In Conversation with Kenneth Rawnsley: Part I

Brian Barraclough interviewed Professor Rawnsley on 4 June 1987 at his home in Cardiff.

BB Did you come from a medical background?

KR No. There were no doctors in my family, near or remote.

BB How did you come to take up medicine?

KR When I was about 14 I became interested in bacteriology; microscopes rather than bacteriology I suppose because I had one through which I used to look at all sorts of things. Eventually I focused on bacteria and decided that this was what I wanted to be, a bacteriologist. Not having any notion as to what was entailed and having no medical contacts in the family I went to see the clinical pathologist at the Victoria Hospital in Burnley, Lancashire, which was where I was brought up, to ask his advice. He told me to take a medical degree first.

BB That was standard form then, wasn't it, for a scientific career in biological sciences?

KR Yes, I suppose so. His view was that a bacteriologist with a training in medicine had a more interesting career with a wider choice later on. This was the main reason why I decided to do medicine, to do bacteriology. I applied for medicine in Manchester and started there in 1943. During the first two years of anatomy, physiology and chemistry, I was wedded to this idea. But when I started clinical work my view of the situation changed and I became interested in clinical medicine as a career. At that stage psycho-analysis, an earlier interest of mine, earlier than bacteriology, crept in. When I was a lad, my friends and I used to go and look round the Burnley public library, partly an intellectual thing and partly a social gathering point.

BB Did Burnley have a good public library?

KR Very good. In those days it had the highest rate for the issue of public lending library books in the country. I read Freud's *Introductory Lectures in Psycho-analysis*.

BB Did you seek it out because of personal difficulties?

KR No, not at all; I read it by accident. It was on the psychology shelf next to what looked like a book but was actually a block of wood. There was a label pasted on one side of the block with a list of book titles only available on application to the desk, the works of Havelock Ellis, for instance. Of course, I daren't ask at the desk so I picked the next volume. I found it absorbing. It was written in a compelling style and the ideas were so new to me and so interesting that I read a great deal more in the field. Then for a while I tended to look at everything in psycho-analytic terms and saw complexes everywhere.

BB Did you have a psychologically-minded family?

KR Not a bit.

BB Your mother had a shrewd understanding of human behaviour?

KR Yes, that is true. My parents were quite sensitively tuned in this way. But they would not have been interested in reading Freud. When I began clinical work as a medical student in Manchester I found interest in the emotional, psychological aspects of the work in hospital wards. I suppose my interest in Freud when I was a teenager was a pointer to this. My passion for bacteriology became less intrusive and less persistent, and by the time I qualified I had more or less decided on psychiatry.

BB Do you see a link between psycho-analysis and bacteriology?

KR No. But there is between psychopathology and bacteriology—the detailed study of some aspects of life from a detached view-point. Anyway I went on to do three house physician jobs during 1949 and 1950, the days before mandatory pre-registration years.

BB How old were you then?

KR 23.

BB So you missed the war?

KR Yes, I even missed National Service later on because of eczema. I wasn't sorry about that. During my medical school years, 1943–45, the mind was concentrated by the fact that if you failed the exam you got your ticket into the forces, promptly.

BB One failure and . . . .

KR Oh yes, absolutely. We all worked very hard.

BB Did anybody fail?

KR Yes.

BB Fatal mistake.

KR Disappeared from view.

BB Did the Medical School stay in Manchester?

KR Yes.

BB Not much bombing?

KR There was a lot of bombing in Manchester but the

https://doi.org/10.1192/S0140078900018952 Published online by Cambridge University Press

School wasn't damaged although the blitz destroyed part of the Royal Infirmary. I did not do all that well at medical school, very run of the mill. However because I had come in without any Biology and had to do it after joining the School, I took finals out of time and that gave me the chance of a better house job than some of my colleagues. My first job was with Robert Platt at the Royal Infirmary, Manchester. He was the first full-time Professor in Manchester. I developed a high regard for him. He was building up a new department, a galaxy of talent. His First Assistant was Douglas Black, later PRCP London, as was Robert Platt in his own day. Malcolm Milne was there, Professor of Medicine at Westminster later on, and Bill Stanbury who was subsequently Professor of Medicine in Manchester. Acting as dogsbody for this lot was a daunting prospect. Some of these able men were super-numerary medical officers returned from the war. A number of medical students at that time and some of the senior people had been ex-servicemen and were funded in a special way I never understood. Platt was a man of great intellectual power but withal a good clinician. And a man who had a very keen eye for the psychological aspects of illness. Interestingly his first wife Muriel was a psychiatrist.

BB Judging from his autobiography he had a cyclothymic personality.

KR Yes, he had black dog occasionally. He was the first important post-graduate influence pushing me toward a clinical rather than a laboratory career. After Platt I did a neurology house job, tremendously demanding because Fergus Ferguson the chief, a first class neurologist, worked you ruthlessly hard. If you could stand it, all was fine, you know. After another house job in medicine I had a year in clinical pathology, something often embarked on in Manchester by people preparing for the London MRCP.

I worked for a year in the Clinical Laboratory at the Royal Infirmary, under R. W. Fairbrother whose textbook I won as a prize in school in my bacteriology phase. I chose Fairbrother's book to the surprise and despondency of the headmaster. The bacteriology, haematology, biochemistry and other things that you do in a clinical laboratory finished off any notions I had of becoming a clinical pathologist. When Fairbrother learned of my intentions to become a psychiatrist he was very upset, and went to a deal of trouble to dissuade me from a ruinous path. He said, "You don't want to go into that subject, it is just a lot of mumbo-jumbo and guesswork. Stick to something scientific."

BB Did he want you to be a pathologist?

KR A pathologist or a physician.

BB Do you think he was right, looking back?

KR No.

BB What did you do next?

