was going to become well, and if you could demonstrate that there was no sociological factors for that same periodicity, then there must be a biological clock which would account for the periodicity. He used the analogy of a laboratory pipette washer - it fills up with water and then it syphons out. Once you knew what the clock was, you could trace back mechanisms and so explain the disorder. Of course the whole idea of a biological clock and biological rhythms was fairly primitive then but it had a long history. I don't think he knew about "circadian" rhythms, that came later with Halberg and other writers but that was the whole idea and these were the model cases. If we could only understand what was going on here we would get a hold on the biological input to psychosis. This kind of patient was precisely God's gift to science. Gjessing was a very cultured well read man and he didn't rule out sociological factors - he saw all these factors as interactive - but he wanted to study the ones which could be studied very precisely and he felt what a scientist had to do was to exclude a lot of other sociological factors which obscured the work. It was that sort of view. I was a young chap when I went there. Sir Aubrey Lewis encouraged me to go as well as Krebs.

How did the links between Krebs and Gjessing come about

Oh Krebs was of course thrilled by the fact - and it is remarkable that before Krebs got a Nobel Prize, when he was working on intermediate metabolism - this psychiatrist recognised that this had a fantastic future in terms of cerebral metabolism. Gjessing came to Krebs saying "Can you with your obviously great steps forward in biochemistry help me in this work?". He brought all the papers and graphs. There was no question he impressed Krebs enormously. Krebs thought he was the psychiatrist who was going to make the real breakthrough. I think they corresponded for quite a while after that.

I knew Leiv Gjessing, his son very well. He took over afterwards. The old man had died before we did the translation of his work which Leiv and I did together. Rolv Gjessing's English wasn't good. He did publish one English paper in the Journal of Mental Science but he'd studied in Budapest and he could only really write in German or Danish/Norwegian. He was anti-German at the end of the war because of the occupation and he didn't want his book published in German even though it was written in German. But it was translated too late. The whole incident is still an interesting phenomenon to understand how it happened. I can swear blindly that the patients were as mad as you make them. Some of them when they had the Thyroxine in these enormous doses did well, not all of them, but some of them did get fantastically well. So the story looked very impressive.

Krebs himself got interested in juvenile delinquency when he was an older man. He went to Oxford and got interested in semi-psychiatric problems. He was half-Jewish and he reverted to a more strictly Jewish life. He was very concerned about what was happening to the youth of the country and so on. Not his most creative moment I would have thought. I think he did keep in touch with Gjessing, and he remembered me when we met, even at a meeting in Berlin very long after he had retired..

What can you tell me about Krebs?

Krebs was a medic who studied in Freiburg. I think his mother was Jewish, anyhow he had to escape from Germany and he came to England. At that time he had produced the Krebs-Hesselheit solution to to replace Ringer's solution. I think he had already worked out the Urea cycle in kidney and he was halfway to working out the tricarboxcylic acid cycle. He went to Cambridge but I think the truth is Cambridge didn't want him - he was a bit too distinguished for the people in Cambridge. Rudolf Peters, also a German, was the Professor in Cambridge and they obviously didn't get on very well. Krebs was given very humble facilities, despite already having done some very distinguished work. To their enormous credit, two people who were in Sheffield, the Professor of Pharmacology, Ernest Wayne, who later went to Glasgow and became Sir Ernest Wayne, and a chap called David Smyth who got an FRS and with whom I also worked and who had some inkling of this. They thought that Cambridge was treating a very distinguished scientist poorly.

In those days, Sheffield had very little to offer but they both gave up a great deal to make room for this chap. I was only junior when it happened so I am not certain but it seemed that Smyth in particular was willing to be a sort of lackey for someone he recognised as a greater scientist although Smyth himself was a man who could speak Russian, German, French and Spanish. He got a first in Crufts for breeding dogs, was the Professor of Physiology and got an FRS as well as played the piano, so he wasn't an undistinguished friend of Krebs. Anyway Krebs said he would help Smyth, who was interested in the way in which ions were transported across the body. He suggested to Smyth that he should study the impact of what he, Krebs, was doing with the tricarboxcyclic acid cycle on the whole animal. Unfortunately, what Krebs had done was to study yeast and it was infinitely easier to do it with yeast. Smyth's part just couldn't work - you couldn't do it at that stage, there weren't the techniques. The problem Krebs had, of course, was to be sure that what he worked out in yeast really was working in human beings and other animals. Anyway that's how they started. They were also incredibly good friends. I did a PhD with Smyth on the transport of inhibitors into the bile and the intestine. This kept me in touch - so I was the junior guy looking at the big guys from below.

Krebs, after he got the Nobel Prize, was offered all sorts of things by Oxford and Sheffield could no longer compete. I think he was ambivalent about moving. He had married a very nice local woman and his son is now an FRS in animal behaviour. And he recognised that Sheffield had done him a very considerable service but I suppose they couldn't really compete with what Oxford could offer him so. But he had already done his most important work here and in Freiburg before coming to England..

And were Oxford able to compete with an offer of La Scala?
To begin with, I think this is true, I was certainly told it, he had an income of only £50 a year to do this work. He bought drain pipes and cut up his own blotting paper to do the paper chromatography. So although Smyth and Wayne were obviously doing what they could for him, it wasn't that much. But later there was a cinema across the road from the university which has since been knocked down called La Scala and they did give him La Scala. He worked part of the time on the balcony of the cinema. It had a sloping floor so the Warburg's had to have longer legs on one side than the other. Of course, because he was up there he could see what other people were doing down in the stalls in the rest of the laboratory. Its quite an amazing story really. He was a bit of a tyrant - that's not the right word because there was something very nice about him but you couldn't work with him and muck around. You had to have the graphs and everything ready the next morning and an explanation of why it didn't work. The famous stories are that he would see the technician leaving about 2 o'clock in the morning when they were doing excellent experiments, he would say are you going already or something like this.

