Psychiatric Bulletin (1993), **17**, 260–273

Interview

In conversation with Robert Cawley

Hugh Freeman interviewed Professor Cawley in August 1991 and on subsequent dates.

Professor Robert Cawley
BSc Hons 1947 PhD 1949
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Robert Hugh Cawley was born in 1924 in Birmingham and was educated at Solihull School, University of Birmingham and University of London. He was House Physician then House Surgeon at Queen Elizabeth Hospital, Birmingham in 1956 and from 1957–62 he was registrar then senior registrar at the Bethlem Royal and Maudsley Hospitals. Professor Cawley was Senior Lecturer and First Assistant at the University of Birmingham Department of Psychiatry from 1962–67 and then became consultant psychiatrist at the Bethlem Royal and Maudsley Hospitals from 1967–89 and at King's College Hospital from 1975–89. He was Professor of Psychological Medicine in the University of London at King's College Hospital Medical School and the Institute of Psychiatry, 1975–89 – Emeritus since 1989. Professor Cawley was an examiner in the Universities of Glasgow, Edinburgh, Manchester, Birmingham, Southampton, Bristol and London, and was Chief Examiner, The Royal College of Psychiatrists from 1981–87. He was a member of the Medical Research Council from 1979–83, Consultant Adviser to the DHSS, 1984–89, and Civilian Consultant Adviser to the Royal Air Force from 1986–89. He has published work on biological, medical and psychiatric topics in scientific books and journals.

You have described yourself as a late developer – as a psychiatrist and indeed as a doctor. Could you tell us something about your earlier life?

I was born in Acocks Green, an unpretentious suburb of Birmingham, and lived there until I was 16, when the family home was blasted to pieces by one of Marshal Göring's landmines. I was fortunate in obtaining a scholarship to Solihull – a minor public school – which was remarkably good in many ways and rather appalling in others. I owe a lot to the place. Perhaps the only teacher who made any lasting impact on me was Margaret Noyes, one of two women appointed to meet the exigencies of Hitler's War, who did much to repair the depredations left by those earlier responsible for educating me and my luckless colleagues in music and the visual arts. Thanks to her, I was able to establish a love of music, which has been one of the outstanding pleasures of my life.

At the age of 16, when I took the School Certificate (now called 'O' levels), I was interested in arts subjects – English Literature and Modern Languages. I had a lofty attitude to science, which was for the boors. But already, I was having long episodes of illness, which caused me to slip back three years in my school career. I spent long periods in hospital, with severe anaemia, which nearly killed me and was eventually cured by splenectomy. The doctors and medical students were all very friendly and in fact provided engaging seminars on my condition. I learnt about reticulocytes and normoblasts, mast cells and differential counts, and picked up some rudimentary immunology. I became fascinated by the subject and also developed an enormous respect for medicine as a career, deciding to aim in that direction. But in my enfeebled state after the illness, I had some difficulty in changing tack and turning to physics, chemistry, biology and mathematics. I eventually obtained these higher certificates ('A' levels), but by that time, I was nearly 20. At the eleventh hour, I was rejected as a medical student because the authorities considered I wouldn't have the stamina. So I turned to zoology, which became my first love, academically speaking, and my first degree.

Is there anything else about your early life or pre-university period that you would like to recall, or which you feel may have influenced you in your later career?

Our family structure was a bit unusual, and perhaps illustrates my parents' approach to family planning. I had one brother and one sister, respectively 16 and 15 years older than myself. My arrival must have been a shock for all concerned. They were very decent about it, and I had quite an agreeable childhood. My father was a headmaster and my mother had qualified as a teacher at the turn of the century; two of her sisters and numerous other relatives did the same. My sister became a teacher, but my brother escaped, though even he married one. So I was brought up in a milieu where teaching was an unremitting topic of conversation, day in, day out. The strange and fearsome political and social events and alignments of the 1930s were also kept under review, and so was gardening – my father had a nice garden. It was a pleasant, sheltered, and essentially humdrum childhood, but by the age of 14, the health problems I have mentioned began to dominate my adolescence. In one of my several attacks of haemolytic anaemia, my haemoglobin went down to 18%, causing an episode of right hemiparesis and dysphasia, which gave me my first neurology lesson. It also alarmed me, and everybody else, more than somewhat. I recovered from that fairly quickly, with the help of blood transfusions. In those days (1940–41), there were no blood banks to speak of in civilian hospitals. So it became the duty of the unfortunate registrar, who shared my blood group, to become the donor. I remember him well, an extremely nice man named Christopher St Johnstone, who later became a consultant physician at the Queen Elizabeth in Birmingham – and sadly died prematurely – not from bloodlessness, thank goodness. His chief was Sir Leonard Gregory Parsons, FRS, an illustrious paediatrician and general physician. So my development was interrupted, but I met some fascinating people and the world seemed a generous and indulgent place, even though I was very aggravated by these recurrent illnesses.