KR Manchester had an academic department of psychiatry, then in existence for three years. I went as a SHO for my first psychiatric job. I spent three years there, a happy time. The Professor was Edward Anderson and the Lecturer Bill Trethowan, who started work about the same time as me.

BB It was Anderson, Trethowan and you?

KR There were three others, Jack Kenna, lecturer in clinical psychology, May Irvine, lecturer in psychiatric social work, and Lawton Tonge, the Registrar.

BB Situated in the Infirmary?

KR Yes. We had eight beds in two medical wards. The level of disturbance we accommodated in these wards amazes me still. In fact many of the very disturbed patients we saw were not ours, but surgical cases having post-operative psychoses or deliria, the demented elderly and alcoholics with DTs. So it was a small in-patient experience but we did have out-patient work.

BB Did you have mental hospital beds?

KR No. Later we had beds at The Cheadle Royal Hospital, a private psychiatric hospital near Manchester.

BB Still is private?

KR Yes. Though now it has a substantial number of NHS beds I believe.

BB Tell me about Anderson. Where did he come from?

KR Anderson was a Scot who spent the greater part of his professional life in Southern England before coming to Manchester. He spent part of his early career in Germany and was particularly influenced by the work of Kurt Schneider and Karl Jaspers. When he was appointed to the Maudsley Hospital as a consultant he carried the flag for the phenomenological school. He came to Manchester as the first Professor of Psychiatry in 1949.

BB One of the first provincial chairs?

KR It was an early one. There was nothing there apart from two psychiatrists who pre NHS, pre 1948, had been working in the Royal Infirmary. They were mainly concerned with private practice and came in, like the honorary physicians and surgeons, to do clinics in the hospital. Anderson started from scratch, and found it hard adjusting from the Maudsley into what he regarded as a psychiatric wilderness. Nevertheless he set about it. As an undergraduate teacher he didn't come across well. As a postgraduate teacher he was superb.

Up to then I had thought of psychiatry as psychoanalysis. To discover that in Anderson's view clinical psychopathology was essentially phenomenological psychopathology, and dynamic psychopathology was something, well, to talk about and discuss, but not a serious enterprise, was a shock. Of crucial importance to him was the ability to relate to patients in the traditional phenomenological way, the empathic "living in the world of the other individual" and then to set down, in detail the elements

https://doi.org/10.1192/S0140078900018952 Published online by Cambridge University Press

of morbid subjective life which emerged from the discourse, no theoretical position being taken. Anderson's way of doing this was remarkable. He was, of all psychiatrists I have known, by far the most painstaking, the most penetrating, the most formidable in being willing to spend a long time discussing a problem with a patient, analysing the mental state and producing a phenomenological formulation. Having done that he stopped. His interest was in the delineation of mental states and the making of a diagnosis, an expanded diagnosis. Treatment was not high on the agenda. He was a microscopist, if you like; perhaps this is why the approach appealed to me, a failed bacteriologist. He dissected problems into their elements and brought them into a sort of order. Although humane and sympathetic he was a therapeutic nihilist.

In 1951 treatment in psychiatry was rudimentary. ECT, leucotomy, insulin coma, modified insulin therapy were used, but no modern psychotropic drugs. If you believed in psychotherapy this could be tried, but in Anderson's department the psychotherapy in vogue was supportive therapy which he regarded as valuable in the management of chronic personality problems and for certain neuroses.

Anderson's prime contribution was in the approach he recommended, to take a good history and analyse the mental state fully, without theoretical bias. He gave me what he gave many other people, an orientation to psychiatry which is there as a basis whatever other aspects one pursues. His second contribution was the time he was prepared to spend talking to his postgraduates. He didn't rush off to committees, he hardly ever went to London. He was there, and you could always take a patient in to see him. He would never refuse to discuss a problem. When I think about my own teaching of postgraduates in later years I blush how inadequate I have been compared with Anderson. He also taught me to be aware of the importance of personality in psychiatric diagnosis. Personality has a pathoplastic and pathogenic role in psychiatric disorder. Personality determines a range of conditions which pop up in the out-patient clinic and which one can easily be misled into thinking are related to something outside the character; but the character is the essential feature. I regard his training as absolutely bed-rock in this matter. There was a University DPM which existed before the Academic Department was established.

BB The University of Manchester DPM?

KR Yes. A pre-war diploma.

BB How did this come about?

KR There were a number of diplomas in various branches of medicine, public health and so on. Psychiatry was one of them.

BB Did he initiate research?

KR Yes. I worked with him on the psychopathology of the psychotomimetic drugs. We heard about lysergic acid diethylamide which had been synthesised by Hoffman in Switzerland in 1943. We had no real idea of what it was or what it did. It became available in Britain and sounded interesting. So we decided to look into its effects. We first took it ourselves. I mean I took some and one or two other members of the staff did.

BB Did Bill Trethowan?

KR No.

BB Too sensible?

KR Yes, absolutely right.

BB And Anderson—did he take it?

KR No. There was myself and Bob Mowbray, a clinical psychologist, the late Dr Paul Scott and the Departmental secretary, Miss Doris Bee.

BB I have had it so I know what you are talking about.

KR Well that was a very, very interesting experience indeed, but one I would never repeat.

BB You took it once?

KR I took it once. Later on I took half a gram of mescaline and had a similar but more muted experience.

BB What happened with the LSD?

KR Ah well, it was the first time in my life that I saw the world quite differently, totally differently. Not just in terms of visual, spatial and temporal distortion but from the point of view of ego change, the sense of ego dissolution, a terrifying experience.

BB What do you mean by ego dissolution?

KR I can describe it to you, I think.

BB You can still remember this, can't you?