It was also true that his wife, when he went abroad giving lectures, used to come to the lab and ask whether they had heard from him which was a bit unpleasant. Gjessing was the same. Both of them were so ruthlessly involved in their research that everything else came second - of course that was more acceptable then than it would be now. You know they give the presents on Christmas Eve in Norway and Gjessing's wife once told me she phoned him to say that the children were all waiting and he said why? Its Christmas Eve, she said. She told me this after he died when I went to see her. These women paid a big price for these great men really. You didn't need to be a great psychiatrist to see what she was saying really. She was a very charming women and embarrassed me by knowing a lot more about English History than I did. I was sensitive to her situation as perhaps I was doing to some extent at times the same insensitive things to my family for less worthwhile ends.

What about Erwin Stengel?
In 150 Years of Psychiatry, I wrote the Chapter on Stengel which gives you the story. Stengel studied in Vienna where he was the Privat Dozent, Senior Lecturer is probably the English equivalent, working for Wagner-Jauregg who also got the Nobel Prize. Wagner-Jauregg told him not to worry about the Nazi's, there wouldn't be much of a problem there. Stengel's father was, I think, a Rabbi or certainly a significant leading Jewish figure. Stengel also had a twin and I have subsequently found that the twin was killed in one of the concentration camps Stengel never mentioned it although I worked very closely with him and I think it would be honest to say I was his blue eyed boy which was how I became the Professor really. He favoured me for some reason.

His wife was a Catholic and she realised that the Nazis were a devastating problem and she wanted him to emigrate. He didn't want to emigrate. They went to the International Meeting of Psychoanalysis in Paris, I don't know which year it was, and she had packed as much as she could in with the intention of bringing him somewhere else. She saw Edward Glover and other British Psychoanalysts and somehow or other the Bishop of Bath and Wells was involved in this. They came to Britain with very little other than a champagne reception and evening suits. He lived in the Palace of the Bishop of Bath and Wells who of course was neither Catholic nor Jewish and he had there the job of fire watching when the war came. Anyway the British made him take his finals again and restudy medicine because although you could come from Karachi and practice in Britain, Vienna clearly wasn't up to it. I don't know what the poor chap who had to lecture in psychology or psychiatry felt like with him on the front row.

As a young man, he was really a neurologist cum psychiatrist. He wrote several papers as a medical student on asymbolia and other neurological things. He worked in the standard Viennese psychiatric hospital clinic but he was enamoured by and always talked about Freud. One day in the out-patients, one of Freud's housemaids came and Stengel, saw this was a fantastic opportunity. He got in touch with Freud to say well what would you do with her. He became bowled over by psychoanalysis, which was to become for him the God that in the end failed. He went into psychoanalysis but for some reason or other he had two analysts both of whom died before he ever got anywhere, which laid the basis for Sir Aubrey Lewis famous remark that Stengel was only singed twice. He wasn't analysed really.

Anyhow Stengel came to this country and he worked in all sorts of places like the Crichton Royal and Chichester and so on. He had to take his Scottish Conjoint finals again, which he managed to do while his wife worked in all sorts of ways, as a technician and nurse. She was a very charming Viennese woman interested in the opera. They were very lively, typically Viennese, very vivacious people. He was obviously a sizeable academic and he gradually worked his way up to be Reader at the Institute of Psychiatry at the Maudsley. But relations between him and Sir Aubrey were dreadful. Stengel used to say "Sir Aubrey Lewis is a very great friend of mine <u>but</u>..", while Sir Aubrey talked to me once about Stengel not being very reliable. It was an incredibly child-like level of talking about each other. Sir Aubrey was very powerful and Stengel came to Sheffield to get out of his way. He was being sat upon. They were such different minds - both brilliant in different ways. Sir Aubrey was meticulously critical and logical whereas Stengel would intuitively grasp things. He sometimes got it wrong and when he did there was no way of changing his mind but sometimes he just shattered you by suddenly seeing what was wrong. He was artistic, larger than life rather than meticulous and obviously they weren't going to get on. Sir Aubrey was fundamentally a social psychiatrist - he had this great belief in social factors and a great disbelief in psychoanalysis. In Stengel's case, if you praised Freud you got into trouble because he told you about the drawbacks but if you attacked him you got an earful too. I learned not to mention him. I think he was a bit like a lapsed Bishop of the Church who didn't know what to do really. But he was incredibly brilliant and able, and a lively clinician and a writer.

Stengel came to Sheffield to escape Sir Aurbrey and he set up the Sheffield Department of Psychiatry. It had been just an ordinary provincial town and it wasn't up to much academically in psychiatry - we used to go to the mental hospital for our psychiatry. Stengel had a great interest in suicide. He thought that attempted suicide was usually a gamble. It wasn't a direct intention to kill yourself. He said there is a balance in the mind and that the person only going two ways at the same time is very lucky because human beings usually go seven ways at once. If you want to understand what's happening when a person does something like this, its because he is so angry part of him is trying to hurt someone else and its a gamble with what will actually happen if I do this. I can remember William Sargent saying in a very cynical voice "Do you think someone who throws themselves in front of a train is gambling Stengel?". Stengel said "You know that is one of the very best questions I have had. When I came from Vienna I was impressed by how many railways there are in England. There's nothing like that in Vienna or in Austria. And you know I have seen two of three people most days who have made an attempted suicide but none on the railways, so I am explaining 99% of the problem". That was a typical sort of spontaneous and rather brilliant retort. He was very clever at arguing and he was vivaciously enjoyed by the juniors and everyone around the place as long as you weren't in the firing line. It was intuitive, operatic almost.