Could you tell us something about your time as a zoologist?

I wasn't a very good one. Never having collected butterflies and moths or gone out fishing, I wasn't particularly competent as a natural historian. But I became interested in genetics and ecology, and in marine biology, spending some time in marine biological stations – at Millport in the Firth of Clyde, at Port Erin in the Isle of Man, and at the very famous one in Plymouth. In my final year, Lancelot Hogben, the head of department, returned from War service. He was a remarkable man – displaying vast knowledge and enormous energy, sparkling originality and passion, debunking the conventional wisdom, all with a background of infectious enthusiasm laced with flashes of impatience and interludes of moroseness. With him, there were no dull days, but often some pretty testing ones. Recently engaged in developing medical statistics in the War Office, he was planning to exploit its powers and expose its follies more generally in medicine and biology. A most gifted teacher, he was very clear about the social and political implications of science, being for example a vociferous opponent of the then popular eugenics movement. Through him, I became more interested in human genetics and medical statistics, and in experimental biology. Later, I moved into his new department, and completed a PhD in Medical Statistics. At that stage, I found that though careers in biology or statistics had their attractions, I was still intrigued by medicine, and kept looking in that direction. Lancelot alleged that I was out of my mind, stormed at me, and tried very hard to dissuade me by offers of some fascinating jobs – in the end, though, he graciously accepted my refusal to take his advice, and our friendship lasted till the day he died.

Is there anything about your first university career, your life as a BSc student, that you would like to recall?

It was a curious time. I went to the University of Birmingham in 1944, at the age of 20, and of course most able-bodied men and a lot of women as well were doing their War service, so it was a very attenuated group of students and staff. I had had an instructive medical board for National Service, being required to stand naked in front of several ancient, whisky-sodden doctors, who staggered round me, and said they didn't think much of me. Therefore, I was graded Four – unfit for any form of National Service, which was all rather humiliating when most of my contemporaries were heading for accelerated maturation in the armed forces. But at least I started my university career then, instead of much later. It was, I suppose, like the university in pre-War years; we felt ourselves to be privileged and made the most of it. I did a bit of work as a reporter and later editor of the university newspaper and subsequently of a literary magazine. I can't remember that I worked very hard, but on graduating, I was fortunate enough to be awarded a University Research Scholarship, which enabled me to stay on without having to pay any post-graduate fees, which I didn't have the money for. So I did my PhD work on that scholarship, and later my own research grant, for $2\frac{1}{2}$ years. It was then that I really learned the meaning of work and also something about research under Hogben.

https://doi.org/10.1192/pb.17.5.260 Published online by Cambridge University Press

Were there other people who influenced you during your studies of biology?

The acting head of department was Minnie Johnson, herself a gifted teacher. Her methods were informed by a great interest in social psychology and group dynamics, though that was all a bit lost on me at the time. She was an articulate person who could take a warm interest without being intrusive. Forbes Robertson, who has only recently retired from being the Head of Genetics at Aberdeen, was a very bright, amusing Scot, who gave a superb course in genetics. There was Michael Abercombie, married to Minnie Johnson, a man of great erudition, with enormous scientific flair and curiosity, who later became Director of the Strangeways Laboratory at Cambridge and a leading biologist of his generation. John Waterhouse, a Cambridge mathematician, was a most agreeable man and an excellent expositor of statistics as applied to biological and genetical work. To him in particular, and to the others, I owe a great deal. Later, I was also influenced by a man a bit younger than myself, Raymond Wrighton. He had achieved the heights in his mathematical studies at Cambridge, and worked in R. A. Fisher's department where he obtained the rather rarefied Diploma in Mathematical Statistics. He tried to teach me how to mistrust authority, but I lacked the mental power to follow him closely.

I'd like to ask you now about your time as a medical student, which I think was also at Birmingham.