KR Yes I can. I will try to describe it, but it is difficult. My percepts, the view of the door, the view of the table somehow became me. If somebody left the room, for example, they disappeared, they ceased to exist. Because I had perceived them they were part of me and I was bereft if they left. This was threatening and worrying. At the height of the experience I decided it was too much to cope with, so I closed my eyes, a great mistake. Because all visual percepts disappeared I felt I was breaking up, that the ego was somehow going up the chimney. That was so frightening I opened my eyes again and saw everything distorted and jumbled, but at least it was there, and I was there again. I believe I realised for the first time what a patient with schizophrenia meant when he saw someone hammer a nail into the wall and said "That nail is being hammered into my head." My ego boundaries had dissolved. I was the wall, the table, everything around me and the two things were indissoluble. If they were affected I was affected.

The importance of that experience to me was very great for two reasons. I now had some personal understanding of the psychopathology of psychotic illness, organic states and schizophrenic states particularly. And secondly I realised one's everyday experience of the world is idiosyncratic and probably not shared by anybody else. For purposes of communication we assume we see things in a similar way,

https://doi.org/10.1192/S0140078900018952 Published online by Cambridge University Press

but I don't believe this is true. LSD gave me a subjective view of personal psychology and a willingness to try and live into the world of the psychotic patient with a new sensitivity.

BB Do you think the experience fitted a DSM III classification?

KR Yes I do. An organic psychosis, fundamentally.

BB Not schizophrenic?

KR No, organic. Not because of the fact that I had taken a drug, but phenomenologically. Whenever I have talked to patients with organic disorders I have looked at their experience from this angle.

I'll give you an example of what I mean. I was given a problem by Trethowan to solve while I was under the influence of LSD. He said "A train sets off from A at 60 miles an hour for B and a train sets off from B at the same time at 40 miles an hour for A. A and B are 100 miles apart. How long is it before they pass each other?" Well that was absolutely impossible. If I thought about A, B didn't exist. I couldn't retain two ideas in my mind simultaneously. I couldn't blend ideas. The same thing happened with taste. I was given a plate of meat and veg for lunch I could not blend the tastes. I was either tasting the peas or potatoes or the meat but there was no combination.

BB And how did it end?

KR I had a good night's sleep. The next morning everything was pristine, new, seen for the first time. Rather like Adam looking at the world.

BB It's astonishing such a powerful drug has revealed so little.

KR Yes.

BB I expect that is to come.

KR Maybe. We were excited about the drug. I went to talk to Elkes who was working with LSD in Birmingham. It all seemed full of Eastern promise but nothing much has come of it.

BB Did you publish?

KR Yes.[1]

BB With Anderson?

KR Yes. We also gave it to a number of patients. We did it not because we thought it would do them any good, but we wanted to see whether a schizophrenic patient could distinguish between the disturbances produced by LSD and the endogenous disturbance.

BB And could they?

KR Yes. On reflection it was not a good thing to do. Later on I was against using LSD therapeutically. It is a powerful and dangerous drug and produces, I have seen it, persisting psychotic illness in patients and in 'normal volunteers'. I would never do it again.

BB No. Neither would I. I remember a nice beginning which I would live again, but not the awful termination.

So you learnt about phenomenology, how to run a general hospital psychiatric in-patient service, out-patients, and you did some research on LSD. Anything else?

KR We did some research on induced psychosis. Anderson was interested in the Ganser syndrome.

BB He kept up his German contacts?

KR Yes. We had visitors from Germany who kept us abreast of the latest developments in phenomenology. Some of them were rather worrying because they turned out to be psychoanalysts rather than proper phenomenologists.

BB Anderson remained firmly on the continent?

KR Yes, permanently.

BB He was unique in Britain in that way, I suppose?

KR He was in purer culture than any of the other British psychiatrists interested in phenomenology.

BB Than even the émigrés?

KR Even the émigrés. Anderson was interested in pseudo-dementia and the Ganser syndrome. He set up an experiment. Medical students were given a little brief to read. They had been arrested by the police on a murder charge and were to be examined by a psychiatrist. It was up to them how they presented themselves but they were more likely to be leniently dealt with if they were found mentally ill. They were allowed to brood on this for half an hour and then put through a formal mental state examination by Anderson and Trethowan to see what kind of stuff they produced. This was interesting and on one occasion funny. One student produced a paranoid psychosis during interview and then at the end of the proceedings Anderson relaxed and said "Well Mr so-in-so, thank you very much you have been extremely helpful. We are grateful to you for helping with this research." "What research?" says the chap. "Was this research?" And he insisted on continuing this phase for some time afterwards to the alarm of Anderson and Trethowan. They thought they had sent him over the edge.

BB Anderson wrote a successful short text didn't he?

KR Yes. Later on it was Anderson and Trethowan and now Trethowan and Sims.

BB What were Anderson's achievements?

KR His pupils were, in my view, Anderson's most important achievement. Perhaps too, the influence he exerted on other members of his staff who joined him after their training elsewhere. I left the Department in 1953 and Anderson retired in 1965 to be succeeded by Neil Kessel who was joined not long after by David Goldberg. Much occurred in those 12 years which I think of importance to understanding Anderson's achievements some of which I would like to see recorded because I believe him to be underestimated.

BB Who were his pupils?

KR Trethowan came to him as lecturer from the Maudsley. He later had a distinguished career in Australia and in this country where he became Professor and Dean at Birmingham—his interview

https://doi.org/10.1192/S0140078900018952 Published online by Cambridge University Press

with you published in the *Bulletin* (February and March 1984) does not give quite the same picture of Anderson as mine will; Lawton Tonge who did some useful research at the Social Psychiatry Unit before settling in Sheffield with Stengel; Clive Mellor now Professor in Newfoundland.

BB And Hoenig?