One of the other things he did, which was quite important, was for the World Health Organisation. He tried to produce a compendium on how to make different psychiatric nomeclatures throughout the world compatible. He had good German and English and was presented as having good French too but when Henri Ey came I think I translated more between them as neither of them could speak to each other really. Anyhow he nevertheless did have a European and English background by then and a great and deserved reputation.

What was he like as a person?
He was a colourful person and he was great fun. He had great repartee and it was great to see him in argument with someone. He had his hates - people like Sir Aubrey Lewis or William Sargent and so on; he practised psychiatry so differently from them. He wrote a rather unfortunate review of Aubrey Lewis' last book in which he said that if you want to put students off for 300 years let them read this. The Maudsley all protested against what he had said and by then Sir Aubrey had Parkinsonism which made it worse. I was quite junior when he showed me this chuckling all over. I actually had some temerity and said I wouldn't do it if I were you. Oh he said people will be delighted. Some people were, but the majority of people felt it was totally unnecessary. He certainly didn't think much of William Sargent either.

There was something operatic about both him and his wife. They were both very amusing, lively, bigger than life characters who really had been through hell in many ways. Her family disowned her for marrying him. Then there was all this Nazi problem and they lost their money but obviously he came back in a pretty powerful sort of way. He had great friends like Elliot Slater who was the editor for the British Journal. Another funny story comes to my mind; the Journal of Mental Science as it was then had turned down one of his papers on the comparison of rates of suicide in different towns. He was interested in why Sheffield had a low rate. They turned it down because he hadn't applied any statistics and they recommended Chi-squared. Well Stengel hadn't a clue what that meant and he said to me do you know what this is all about. I said yes its not very difficult. He said well you do it. So I did it and of course in some cases I got the wrong answers from his point of view. He said you know I never thought they were any good these things. He wrote an enormously long letter to the Journal and they published much rubbish as far as I was concerned.

There was something very intuitive, imaginative, creative about him and what he hated about Sir Aubrey Lewis and the Maudsley was that they were so critical, there was no chance of being creative. I think he was partly right, but some sort of combination was required. He had enormous energy. He built the department up and made enemies and friends everywhere. So he was the important psychiatrist for this place. Sheffield was very lucky to get someone like him. And of course he had all the contacts. I mean he knew Menninger and he'd worked with Schilder on Schilder's disease and he knew the Freud family especially Anna Freud. He added a colourful dimension to the place which before then was very provincial.

So what brought you into psychiatry?
I actually went into psychiatry in 58. My history is a funny one. I joined the Navy. I was rather stupid. I was going to be a mathematician but I decided I didn't want to do that then. The War was still on so I joined the Navy which I could have avoided by going to University and doing maths. In the Navy I did electronics because that's what they did teach. There I got interested in doing medicine. We were demobbed slightly more quickly than we thought and it was quite easy to get into Sheffield so I came here. All on the spur of the moment. The point about the electronics is that when I was a House Physician I produced an automatic machine for recording diastolic blood pressure. This is now trivial but then there wasn't such a thing. The boss said to me you've done very well Alec but there's no room for the patient in the room with your machine! It had got so many wires etc. Anyhow at just about that time, Sputnik went up and the Americans miniaturised all these things for space so mine was utter rubbish really. I was helped by a successful business man Herman Lindais a distinguished musician friend of Sir Thomas Beechman. He also recommended me to David Smyth in physiology. That made the department of physiology interested in someone who was applying electronics to things. I went there and I worked then with Smyth who was working with Krebs who knew me although I had only been a student. I produced a PhD on biological transport which fitted in well with the problems that both Krebs and Gjessing were concentrating on. Gjessing was focussing on nitrogen metabolism but if you actually look at his results there were enormous changes in sodium. I now think that it was all due to sweating but at the time I didn't realise this.

When Stengel came to Sheffield, I was secretary of the University Philosophical Society and I invited him to come and talk and he decided to talk about "Freud and Religion" which I thought was quite interesting. He said that Freud would never have written as he did had he known the Church of England! Freud, he said, saw religion, either as that of the Orthodox Jews, or that of the Austrian Catholic. They were very strict in the Vienna of the period, not very flexible or liberal and not like the Church of England.

I explained to Stengel that I was doing work on the transport of sodium and potassium and that neither Gjessing nor Crammer, who was also doing these things, had realised that aldosterone which had just been discovered offered a very powerful explanation of the findings. The current theory was that it was the one steroid that was not affected by the pituitary but I said that was wrong and that for some reason in these patients it had become entrained and it was what was causing the swing - it was a feedback circuit. I don't think Stengel had a clue what Aldosterone was. He may have thought it was an Italian politician. But anyhow he was trying to build up his staff and I had rather supported him in the discussions in the philosophical society and I had finished the PhD and he wanted someone so, even though at that stage I'd done no psychiatry, I was made a lecturer as my first job in 58. I have the privilege of not having applied for a job yet.

You also tell a story about the girl next door stimulating your interest in psychiatry.
That was a long time before. This was among the many factors that made me interested in psychiatry. Yes, my first love was a young lady called Betty who lived along the road. I was a very young boy and I was infatuated by this girl. One day I went to call for her and her mother sort of pushed me off. I didn't know what was the matter. We hadn't fallen out or anything. Her mother was obviously anxious. The girl had been admitted to Hanwell Asylum. The mother said she didn't want me to visit her but nevertheless I did. I worked my way in there. You needed to be there to know what the condition of those hospitals was like in those days. You would need to go to India to see something similar now. There was this rather beautiful girl with lank hair and her breasts half showing because she wasn't dressed properly. There are pictures of Bedlam, which were rather like it. It was a shock to me actually. She didn't talk much to me when I went there. I don't know how much she liked me but I know that I was nuts on her for a while and we were good friends. That made me immensely interested I suppose in psychiatry. Intrigued by it. I worked there in my spare time doing odd-jobs and later when I was a student. I wouldn't have known what she had but I think she was catatonic - something which you hardly see now, but which came on acutely and apparently out of the blue. She has always stuck in my mind but I don't know what happened to her in the long run.