It was. First I had to persuade the school to admit me, and secondly I had to find the money. Neither of these were easy, and I was impeded by Charlie Smout, the Sub-Dean, who looked and behaved rather like a complacent churchman, authoritarian and ever on the look-out for sin and idleness. He considered I had already had my innings: with a first degree and a PhD, it was unfair that I should occupy a place in the medical school when some bright boy from King Edward's school down the road might be taking it. And, he said, working your way through medical school is out of the question. So your wishes can't be fulfilled for two separate, valid, and unalterable reasons, he concluded with relish. I was not the first, or last person to have trouble with his suburban outlook, but two people came to my rescue. The first was Sir Leonard Parsons, the Dean, who had known me since I was his patient, years earlier. I had an interesting encounter with the two men, in which Charlie's pugnacity was elegantly reformed. The second was Thomas McKeown, the Professor of Social Medicine, who offered a part-time research fellowship in his department, which would provide me with enough to live on. I have felt eternally grateful to those two men.

Having started, I found anatomy extraordinarily difficult – I couldn't remember things and I sometimes questioned the authority underlying some of the assertions we were expected to swallow. I managed to struggle through, but it was a humbling experience, because I wasn't nearly as good as the bright boys who had come straight from school, with their state scholarships. Once anatomy was over, I enjoyed reading medicine, though I didn't distinguish myself: I was occupied with other things which I thought I could do fairly well, such as medical statistics, research, and teaching. I graduated six months late, at the end of 1955, at which time I was 31.

Presumably your extra years and the experiences you already had as a scientist must have given you a rather different outlook on the medical course, compared with the students around you. You said you questioned the anatomy teaching, which must be a very rare event. What other differences do you think there were in your approach to medicine from those of your colleagues?

It's not that I was so arrogant as to question the anatomical structures, but I felt upset by the naive biological assumptions about form and function. I had been trained to doubt. I enjoyed all the clinical work, but found some of the underpinning of clinical knowledge – much of the pathology, biochemistry, and microbiology – disjointed and uninspiring. As a result, my clinical work obviously suffered, but the general approach, the clinical reasoning which I was taught, I found congenial. The Professor of Medicine particularly interested me: Sir Melville Arnott had a most rigorous approach to clinical medicine, as well as a huge store of knowledge.

What about your exposure to psychiatry during your time as a medical student.

We weren't formally taught anything to speak of. There were a few visits to the local mental hospital with the Medical Superintendent, J. J. O'Reilly; his demonstrations carried conviction, but I don't think anything very much was conveyed, though he and his colleagues certainly did their best in limited time. There was also Myre Sim, in the teaching hospital; he tried to teach me something. In my year, there were precisely 100 students and at least 12 of us became psychiatrists, including Alwyn Lishman and Michael Rutter, later my colleagues at the Maudsley. I think this must have been the play of chance, because there was nothing to link the teaching we had with a wish to practise psychiatry, nor any common thread binding the 12 of us.

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Any other personalities you would like to recall from your student or early medical career?

Tom McKeown's department had some talented people – Reg Record, Ron Lowe, and Brian MacMahon – each of whom later reached the top of the tree academically. They were extremely friendly and encouraging and taught me a great deal, not only about medicine. In Arnott's department, there were two excellent senior lecturers, Trevor Cooke and Ken Donald, as well as George Whitfield, who was a superb teacher; I was fortunate, when I graduated, to become one of two house physicians in the Medical Professorial Unit, and then a house surgeon in neurosurgery with Professor Brodie Hughes. He was a cultured and sceptical man and neurosurgery was, of course, a fascinating topic, having some relationship to what I hoped to do in the future.

At the end of your pre-registration year, what did you decide to do next?

In earlier years, while I was doing my PhD, I met another doctoral student, a medical graduate named Vera Norris. She was appointed to the Maudsley as a lecturer in medical statistics, about the time I became a medical student, and made a very considerable impact there. She wrote the first *Maudsley Monograph* and did a lot of work in collaboration with the medical staff. We were good friends and she was enthusiastic about the Maudsley; what she said attracted me greatly. When the time came for her to leave, to accompany her husband to Scotland, Aubrey Lewis, having heard that I had been reared in the same stable, was quite keen to meet me. This happened when I graduated, before my house jobs; it was the first of my fascinating encounters with him, and he invited me to take Vera Norris's job right away. He thought that having obtained a medical degree, I would be wasting my substance if I trained as a clinician and was rather keen that I should get on with developing medical statistics in relation to psychiatry as soon as possible. I resisted with some difficulty because, of course, I was very flattered by his offer. However, I wanted to become at least a registered medical practitioner, even if I decided not to train as a psychiatrist. Incidentally, Vera Norris died with a tragic illness, two or three years later, while still a young woman.

Having resisted that flattering invitation and completed your year as a houseman, what did you do then?