KR John came fully trained to Anderson's Department sometime after I left. He had a European training I think. Following Anderson's retirement he took the first Chair of Psychiatry in Newfoundland, preceding Mellor. He and Marian Hamilton together translated Jasper's *General Psychopathology* which until then had only been available in the German. I count this a most important event. It must have had a great influence on the outlook of English-speaking psychiatrists who had no German.

BB That is most of us I expect.

KR Anderson wrote the Introduction to the English edition of Jaspers, a tribute I think to his helpfulness and encouragement to Hoenig and Hamilton who were in his department at the time. Marian Hamilton while still with Anderson had earlier translated Kurt Schneider's *Clinical Psychopathology and Psychopathic Personalities*. Both books, through being accessible in English, have I think, had important influences on clinical practice and also on clinical research.

BB What happened to Anderson?

KR On retirement he left Manchester and went to live in Sussex. I don't think he ever felt quite at home in the North. Then he became a Lord Chancellor's Visitor, retiring finally in his 70s.

BB What happened next to you?

KR Anderson was keen I should go to the Maudsley. He was aware that the Manchester offering was rather narrow and one should have the opportunity for wider experience. I went off to the Maudsley in 1954 having got my Manchester DPM after three years of experience, and not quite knowing what was going to happen.

BB Did you have your MRCP?

KR Yes, I had that before I joined the psychiatric department. I arrived at the Maudsley and was interviewed by the Dean, Dr David Davies. He said "You have been working with Anderson. What you need is some psychotherapy experience." So I was assigned to Dr Denis Leigh, another Manchester graduate. We took to each other. He said "I want you to go down to a mortuary in East London. One of my patients has died and you must get the brain because I am very interested in this case and I want that brain. Bring it back in this tin." And gave me a biscuit tin. So on my first day in the psychotherapy unit, I went down to a mortuary somewhere in East London. I had a tussle with the pathologist who wasn't keen to give up the brain. Anyway I managed to get it off him and brought it back in the tin on the tram. I didn't have a car. That was my first day on the psychotherapy unit.

BB Why did you go to the Maudsley?

KR Anderson sold the place to me.

BB What was your aim?

KR I had no aim. I knew my experience had been limited and that psychiatry was a big subject and there was more of it to be seen at the Maudsley.

BB It wasn't for an academic or research career?

KR No. I had no idea what I wanted to do up to that point. It was strange—I just didn't know what I was going to do at all. I did learn quite a bit about psychotherapy with Denis Leigh. We were taught the Finesinger method. Ted Marley was on the firm with me and we had a good time.

I remember my first experience of the Special Problems Conference held on Monday mornings. That was the first time I had ever seen Aubrey Lewis. I sat at the back of this large gathering of people in the out-patient room and Aubrey came in and we had one of these remarkable conferences. I was intrigued by the widely disparate opinions expressed by the people present, in a way that later on, when you got to know them, became so predictable. At that time I didn't know who they were. And then I had contact with Aubrey Lewis at the journal meetings, which were on Saturday mornings. Unthinkable nowadays.

BB It is, isn't it, nobody would come.

KR I found his way of handling them interesting. Requiring people to defend their position, expecting reasonable background knowledge of what you were supposed to have read, and so on. I quickly began to feel a little sorry for registrars and SHOs there because most of them had come into the Maudsley to start their psychiatric careers. It was such a lottery to which firm they happened to be placed for their first experience. The more I saw of it the more I treasured my own experience of having the ABC of psychiatry, clinical phenomenology, to start with, rather then being put onto a specialised firm.

There was a feeling of uneasiness and uncertainty among many of the trainees. The level of feedback, the level of information coming out of the 'oracles' was not terribly high. People didn't know what their future was going to be. And they got worried about it.

After six months with Denis Leigh I moved to the Professorial Unit to work with David Davies, a man for whom I developed a high regard. He had a balanced and broad church approach to psychiatry which appealed to me. The senior registrar was Michael Shepherd who I found stimulating because he required me to think accurately and clearly and defend my statements. After three months Aubrey Lewis asked me to join his MRC unit, which was then called the Unit for Research in Occupational

https://doi.org/10.1192/S0140078900018952 Published online by Cambridge University Press

Adaptation and later became the Social Psychiatry Research Unit. He told me about the work of the Unit, although I knew something about it already. Lawton Tonge had preceded me to the Maudsley and was working in the Unit and filled me in. The Assistant Director, Morris Carstairs, later became Professor of Psychiatry in Edinburgh and then Vice-Chancellor of York University. The Honorary Director was Aubrey Lewis. There were a number of interesting people working there: Jack Tizard and Neil O'Connor, both psychologists; Peter Venables, later Professor of Psychology in York. And Jacqueline Grad working with Jack Tizard on the mentally handicapped living with their families. George Brown was recruited after I joined. John Wing arrived later, about the time I moved to South Wales in 1957.

My first exercise was with Neil O'Connor to set up a workshop for chronic psychotic patients at Banstead Hospital.

BB Before you go on to that, what were the origins of the Unit?

KR The Medical Research Council established it on Aubrey Lewis's request, in 1948. Aubrey had been interested in the social aspects of psychiatry and worked on the occupational patterns of the mentally ill.

BB A strange subject, don't you think, at least it seems so from this vantage point. There it is, the premier post-graduate institution in the English-speaking world and its first research unit set up by the MRC is concerned with work adaptation.

KR Aubrey was a man of vision and probably took the view that other aspects of psychiatry—genetics, neurobiology, psychology—would look after themselves.

BB Or were unapproachable at that time because of lack of techniques?

KR They were running and, of course, he had given them all a good push within the Institute. I suspect Aubrey deliberately chose a 'soft' area, an important soft area, to test the boundaries and try to develop a scientific framework for the social aspects of psychiatry.

BB A soluble problem?