Shortly after starting, you got involved with the benzodiazepines?
Oh well when I came into psychiatry, while Stengel was all quite keen in one sense on research, he had no idea how to do the sort of research I really wanted to do. I had a little room, no more than the size of a cupboard actually to do this scientific research in which I was going to solve the problem of schizophrenia and manic depressive psychoses. Stengel said oh well you've got to do psychiatry too now, you're the lecturer in psychiatry. This was important also because it had caused a lot of trouble with the other staff because a lot of people were registrars and housemen and so on and here was someone who didn't know a thing about it made a lecturer overnight.

So I ran the out-patients with other people. Then one day we read a piece in the popular press in 1958; I thought was in the Daily Mail, but they haven't been able to find it. It was an article about Lion taming and a drug or a substance that Roche products had produced, which made it easier. I talked to the others who were Registrars then and we said well we'd better go and see them and see whether its any good for taming very difficult people. And that's how we did what turned out to be the first controlled clinical study of Benzodiazepines. I mustn't give you the view that that was how benzodiazepines were introduced into psychiatry - I think Roche products probably had the intention of doing trials and other people did also but that's how we got in touch with them.

What we did was to give patients bottles A, B or C in a series of different studies comparing placebo, benzodiazepine and barbiturates and asked the patients which they found most helpful. At that stage, the answers came out very strikingly in favour of the benzodiazepines. Much to my chagrin later on this was switched around by the lawyers who were suing Roche who said to me did you not think that people might prefer an addicting drug. At the time, I have to be totally honest we hadn't a clue. Now looking back we should have thought of that but we didn't. Anyhow that's how that started.

It was an unusual trial design to have such a big patient input to the issue

I suppose so. It seemed a good way to do it. In those days you didn't need to be as strict as you do now - there weren't such things as ethical committees and so on. But it wasn't unethical. We told them that this was a study of a new substance and that we wanted to know which helped them most. There were a series of studies. The first trial involved A and B with one being placebo and the other Librium - methaminodiazepoxide as we were told it was. The chemists at that stage had even got the formula wrong. They had missed the chloride which was rather simple. In later studies, the patients got A, B and C with placebo, Librium and barbiturate. Of course there are a lot of things such as the problem of what is a fair comparison of doses and so on and so we did studies with various doses. We played around with it all really. We were very convinced Librium was very effective.

Where did you get your ideas that this should be a controlled trial and was this pre-launch?

Oh the controlled trial was around by then. I suppose the other students and so on in the department of pharmacology had talked about that. That wasn't pioneering really. This was pre-launch. The drugs hadn't even got a name. They had a chemical name which as it turned out was the wrong one.

When did you come aware there would be problems with them

I should think it took us 10 or 12 years - the addicting problem that is. We spotted that they caused muscle relaxation and drowsiness and that there may be dangers with alcohol and this sort of thing quite quickly but we thought these were rather trivial problems. We also thought which is true that you could get ataxia but ataxia and drowsiness aren't major problems in reasonable doses of Benzodiazepines. So we pushed this to one side. As a matter of fact the earliest paper on addiction, which perhaps made me think, was written by a man who was a Senior Registrar cum Lecturer of mine called Hanna. It was an obscure paper. No one will know him - he died rather young actually with a heart attack. He had worked with me in Birmingham, and then ages later when I was in Sheffield and he was in the States, that would be 10 years afterwards, he wrote to me saying "you know I'm pretty sure benzodiazepines are addicting". I said I didn't think so. The Benzodiazepines weren't a central plank of my existence at that stage. They had been just a one-off thing.

We were then very much more interested in Lithium and we had another cockeyed theory that I don't really know is wrong even now. The idea was that my steroid theory of mood disorders wasn't right, but that something that was more like vasopressin was relevant. The reason for this was that we had a patient with massive water retention and we found that injecting his urine into rats produced water retention in them and it worked in a way that vasopressin does. But if you incorporate the urine with thioglycolate, which splits the SS bonds in peptides and therefore would have damaged vasopressin, this didn't inhibit the effect. We were in the middle then of a great enthusiasm - my life is a lot of mistaken enthusiasms. We thought we had discovered a hormone that makes you mad. I'm not even sure its not true but the real problem was we never found another patient who was as wild and showed this in quite as big a way and the techniques we were using to estimate the stuff were very insensitive. It was interesting that something like vasopressin could do this and the theory was that this new hormone was very potent on the brain and had a minor effect on the kidney. In the process, I think we produced the first paper showing that Lithium inhibits the vasopressin sensitive adenyl cyclase in the distal tube of the kidney. That of course fitted in with this idea that if you did that then there would be a repercussion on the brain. Systematised delusions of that sort really. I still don't know its not right. I think science requires you to make big guesses and try. But the techniques at the time were very difficult and the results were all fuzzy although no one denies that the diabetes insipidus due to lithium is due to the inhibition of the adenyl cyclase in the distal tube of the kidney.