After two or three months completing some research, I went to the Maudsley as a senior house officer. I found it immensely rewarding, and became convinced that clinical psychiatry was the career for me. So I went through the training, and the consultants I was working with taught and inspired me – Felix Post, Kräupl Taylor, Michael Shepherd, and Edward Hare. In due course, I became a senior registrar. I had been encouraged by Aubrey Lewis to take the Conjoint DPM, which I could get more quickly, so that I could accept a lectureship. It was agreed between us that it should be a clinical lectureship, rather than one in medical statistics, and after I had completed my DPM, the plans were activated. I had been very careful to say that I wished for an honorary senior registrar post but, for reasons which were pretty complex, it turned out that Lewis wasn't able to manage that. So he told me that I would be a clinical assistant in out-patients, and I told him I would have to withdraw my acceptance of the job. We then had some rather frank meetings and correspondence, in which he told me I would be throwing away my career if I rejected the post I was being offered, but I said I wanted to be a clinician and therefore had to do what was necessary. Soon after, I was appointed as an NHS senior registrar and a little later achieved what I wanted – being appointed a clinical lecturer and honorary senior registrar.

So it sounds as though Sir Aubrey's prognostication on that occasion was not very accurate – was it really important to stand up to him, and perhaps possess a strong personality of one's own on that sort of occasion?

Well of course, people used to find Aubrey a rather awesome person, but I had been brought up in the school of Lancelot Hogben, who could be far more scorching than him. With Lancelot, the legend was that if you could last a year, nothing would destroy you. I never found Aubrey anything but charming and genial, although we did have the occasional brush. He thought on two occasions that I was making a great mistake with my career, but he later conceded that he had been wrong. There have been a great many panegyrics of course about him, but none I think has given the right emphasis to his sense of humour. As long as one wasn't intimidated by him, he was a delightful person, with an immense sense of fun; he could throw back his head and laugh and demonstrate a belief that one should be flippant only about serious topics. When in that vein, he was an entertaining, warm and friendly man.

I must confess that I saw little evidence of any of these particular qualities that you're describing in my encounters with him. Can you say a little more about the personalities you were in contact with at the Maudsley or who taught you then?

Felix Post, my first consultant, struck me from the beginning as a man with an impressively logical

https://doi.org/10.1192/pb.17.5.260 Published online by Cambridge University Press

approach to clinical problems. It was a general psychiatric unit – he later specialised in old age psychiatry, of course. To a beginner, his logic was compelling and he set high standards of clinical reasoning. He never failed to recognise the distinction between facts and suppositions, observations and conjectures. He emphasised the value of a detailed, objective examination of a person's mental state. So he taught me the basic stuff, and I have found myself going back to things that I learnt from him on many occasions. Then I worked for Kräupl Taylor, who some unfriendly people said was a psychotherapist who didn't believe in psychotherapy. That was a harsh judgement, not correct by any means. He was a superbly intelligent, able man, demanding of rigorous standards in his registrars. Case presentations never took less than an hour, and often at the end, he would indicate his disappointment that you had really said nothing of value. He was critical, fierce in some ways, but immensely engaging and again very logical. He assumed that you knew your psychopathology and was very fretful if he found you didn't. He argued that psychoanalytic technique was far more important than the content of its theory, so that you might use the techniques associated with transference and interpretation to the advantage of the patient, though you didn't have to accept the theory behind them. From that developed what was known as his challenge therapy – prokaletic psychotherapy – in which, on the basis of having established a good transference relationship with the patient – not an easy task – you were then able to challenge or encourage him or her, and offer interpretations of the subsequent response. The doctor's approval or disappointment had a strong meaning and effect, especially for someone with a personality disorder. It certainly helps the patient to reflect about his or her own feelings and assumptions, and this is potentially very important. Kräupl Taylor described it in detail in a paper in the *British Journal of Psychiatry* (1969, **15**, 407–419), but unfortunately it is hardly ever referred to now.

I can confirm what you said about Felix Post, because my own first placement was with him, but so far as Kräupl Taylor is concerned, do you think anything is left now of that rigorous intellectual tradition that he maintained?

That question is very important and difficult to answer optimistically. A lot of that searching, critical, insightful approach to psychopathogenesis is a thing of yesterday. We now find a great emphasis, perhaps correctly, on descriptive psychopathology and phenomenology, which have recently come into their own, whereas psychoanalytic or dynamic psychopathology is very difficult to understand, unless your standpoint is that of a psychoanalyst. It's understandable only within its own terms. Beyond that, I would find it very difficult to think of any strand which measures up to the highly intellectual and at the same time ingenious approach that Kräupl Taylor practised.