KR Well, approachable, at any rate. The stuff I did with Neil O'Connor was looking at the effect on a defined measurable index of behaviour of a deliberate change in the social environment of chronic schizophrenic patients. We used the hourly production rates in a hospital factory workshop which these patients could develop under a certain stimulus. We compared them with control groups and groups under other kinds of work stimulation. Although it was apparently a soft field, from the beginning we looked at it quantitatively.

I don't think I have answered your question about why he went for work adaptation—the prognosis of neurotic illness and the importance of personality in adaptation to work. This had interested him since before the War.

BB Was it an important war-time problem?

KR Indeed, but he also saw it as a general problem. Here are people with neurotic illness. Now what determines whether or not they do well? Is it the illness? Is it personality or character? How can we look at this? One index, which is more or less measurable, is adaptation to work, studied through a process of rehabilitation and assessment of output related to psychological and social variables. That's certainly how it was being evolved as a research exercise, initially in the mentally handicapped.

BB Did Lewis choose mental handicap because he was beginning something new and mental handicap appeared to be easy to define, easier than say schizophrenia or neurosis?

KR I don't know. He may have been interested in testing out the stereotyped view of the severely mentally handicapped being incapable of work. Now is that really true? You then discover that under certain conditions the severely mentally handicapped can work, and show a learning curve not so very different from normal except for taking longer to learn a skill.

BB Was he trying to find out something about the handicapped or techniques?

KR I think both. But the methodology and the techniques which had to be developed he regarded as an important part of the exercise. He was happy for a lot of time to be spent on developing them.

BB What part did he play in the Unit?

KR We had regular meetings with him, mind-concentrating meetings. One of us would present a research proposal or give a progress report about ongoing research. He would take it apart. One had to defend this as best one could.

BB One of his least appreciated attributes, to some.

KR When you say least appreciated, you mean they didn't like it?

BB Hated it, were frightened by it.

KR You see I don't agree with that at all. I don't think he was destructive in any malicious or negative sense. It was a constructive attempt to make one think clearly, cut away the sloppy thought, force you into the most economic mode of formulating an idea, testing an hypothesis if you like and producing the methodology and techniques to answer that question. Provided you were willing to play the game it was a bracing and stimulating way of tackling problems. You could sharpen your brain against his. You recognised he was cleverer than you were and knew more about the subject that you did. Provided you did not wilt or regard it as a personal attack you learnt a tremendous amount, about ways of thinking, ways of criticising, and you produced a much better project at the end of the day. I accept

https://doi.org/10.1192/S0140078900018952 Published online by Cambridge University Press

that people were threatened by him. I think it a great pity. I came to know Aubrey Lewis well, as I worked with him for many years. He was a man of great sensitivity and humanity, tremendous warmth and he had the interests of his students at heart. It would have bothered him greatly to feel that people were being put off by his approach.

BB He must have seen that some people were stirred up by it.

KR Yes, I think perhaps he did. But at the same time this is the way he felt one had to winnow the wheat from the chaff in ideas, thinking, procedures, and so on. I can only speak personally. People vary. There are some who need an entirely different, maybe a gentler approach, to bring out the best in them. I personally found it a stimulating and educational experience to have to present anything, a case, a research proposal, a set of ideas to Aubrey, and let him have a go at it.

BB What was his aim with the Social Psychiatry Unit?

KR His aim, having started it off on rather occupational lines, was to let it grow, in whatever way seemed scientifically profitable. I'll give you an example of that. When we agreed that I would join the Unit he said "I would like you to go abroad for a while. I want you to find somewhere to study social research methods." He left it at that. So I went away very puzzled, wondering what to do. Various people came up with various suggestions. Anyway, at the end of the day we agreed I would go to New York City, to the Columbia University Bureau of Applied Social Research and spend some time picking up the latest American social research methods. So I went.

BB Who was there?

KR The Director was Charles Glock at that time but his predecessor was Paul Lazarsfeld who wrote an interesting book called *Mathematical Thinking in the Social Sciences*. He was an unusual, intelligent man who tried to bring numerics into social research in a big way. Of course, this was in the early 1950s. I spent a few weeks there but I found it a bit up in the air. I wasn't able to get into any particular research project. Eventually I decided to look around. I fell in with Ernest Gruenberg of the Milbank Memorial Fund who was kind to me. He gave me some advice and I worked out a deal with Professor Alexander Leighton and went from the humid heat of New York City, it was July, to Nova Scotia.

BB Was Leighton part of the Bureau?

KR No, Leighton was Professor of Psychiatry and Anthropology at Cornell University, which is in Ithaca, upstate New York. But he was working at his field station in Nova Scotia.

BB American?

KR Yes. He was running a large-scale study in Nova Scotia with two teams, one a team of social scientists studying communities in Nova Scotia, the other a team of psychiatrists studying the same communities from the psychiatric point of view. They were supposed to be separate. Never the twain did meet, at least in terms of the data, to avoid bias. Surrounding all this he was building up a body of theory about the relationship between social structure and psychiatric illness. Broadly speaking he predicted social disorganisation was positively related to mental disorder. He defined social disorganisation operationally. The aim of the exercise was to see how the map of social organisation and disorganisation related to the map of psychiatric morbidity.

BB One hardly thinks of Nova Scotia as being a place of social disorganisation.

KR It's a complicated set up in Nova Scotia. This was rural Nova Scotia not Halifax. It was on the other side of the peninsula. Small fishing villages, farms, communities of mixed French and British origin. There were some areas which were pretty disorganised and some affluent areas. Anyway, this gave me an opportunity. Alexander Leighton was helpful and kind to me. He gave me the chance to work with the psychiatric survey teams in Nova Scotia. For the first time I was into epidemiology at a practical research level and learnt a lot about field work, both the social and the psychiatric research sides.