Did you have much contact with Mogens Schou. What did you make of the controversies with the Maudsley?
Yes, I'm a very good friend of his. Mogens Schou's father was a manic depressive who had worked with Gjessing because he recognised him as a great scientist. Mogens Schou's father wrote a thesis on manic depressive illness which I read with limited understanding as it was in Danish. I think he wrote it very much with Gjessing. So in that sense I had a contact with Mogens Schou before all this happened and he contacted and discussed it with me. Mogens not only had a father who was manic depressive but he had a brother who was manic depressive and a daughter who had Downs Syndrome so they were in a very difficult situation. I knew him before he really got involved in the Lithium controversy. I'd been made the physician in charge of the Medical Research Council Unit by then and Mogens came over to discuss metabolic studies in psychiatry in general particularly with units in which they could be studied. I was also among the people with whom he was concerned to discuss the controversy with Michael Shepherd. Michael Shepherd was very like Sir Aubrey Lewis. They were both incredibly critical individuals who had good scientific minds but probably in a way that did stand in the way of creativity. Michael Shepherd made a point in a way. Mogens Schou was convinced because his brother had got so well, and Michael Shepherd picked an interesting point intellectually that if people had been ill and you give them something and then you compare before and afterwards you can only get good results or no results - you are not likely to get bad results even using a placebo in a relapsing condition. In a way that's right but in another way its wrong. In Gjessing's patients, if you'd known them for many years and then see something fantastic happen, that was evidence. Schou was terrified of the controversy really. He was very hurt and thought it was personal. He came here and Barbara my wife chatted him up and told him don't take it seriously, not all of Britain is against you. It mattered to him and he discussed it with me but I can't say I had much to do with how to

design a study that would deal with the objection. He did the experiments and I think if anybody showed anything well in psychiatry he did show that lithium worked very well. But he was a missionary where Lithium was concerned.

At the time there was a suggestion from the chemical pathology end of things that if you could measure everything that went in and came out..
That was Gjessing's view of course. And as a matter of fact there's even more than that. Gjessing had this fluid diet, so he knew exactly what went in and all the faeces were weighed and everything was elementally analysed and he was convinced that if you had the clock going as well you would necessarily find out, maybe after you went along a lot of false channels, you would ultimately find out what was wrong. This was the philosophy we were going to follow when I was appointed to the Medical Research Council Unit in Birmingham which Ian Bush ran.

Bush was another interesting character who had produced a celebrated PhD as a young man in Cambridge, on the separation of the steroids using paper chromatography. Sir George Pickering who was a very influential figure at the time thought that Bush was going to get two Nobel Prizes, he had a fantastic belief in this young man. Bush was an egocentric narcissistic genius I suppose. Anyway the MRC decided to pour money into this unit in Birmingham and to put Bush in charge of it. They gave him a fantastic grant to run the MRC unit for the Chemical Pathology of Mental Disorders. It was at the time the most enormous research empire in the country. Bush was also the Professor of Physiology and Biochemistry as well as the Director of this sizeable MRC Unit at the same time as having a job with the British Empire Rheumatism campaign. It was unbelievable when you think of it. He was only a young man in his 30s.

They wanted a clinician to join him. Sir Aubrey Lewis backed me although he hated Stengel. I was in a funny position because they discussed each other with me which was not an easy position. Sir Aubrey, then, was really the grey eminence who put people in positions in English psychiatry and somehow or other Stengel, not to be outdone, got himself on the appointments committee for the job. I asked some things about what Bush's views on schizophrenia were and so on and Sir George Pickering said "don't worry he'll clear all those things up, just do what he tells you". Even though, I was to be the Consultant Psychiatrist in the unit, I was to do what this Biochemist told me to do. It was obvious to me that Bush didn't know a thing about psychiatry but I was so pleased with the facilities and the job and everything that I jumped at it.

It was a beautiful research unit. Harold Wilson came to it. This was in the days of the white heat of technology. But Bush never came to it. He did come to a dance which we had to open the clinic and that was the last time he ever came into the clinical unit. Then he complained to the MRC that they weren't paying him enough and he couldn't do all these jobs and that what they had was inadequate and what they really wanted was a Brain Research Institute. He threatened to go and take his people to the States. All sorts of people from the Cabinet came down to see us, the high and mighty; it hit the headlines. It was a very intense time. Quentin Hogg came to see me after I had written to the MRC saying that the whole situation was deplorable. In the end, when Bush said give me the money or I'll go, Sir Charles Harrington said well if that's how you feel go to the States. I don't know whether Bush meant to go to the States. I think he meant to get an enormous Brain Institute in Birmingham. He was very brilliant but he just had too much on.

But the point about it was Bush had an engineering group in the research team and they were to produce a machine which was to analyse paper chromatograms automatically and feed data into a computer system. I was to keep the people on diets like Gjessing had. We were to collect everything meticulously, urines and so on, and this machine was to automatically analyse almost everything and then we'd know the cause of schizophrenia. So here was the MRC backing this sort of idea. It was all beyond anything like the potential of the machine. In fact the machine never worked, I was left running the clinic without a laboratory because he said we'll have to make the machine first of course.

In response to my complaints, I got a letter from the MRC saying that I should discuss the difficulties in the department with the Director - who was Bush of course. They apologised afterwards when they realised that he had spent enormous amounts of money. When I took over there was all sorts of equipment that hadn't been opened, just bought in some mad hatter way that one day we'd get round things and do it all. I did well out of it, I was made the Director. Probably the MRC felt guilty that they hadn't listened to me. Bush went to the States. Rather sadly he achieved nothing there. I don't want to underestimate him - they were right to see he was very brilliant but he was hypomanic and grandiose. I suppose as a student doing the work on steroids in Cambridge he was kept in some sort of check but to be given all this was ludicrous.