BB What did Leighton find out?

KR About his hypothesis? Well, it was supported, broadly speaking.[2] I learnt a lot from Leighton and much about the use of indices both medical and social. For example, Leighton used lifetime prevalence as a major index of morbidity. That is a difficult index to interpret. The work I did later on in South Wales used period prevalence and incidence. Leighton later took over the mid-town Manhattan study following the death of T. A. C. Rennie in New York City. They found the lifetime prevalence of mental disorder in mid-town Manhattan was 81%, which is meaningless really.[3]

BB So you learnt from the way he did things and from the way you might have done them if you had been him?

KR I also learnt from Leighton the difficulty of working with an all-embracing theoretical framework rather than less complicated, more tightly defined theories and hypotheses which one could test in a more limited exercise, than the huge surveys going on in mid-town Manhattan and in Stirling County, the pseudonym for the study area in Nova Scotia.

BB Is it a secret where Stirling County was?

KR Not now. The main place was Digby, Nova Scotia, a small fishing port on the Bay of Fundy looking over from Nova Scotia towards New Brunswick.

When I came back to the Unit I reported all this and then went onto something quite different, the work with Neil O'Connor on chronic schizophrenics and their response to social change. I had been doing that for about two years when we had a visit from Archie Cochrane, an epidemiologist working for the

https://doi.org/10.1192/S0140078900018952 Published online by Cambridge University Press

MRC in South Wales. He was studying the prevalence of illness, starting with pneumoconiosis, spreading into coronary heart disease and diabetes. He had some well studied and documented communities in South Wales, in the Rhondda Valleys and in the Vale of Glamorgan, a rural area near Cardiff. Archie Cochrane has a number of Aubrey's attributes. He is an iconoclast, a man who requires proof, demands hard evidence for statements made. Clinicians regarded him with apprehension. He came to the Social Psychiatry Unit and offered access to his communities in South Wales for a psychiatric study. Morris Carstairs and George Brown did a reconnaissance. I told Aubrey I was interested. He said straight away, "This is where you can begin to use your North American experience".

BB Had Leighton found something which Lewis thought could be pursued in South Wales?

KR No, only in methodology.

BB You were explaining this as an example of the way Aubrey Lewis would take something and allow you to develop it.

KR Yes. He accepted Cochrane's offer.

BB What was Cochrane's background?

KR Cochrane was a doctor who saw service with the Republicans in the Spanish Civil War. He was captured in Crete during World War II and spent three years in German prisoner of war camps working as a doctor, part of the time using X-rays. He was only a General Medical Officer but did quite a bit of X-ray work in the camps with crude equipment. After the war he became interested in the public health aspects of tuberculosis and other chest diseases and joined the newly set up Medical Research Council Unit in Penarth, just outside Cardiff, the Pneumoconiosis Research Unit, to look at the epidemiological side of pneumoconiosis.

BB Was he interested in the social and psychological side?

KR Yes, and in psychiatry. At one point he said he had considered becoming a psychiatrist.

BB So many people say that.

KR Yes, I know. Anyhow Aubrey accepted his offer. This meant a new chapter in the work of the MRC Social Psychiatry Unit because the work in Wales was an epidemiological venture.

BB There had been no epidemiology until then?

KR Yes, there had in mental handicap. Jack Tizard did some in mental handicap at an earlier stage.

BB A survey of the prevalence of it in London.

KR Yes, a repetition of E. O. Lewis' study, but none in mental illness. My brief was to develop methods for the study of mental illness in South Wales.

The first thing was a social investigation to see how we could best get going epidemiologically. I was joined by Joe Loudon, a medically qualified social anthropologist, and Lewis Miles, a psychiatric social worker, who later went to Australia and is now retired. We worked as a team in the context of the Pneumoconiosis Unit. Joe Loudon concentrated on the Vale of Glamorgan for the anthropological study but we worked together studying the process of the recognition of mental illness, at different stages of declaration. For example, we studied how general practitioners recognised mental illness and referred it to psychiatrists. We looked at the way samples of the population recognised mental illness or aberrations of behaviour, defined it and dealt with it. We were later joined by two psychologists, Jack Ingham and Jim Robinson, who produced instruments for measuring morbidity.

BB Did you develop new instruments?

KR Yes, we used modifications of the Cornell Medical Index. Jack Ingham also developed sophisticated symptom rating scales which we applied to random samples of the population.

BB Not in a city, but the valley villages.

KR In two areas, the Little Rhondda Valley or the Rhondda Fach, a mining community with small villages and townships, and the Vale of Glamorgan a rural area with a market town in the middle. We had private censuses for both areas and were able to draw straight random samples or stratified random samples.

BB Why did you choose such contrasting communities?

KR Because we wanted to study communities where we thought that attitudes, values, perceptions of mental illness and the way in which people dealt with it differed. We knew from hospital records that prevalence rates were much higher in the mining valley than in the rural population. Was the difference due to a difference in grass-roots prevalence or a difference in recognition?

When it all came out in the wash the answer was complicated. We found that however measured—as hospital cases, GP recognition, or morbidity by population survey—the mining valley prevalences were higher than those of the Vale of Glamorgan. The explanation we thought lay in the attitudes of the populations. The Rhondda Valleys are interesting demographically. The population had been sharply reduced since the 1930s; many people had left mining.

BB You were studying a survivor population?

KR Yes. Also with people who were aware of the dangerous nature of their work in coal mining. There was what might be called a rather low threshold for the self-awareness of pathology, by comparison with the rural area. People were much more ready to declare themselves ill or be affected by something or other whatever it might be, backache, headache, depression, than in the rural population. I think the measures of morbidity reflected attitudes which prevailed in those areas.

That's about as far as we got. From that point on you get into difficult waters methodologically,

https://doi.org/10.1192/S0140078900018952 Published online by Cambridge University Press

discussing the relation between social factors and the pathogenesis of neurosis. The two things almost come together.[4]

In the middle of all this we had an interesting interlude in 1961 with the people from Tristan da Cunha. A volcanic eruption on this isolated South Atlantic island prompted evacuation of the whole population by the British Government to Calshot, near Southampton.

BB How many were there?

KR About 260. They were studied from all angles by the Medical Research Council, particularly genetically and for chest diseases. Joe Loudon and I went to look at their psychiatric status and the structure of their society.

BB Why the interest?

KR Tristan was a closed community. Nobody had left or joined it for 50 years. People visited. There was a British administrator, a doctor, a padre and one or two others—birds of passage. The Tristanians had retinitis pigmentosa; at least some of them did, and asthma. They were racially mixed, from America, Europe, Africa, and in colour ranged from rather black to rather white.

They did not regard us as medical investigators, just hangers on—Joe Loudon particularly because of the way he worked. They complained to us about the other doctors who were messing them about. One chap insisted on photographing them naked against a scale for their physical anthropometry. A terrible thing to do, they said. We were harmless. Joe did an interesting social examination of this group. Together we did a psychiatric study, pure gold actually, because when we had finished we found a publication from a Norwegian group 25 years previously. They had landed on the island, unannounced, and studied the medical and social aspects of Tristan. At the moment of landing they found themselves in the middle of an epidemic of major hysteria. People were having fits, fainting bouts and screaming attacks. The Norwegians were meticulous and tracked the spread of the epidemic using personal initials for identification. Twenty five years later, using the initials, we traced forwards and found these people. Most were women. A small proportion were men. The hysteria had probably been triggered off by a Montagu and Capulet situation, two groups of people worrying about a prospective marriage.

We found the main symptom among the islanders, at the time of our study, to be headache, described in a stereotyped way both verbally and non-verbally using similar gestures. About 40% of them had regular headaches and they recognised that emotion could bring on a headache. We correlated the prevalence of headache in 1962 with the prevalence of 'grande hysterie' 25 years earlier. There was a close association between the two: a marvellous example of how predisposed people can take on board neurotic symptoms as a spreading epidemic, or as an endemic condition with stereotyped symptomatology. The same gestures, the same language, in a population on top of each other all the time, sharing values, sharing ideas, sharing symptoms. A simplified and crystal clear example of neurotic epidemiology of two different forms, the spreading variety and the endemic variety.

Although this was in a special population it is the sort of thing that I believe operates in more complex societies by example, contagion, imitation, sympathy, but is far more difficult to study in Western society, where everybody is moving around and rubbing shoulders with lots of other people, than it was on Tristan where everybody was together all the time. From the point of view of psychiatric epidemiology it tied up in an interesting way with the social structure of the population, particularly the leadership patterns. It was the wives of the leaders who had a hypersensitivity to neurosis, which raised the question of assortative mating of leaders with neurotic women, or whether being married to a leader is pathogenic.

BB What happened to the Tristanians?

KR They were fed up with Britain, and didn't like it at Calshot. They all went back except about three who married British people. The longer they were away the greener the island became.

BB You were asked by the MRC to do this survey?

KR We dropped everything and spent a lot of time with the Tristanians.

BB Worth it?

KR Oh yes. A powerful example of the pathogenic and pathoplastic nature of social factors in neurosis.

BB It is in the literature.

KR It has been mentioned.[5] But it loses its impact in the telling. I was more impressed by the Tristan neurosis than any other bit of epidemiology that I have ever come across, because it just shouted at you. I will tell you something interesting. Joe Loudon went with them as ship's doctor on the voyage back. This 'grande hysterie' had not happened for 25 years, but when the ship came in sight of Tristan three or four people went off into fits and swooning attacks.

BB After studying the Tristanians you went back to Wales?

KR We carried on with our work there.

BB What was the relationship with Sir Aubrey Lewis and the Social Psychiatry Unit?

KR He was the Honorary Director. We were a detachment, if you like, of the Unit.

BB He was responsible, ultimately, for what you were doing?

KR Yes. I saw him regularly. He was helpful and supportive. Aubrey retired in 1966 and there was concern about the future of the Unit.

BB Your branch of it?

https://doi.org/10.1192/S0140078900018952 Published online by Cambridge University Press

KR No, the whole thing. In the event John Wing became Director and the Unit carried on.

The Welsh National School of Medicine created a Chair in Psychological Medicine in 1964. I applied, partly in order to secure a base for the continuation of the MRC work when Aubrey retired. My part of the Unit carried on for five more years before dissolving.

BB What were the achievements of the Unit in Wales?

KR We showed it was possible for people from different backgrounds to work together to produce a methodology which transcended the boundaries of social anthropology, psychiatry and sociology. That it was possible to examine random samples of disparate populations using reliable instruments on both the social side and the psychiatric side which were independent of whether or not people had chosen to seek medical advice. Then to address the question whether the apparent differences in prevalence of mental disorder between a mining valley and a rural area were due to differences in patterns of seeking advice or to frequency of mental illness.

We showed there was a fundamental difference between the two areas in South Wales, but that this conclusion begged a lot of questions about thresholds of awareness, of response sets to questions, about illness and symptoms springing from local culture. We finished up answering some questions but posing many more, which I think touch on the fundamental issues of what is neurotic morbidity. Can it be defined independently of attitudes, values and culture? How can one try to measure these things in different sub-cultures or societies? I see the work of the Unit more in breaking new methodological ground than producing answers of value in aetiology or for the provision of services. It is a subject which still has not been fully explored.

BB Your involvement in this sort of work then came to an end.

KR Yes.

BB But it didn't come to an end with some of the other members of the Unit. Ingham continued.