Were there two MRC units in Birmingham - Bradley's unit?
Bradley was also in Birmingham at the same time but I'm not sure that it was an MRC Unit. He certainly got money from the MRC and Bradley had worked for Elkes. Indeed, in the case of Birmingham, the MRC had done the same thing twice - it was a good Oscar Wilde situation. Elkes had gone to the States and Bradley looked after what you might call the remnants of the Elkes' set up. But the Bush unit was a new MRC Unit which I suppose the MRC may have thought was some sort of replacement of the Elkes' unit. Bradley was doing a different sort of thing than Bush had in mind. He was really looking at the biochemistry of the drugs and so on whereas we were meant to be looking at the illness. I had good relations with Bradley but they were more the sort of the chat you had when you met in the bar. He was the Professor of Neuropharmacology and was involved in animal studies but he was never really involved in clinical psychopharmacology.

I had a rather impossible position because firstly I went there rather pleased to go to this fantastic set up with what seemed very good opportunities. But I was kept out of the laboratories which were in the university and dumped into this well-equipped little house in the grounds of Hollymore hospital. I had superb nursing staff all appointed at a high level - five or six of them in this very small place which was only designed to take 8 patients. I had an assistant medic but without the laboratory and the machine, there wasn't anything to do. We did all sorts of other experiments and we kept on asking whether we could have a joint discussion with Bush, who never came although he repeatedly assured me that I shouldn't be concerned about all this.

When he disappeared, the MRC discussed what they wanted to do with the unit. Sheffield asked me whether I would like to be the Professor which I didn't want to be actually because I was much happier doing research rather than administration and teaching. I decided, though, I'd better take it - a bird in the hand's worth two in the bush - but I got two in the hand because the MRC then decided to transfer the Unit to Sheffield and so in 1967 we opened up the Metabolic studies in Psychiatry Unit in Sheffield. This was putting me in a mini Bush position really of having more to do than I could but I took it.

In the mid to late 60s with the amine theories and the pink spot was there any feeling that the biochemical basis of the psychoses was going to be sorted out soon. How did things look? - Gerald Curzon said that when he talked to people about what he was doing there was often a certain sense of shock.
I can only talk about myself. I had read Freud as a schoolboy and I became much more of a sociologist in many ways later but at that stage I was a pretty hard boiled scientist who thought the mind would be explained in the end in a rather chemical sort of way. I think the atmosphere was that there was a good number of people that thought there were some very real psychiatric illnesses which would be explained in a fairly simplistic way. I think the work of Coppen and Shaw was taken very seriously. People thought that the residual sodium theory was correct and of course when lithium came this was immediately jumped on because it seemed to fit in so well with the electrolyte story. The fact that it couldn't be tied together took a long time to filter through.

I got involved at Birmingham with the pink spot story because we had to find things to do. What we actually did was we discovered two new inborn errors of metabolism in the mentally handicapped nearby but they were just shots in the dark. Then we got involved in the pink spot story. Friedhoff and van Winkle had made the story. They published their paper in Nature showing that there was a special pink spot, on chromatograms which was produced by using two reagents - ninhydrin and diazo - together they produced a pink spot from the urine of schizophrenics which didn't appear in normal people. Clark who was a famous professor of medical genetics in Liverpool read this in Nature and told a chap, Bourdillon, quite a junior medical Registrar that at last they had got something of the biochemistry of schizophrenia right - very naive really. So Clark told Bourdillon to go up to Rainhill hospital where they gave him buckets of urine from schizophrenics and he did paper chromatology in a little hut in the grounds. Many of us were invited to Atlantic City and Bourdillon hardly knew what was going to happen to him. He had actually done the world's biggest study on the whole thing, but he hadn't a clue what schizophrenia was. He was just a good medical registrar. But when one of the country's leading human geneticists tells you what to do in this sort of thing - just phone up and get urines - you go and do it. There was a woman who worked with him - but a biochemist without psychiatric training. Anyway she said send the urine down and they confirmed Friedhoff and van Winkle's findings.

This hit the headlines and people in Russia and Japan all confirmed the results. Then we got interested so we repeated these things but I'm afraid we didn't confirm the results at all. We even got urine flown over by a plane from Friedhoff, who wondered whether it had changed in mid Atlantic because we couldn't find the spots. I was working then with a really very brilliant chemist who now lives in Sheffield and he was very much shrewder about these things. The real problem was firstly that the pink spot, which was meant to be 3,4,5 trimethoxyphenylethylamine, a molecule which was very like mescaline and dopamine. This fit in with tentative theorising about dopamine but we had a mass spectrometer - they were coming in then - and it showed that whatever the pink spot was it wasn't that. What we also showed was that you could produce a pink spot by drinking certain types of tea, by taking chlorpromazine or by having constipation. This meant that any time anyone controlled for one of these, the other two could play a part. So unless you controlled for the whole lot, you got a false result. But interestingly, quite a famous Spanish group recently, the Associacion de Castille de Pino, in Cordoba, awarded a fairly distinguished prize to Friedhoff and reproduced all his work of years ago. They published a little booklet and gave it out free to all the members, all paid for by the city council, because of Castilla's great contribution to psychiatry.

Who was Friedhoff?
Friedhoff was a New York academic psychiatrist. I think van Winkle was his assistant. I saw him at the meeting in Cordoba and I didn't know quite what to say because it was just out of this world that this Spanish group were giving him this prize. I suppose it wasn't out of this world, as he did interesting work and made mistakes in a way that I've done less interesting work and made more mistakes but really I think you get a prize for not making mistakes. Anyhow they chose him as their psychiatrist of the year and they re-published all the papers - the fallacies in which are well known. I didn't say anything. I thought it would be rather discourteous. Perhaps I should have. He was actually honest, he said that it wasn't quite as he thought but still that something like it is true, which in a sense may be true. It amused me.

Do you think the pink spot contributed to getting the area moving.
Oh yes it caused enormous excitement and enthusiasm. And remember before it there was Smythies adrenochrome theory. They're all related in the sense that the relationship between dopamine, adrenaline and mescaline was pretty central to them.

What were the forums for biological psychiatry at the time?
Oh there were lots of meetings. I went to a number of things. Kety and people like that were very good at inviting me to things and the World Psychiatric Association were quite good - I went to the Soviet Union and goodness knows what with them. I wasn't a very good meeting attender - many other people

were much better. I read the literature rather more than being very socially involved. I think many people did go to ACNP and The British Pharmacological Society and Physiological Society would take things. They were rather more respectable sort of scientific societies.

Did you have many links with Alec Coppen or Gerald Curzon and others
I met them from time to time. We didn't directly cooperate. I knew more of George Ashcroft who ran the Brain Metabolism Unit in Edinburgh and Eccleston but I certainly knew David Shaw and Alec Coppen and we met sometimes. The Royal Society of Medicine had lots of meetings we would often go to. Gerald Curzon was less involved at a clinical level. Roy Hullin in Leeds was someone who was quite involved in the things we were doing and Richter of course. Hullin, who is Welsh, had worked with Richter in Cardiff. He had the disadvantage of not being medically qualified although in many ways he managed to get round it and he ran clinical units. He had a Metabolic Unit in Highroyds Hospital in Leeds and he was very interested in Lithium and body spaces. In a sense, the innovators were Coppen and Shaw and the rest of us were trying to demonstrate that what they showed was sound, which it really wasn't. Roy was also interested in the cyclases.

Talking about Richters, did you know Curt Richter?
Very well. He was a very brilliant and charming man. He was in Baltimore and I went and worked with him for a little while and saw what he was doing. He came to visit us here many times. He had worked with Adolph Meyer and Horsley Gantt - they were his great mates. He was a very devoted scientist who stuck at it, working in a little primitive place all by himself at the top of the Phipps clinic, in Elkes' set up. He was interested in periodicity in general, but he was mainly interested in how injuries caused periodicity in animals. This tied up with the sort of things I was interested in because I had this idea of the anti-diuretic hormone like substance doing something in mood disorders. He had shown that in rats if you cut off the posterior pituitary, which he did with a little penknife - he was a good operator on animals, you could produce periodic behaviour in the rat. So this was very interesting. I don't think it is relevant but it seemed a possibility at the time. He showed a number of ways in which you could produce periodic behaviour in rats - a number of which we tried to repeat. But he was an artist in doing animal experiments in terms of cutting the right part etc. He also showed that certain cerebral tumours caused periodicity. Most of his work was related to the oestrus cycle in the rat but he also did a lot work on what Halberg later called circadian rhythms.

One of these people who was just working in the wrong area at the wrong time?
Probably yes. What he showed was true but its relevance is what's questionable isn't it.

One hunch I've had is that the circadian rhythm area has suffered from a lack of capitalisation - which may all change if one of the melatonin analogues comes on stream

Of course when I knew Richter, he was already an old man and melatonin wasn't known then. On the other hand he was in the right sort of area. Melatonin is after all only a derivative of serotonin. But he was working before that sort of period and also he wasn't a distinguished biochemist, he was a physiologist. So in a sense he was interested in rather cruder issues, such as correlations between brain damage and changed behaviour - what people like Luria were doing in the USSR. I may be unfair to him but I doubt whether he had a significant knowledge of biochemistry as such. He certainly had collected all sorts of literature on periodicities in patients and he was a great friend of Gjessings as well. They knew each other very well. He must have thought that he was doing something that was going to be relevant.

Also while Halberg coined the word circadian, Richter really put the issue on the map. Halberg would be furious with me for saying so but it was Richter who did the early work on it. He didn't call it circadian - he was looking at periodicity in general but among the things he looked at was the constant time effects in isolated environments and so on. They are circadian rhythms but he didn't use the word. The word was Halberg's. Halberg also came to visit us in Birmingham around 65 and I went over to his laboratories.

Halberg was a very egotistical sort of person. He had a very unusual personality. He was another person who worked on steroid chemistry. He was from Austria and he went to the States. He had an enormous impact on circadian rhythm research because he designed what he called the cosinor analysis. This was incredibly simplistic really. I think the world fell for it because a large number of biological workers really don't understand mathematics. They thought that this was some real discovery that would enable the computer to answer their questions. But it was simply a way of fitting sine waves to long stretches of behaviour. He also insisted that only his computer could do this and you had to send your data to him. Really what he did was he purloined it and published a joint paper with everybody on earth. On the other hand of course he did coin the word circadian. He was actually good at coining words because ciarcadian, circannual, were all his terms. But he dined out throughout the world on a statistical technique which was like fitting a straight line to something, it wasn't anything extraordinary. At least that was my opinion.

If the late 60s in one sense saw psychiatry going down a very biological road at the same time anti-psychiatry was being born and you had some links there. Was there any connection between the two.

I don't know the answer but I can at least claim my ambivalence there because I knew Ronnie Laing very well. He actually said that I was the only classical psychiatrist who talked sense to him which was my great claim to fame. I think actually in this sense I was fairly unique. I think the rest of them thought he was mad. He was pretty eccentric, wild and impossible but there was something a bit like Stengel about him, something highly intuitive and able and you can't just dismiss him in that sort of way. He was friendly. He thought that my biochemical studies were justified as long as you didn't take them seriously until you proved something. He wasn't totally anti-psychiatry in what he said when he was talking to me. And I of course said that maybe the same applied to him that he had to do some proving. The truth is that the medical world had almost no time for him. The arts faculty and the literary world were enamoured by him and the revolutionary attitude of students - the don't-change-your-mind-there's-a-fault-in-reality type of philosophy. I have had some sympathy for that, so much so that sometimes people didn't know which way I was going. There was a sense in which I had a lot of respect for him. In fact I tried to set up a house in Sheffield with him to see if we could do some research on whether you could do better but he was so impossible to organise anything with that he drove me to despair.