KR Yes, in Edinburgh with Kreitman.

BB Building on what he did in Wales?

KR Yes.

BB Do you think he has got any closer to solving the problem?

KR He has done some good work in general practice in Edinburgh and extended the methodology developed in Wales to answer rather more practical questions, about prevalence.

BB Who else from your unit carried on with research?

KR Joe Loudon went to the Department of Sociology and Anthropology in Swansea University College. He continued an interest in this field though he became involved in teaching and did little more fieldwork. Lewis Miles did a prevalence study on the Isle of Anglesey which had practical importance for service development. Jim Robinson carried on looking at the relationship between hypertension and personality in the local populations. So there were strains of the work which continued. But I got absorbed into developing the new Department in Cardiff.

BB You were the first Professor?

KR Yes. There was a lot of goodwill and a certain academic tradition at Whitchurch Hospital but I had to build the foundations of a teaching programme for undergraduates and postgraduates so it was a long time before I could start to recruit people for research. Most of the researchers who did come into the Department were not doing epidemiological work but biological psychiatry and evaluation of services for the mentally ill and mentally handicapped. Epidemiology was not a main feature of the Department.

BB What do you do when you are a new Professor?

KR That's a very good question. One of the problems is the expectations, which are very high, that you will produce a first-class undergraduate and postgraduate teaching programme, develop a lot of interesting research, produce a rapid improvement in the psychiatric services over a large area, in this case the Principality of Wales.

BB The entire Principality?

KR Yes. That you will relate to the Health Authorities and the Welsh Office and make a case for psychiatry in various contexts. Challenging.

BB Including clinical opinions on the distinguished citizens of Cardiff and their wives and relations?

KR I certainly saw a great many special patients. I thought this one of the privileges of the job. It is time consuming. But I thought it important to try in all ways to foster good relations and to develop contacts with colleagues throughout the Principality.

I felt greatly supported at all times by the psychiatric community in Wales. I have found that there has been a readiness to go along with proposals that I made to support the improvement of postgraduate training; a very heart-warming situation. It does lead to dilemmas of how to husband resources and how to spend one's time, more particularly later on when I became involved with bodies outside Wales.

BB In the first few years how did you divide your time?

KR Much time was spent building relationships with the other Departments in the University College, the Medical School and the Cardiff Royal Infirmary, the main teaching hospital before the building of the University Hospital; time spent consulting physicians, surgeons and other medical colleagues both in Cardiff and outside. You see I was appointed by the Welsh Hospital Board as Adviser in Psychiatry for Wales which meant spending a fair amount of time talking to people in the Welsh Hospital Board about planning of services for Wales.

BB What qualifications did you have for that?

https://doi.org/10.1192/S0140078900018952 Published online by Cambridge University Press

KR None at all. None whatsoever, except I suppose some sort of knowledge of the range of clinical problems which were to be found in a population and my scientific outlook, as an epidemiologist, was of value.

BB Who taught you administration?

KR I picked it up by osmosis by watching a few people operate.

BB Do you think the undergraduate professorship, as you have described it, which is pretty standard around the country, is reasonable?

KR In one sense, no, it is a bit of a nonsense because its a Leonardo-type expectation. The Professor has a lot of influence. Rightly or wrongly people look to him for help and advice. Someone purely concerned with research or teaching could not have this kind of potency. It is a paradox in a sense, if you have a broad range of interests and roles then you are regarded as an oracle. What you say carries weight.

BB Is it the same for the other chair-holders in the Medical School?

KR No.

BB In what way does it differ?

KR The other disciplines are not so aware of the need to develop services. Psychiatry is a complex profession when it comes to building services and there are many variables to be taken into account. Psychiatrists are more aware of the need to develop the infrastructure of services, the teams required to produce a good service, and are willing to devote more time to this, than surgeons or physicians. That is because of our preoccupation with the social aspects of medicine in psychiatry. For good or ill your average Professor in the provinces has to be willing to take this broad and extended position, wearing a lot of hats and being willing to forego the luxury of spending vast amounts of time doing any one thing to perfection.

BB I suppose he can be more effective if there are subordinates to whom he can delegate.

KR Yes.

BB What does the President of General Motors do all day one wonders?

KR He has a clear desk. But it's not like that in professorial psychiatry. You don't have a clear desk. You cannot organise your life as you would want to because always you are looking after patients. Whatever else I was doing I would always have beds, and out-patient clinics and domiciliary visits. That seemed to me absolutely essential otherwise you took off into the clouds, lost all touch with the realities. But if you do that these clinical things take priority. People ring you up and ask "What you are going to do about Mrs Bloggs?"

BB What were your achievements, in Cardiff, since you have been the Professor. You were appointed in 1964 and retired in 1985. Twenty one years is a long stint.

KR The Department started with a Professor and a secretary in 1964. In 1985 we had four senior lecturers, three lecturers and a number of research people. Psychiatry was well represented in the undergraduate curriculum. We had developed a good rotational training scheme for registrars and also for senior registrars. There were two main research units, one of which was concerned with the biochemistry of mental illness based in Whitchurch with David Shaw and the other with the evaluation of mental handicap services in Wales run by Roger Blunden. We had a senior lecturer in mental handicap, Valerie Cowie, who since has been given a personal chair in mental handicap in Wales.

I used to feel depressed towards the end of my stay. The University recession I thought had touched a low point and I believed things were never going to be so good again as they had been. However the Department became well established, gave a good account of itself on the teaching front at undergraduate and postgraduate levels and at least in the latter half of its life was beginning to turn out some research. For a new department to build up a research head of steam takes at least 10 years, I would say.

(The references will appear with Part II of this interview which will be published next month.)

https://doi.org/10.1192/S0140078900018952 Published online by Cambridge University Press