I had been very privileged because of in a sense people like Stengel and Krebs and Sir Aubrey Lewis and even Bush. In effect they arranged things so that as I said I didn't even have to apply for a job. But once I was involved in running the MRC Unit for Metabolic Studies in Psychiatry, even while there was a part of me that had very strong sociological and other interests, and a suspicion that things might be totally different, and that what we were doing might be mistaken, I felt bound to continue with that approach. We had a big staff and their jobs all depended on it, so there was no way I could just join Ronnie Laing. I was; playing a certain sort of game and my attitude was okay it may all turn out to be wrong but lets do this as well as we can and if it doesn't work we can say we tried. I think I continued doing the work I was doing mainly because I thought it was possible and I was hopeful. Most of the things were wrong but that's science. Its makes you try and you get it wrong. I guess I was trying to ride two horses at once but I realised I could only ride one at a time.

It was only in the end when the MRC closed the MRC Unit in 1982, that I felt freer really. I didn't feel free to agree with Ronnie beyond a certain level but it wasn't for intellectual reasons - it was for the reasons I'm giving you which are complex aren't they ? When the MRC Unit closed, I didn't feel responsible for the other staff anymore. That was up to them and the MRC. I also think in my heart of hearts, I was getting older by then and I had decided that continually writing grant application, as you would have to do then to get money, was a tedious sort of job and perhaps more sociological or philosophical enquiries would be the sort of thing an older chap could do with a pen and pencil without all the worry. The MRC grant really ran quite extensive and expensive laboratory material and you've got to keep picking up quite a lot of money to go on at that level so I decided not to bother much. I got involved with things like the Chilean study - comparing people in Chile and here.

You became something of a radical psychiatrist
I suppose a lot of people would see me as that. I went toTrieste and tried to see what was in it. I supported the Italian Psiquitrica Democratica and got them to come here and give talks. I'd like to think my mind is still open. I think the major intellectual question of mankind is what's the relationship between mind and matter. I don't think I came to any clear answers but there wasn't much doubt in my mind that whatever is true about schizophrenia, which was what interests me really, whatever is true about it there's not much doubt in my mind that the environment matters a great deal more than I was taught when I started out. You can't afford to do without neurochemistry and so on

but there is a lot you can do in the Italian style. My current view is that you can understand a great deal if you see the individual as wanting to be something very important in life and other people are a nuisance. In this regard you've got to live in some way. If you work this out you're okay and most of us have learnt to be complicit with the common sense. We know how to play the game because it rewards us and you become the Professor of Psychiatry and the Director of the MRC Unit but if everytime it doesn't work then reality matters less.

The results from Chile look quite good. Why do the Chileans not seem to have the same chronicity of schizophrenia. The World Health Organisation's studies suggest the Third World does better. There's quite a lot of attitudinal differences and differences in the family structures that might be relevant. It may also simply be that schizophrenia is such a heterogenous mess in all these surveys that the results are invalid at all sorts of levels. Coming back to the philosophical problem, you can't deny the brain as being relevant but saying why is the big problem. I think in the end we're just left with "are there sentences we can make which are useful". One of our mistakes may be that we think we know what matter is.

NeoDarwinism and sociobiology worry me as obviously powerful and seductive ideas. They are however too deterministic and they depend on that which is necessary for all thought, i.e. extrapolation from some apparently very secure facts to explain other issues. At least in the way that Euclid and Newton can be seen as fantastically successful and useful theories which are so nearly true there is the likelihood that some revision is necessary in Darwinism, or the underlying sciences. Conscious experience, for example, is not likely to be explicable by nineteenth century physics and chemistry, and while natural selection and survival of the fittest are tautologies they are inadequate predictors, the defence will usually be post hoc. There is a logical weakness in that! Further for conscious experience to be advantageous one is forced to be Cartesian or look at ideas like those of Teilhardt de Chardin etc., etc, etc. The crunch line being that as Jaspers wrote a man who eschews philosophy is he who is most ruled by his own! (The quote is correct in spirit but not word for word). Wittgenstein's point that "you must keep the hinges still to open a window" is important especially combined with axiomatic set theory and Goedel's demonstration that even arithmetic requires something to be taken as so which can not be demonstrated to be so. He, of course, became paranoid which may be irrelevant, but he believed that we perceive fundamental truths reliably. Hence, for him, my epistemological problems are ridiculous! In fact my view is that we know a great deal about how to do things, in the sense Heidegger's ***Alltaeglichkeit,*** everydayness, but we are ontologically weak and perhaps mistaken to feel otherwise. The issues always depend to some degree on what is held fast, but may not be so. As Quine and Duhem put it you can hold almost anything to be so if you are willing to change everything else. Goedel's Platonic views seem somewhat similar to those psychiatrists can claim are hallmarks of schizophrenic delusions, holding fast that for which evidence seems at least in question. Perhaps with Descartes he believed that God wouldn't mislead us about fundamental issues, my trouble is that I am not so sure and hold that madness is more represented in confident belief than in the cautious scepticism, which leaves us dissatisfied and painfully curious! Of course you will not be seen as mad if you share confident positions with enough others, while you may be if you doubt too much for their tastes. All this while your own reality must be dependent on the language you can speak and the historical and social position in which you live or have so far lived. Contract too far out and you damage yourself, paint in a new gnre and with new content and you will not be appreciated. You will be like a man trying to kick a football with two feet at once. One foot must be on the ground on to which in Heideggerian sense you were thrown, your times!. That is not to be so relativistic as to deny technological success. Phenothiazines obviously work, but does that solve the ontological question of what is schizophrenia/ It says something about it but perhaps there is a place for chaps like me to nevertheless go on like this from time to time? That may be especially so when they are too old to do much else and are safely tucked away.

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