Does anybody now practise psychotherapy with the challenge as part of the technique?

From time to time I make use of it myself, though not quite in the same rather stringent terms that Kräupl Taylor demanded. Any challenges I have employed in trying to help patients have been rather less overt than this. KT would emphasise that you had to establish a good working relationship with a patient before you start challenging him or her. However, this was too much to ask for some of my colleagues, and I do remember one trainee, now eminent, uttering a premature challenge to a woman with problems of emotional control, "Now you're angry and I think you're going to throw that bottle of ink at me", whereupon his prediction was fulfilled and he acquired a new suit, courtesy of the House Governor (according to the theory, the patient should have denied being angry). Another story was that in the ultimate challenge situation, KT failed altogether. When a man pulled a gun on him in out-patients, instead of issuing the challenge, he rushed for the door and shouted for Sister Lawley! My belief is that it's a very difficult strand in psychotherapy, which calls for careful training. I don't know of anybody who works that way now.

What about some of the others in your list?

I learnt much from Michael Shepherd, and agree with others that he has an outstanding intellect. Edward Hare was a clinician with strong interest in the uses of epidemiology – he was one of this country's pioneers. His influence was great, though he had an idiosyncratic approach to clinical teaching, which at the very least taught one to examine one's own utterances critically. I never worked personally for Eliot Slater, but he was a very influential man at the Maudsley. At his case conferences, he seemed to have a rather lofty attitude, but with a firm basis of experience and his feet very much on the ground. At the same time, he was a tremendous humanist and a man I admired greatly. A very different kind of psychiatrist was Denis Leigh, for whom I was senior registrar for a while. He was hard-headed and blunt, yet compassionate and highly successful in the market place. Douglas Bennett, who arrived at the Maudsley just as I was about to leave, was the first person who made me aware of the scope and future of community psychiatry.

Would you like to summarise the essence of the Maudsley at that time in a few words?

It was a University hospital in the best sense. The things one learnt were as much from one's peer-group as from seniors. Some of the senior registrars were six or seven years ahead professionally, so there was a whole range of people with greater or less experience, talking and arguing in the common room. The set-up was superb as a place for learning a difficult subject; one could both obtain enormous help and support and have a very enjoyable and stimulating life. I looked forward to the days all through that period.

I know that many people (including myself at times) experienced the downside of the institution. However, shall we go on to your next phase, at Birmingham?

The downside became very apparent to me at a later stage in my career. Meantime, I was appointed as Senior Lecturer in Birmingham in 1962, six months after Bill Trethowan came to the Foundation Chair. He'd come from Sydney, where he had led an influential department, so he was an experienced professor, who earlier had known the Maudsley very well. I had mixed feelings, though, about leaving: one left behind one's own coterie and support system. I left the excitement of London for the relatively drab existence of Birmingham at that time, yet it was good to have more responsibility and to see an academic department shaping. It was hard work because in those days regional training schemes were non-existent and Bill was one of the pioneers in establishing organised training for senior registrars. I used to make parochial visits to all the mental hospitals in the Birmingham region, like a bishop's chaplain, and learnt quite a lot about what was going on in them. Earlier, I had been round many mental hospitals in my work for the MRC; so I came to have a pretty extensive view of mental hospital practice. I saw a lot of the better and the worse things about them. Many of the medical superintendents and other consultants in the hospitals were most interesting people, with great humanity. I can remember being impressed, though also sometimes baffled by many aspects of mental hospital practice that I witnessed.

Could you give a few impressions of the mental hospitals in the Birmingham region that you saw at that time, in the early 1960s?

There was the Central Hospital at Warwick, at which the medical superintendent for a long time had been Edward Stern. It was known as 'Stern's Place' and a wit had said that the hospital was run by the Stern Gang. It was a large hospital in the most superb countryside of South Warwickshire, but obviously insanitary; years later, they had one of the most notable outbreaks of typhoid in recent times. Teddy Stern was highly authoritarian – the king of the whole creation. I think everybody there – staff and patients – saw themselves as in fealty to him in some way. One of his lieutenants, Clifford Tetlow, did quite a lot towards the evolution of the College from the RMPA, but I think the other consultants had their heads down so much that they weren't able to see what was going on around them. Stern was quite a scholar and wrote a few good historical papers; he reminded me a bit of Sir George Pickering, then Regius Professor of Medicine at Oxford. I think they were the same type of person, that one doesn't see in medicine so much now – tremendously authoritarian and having reached an exalted position, stubborn, with a benign mien, and unduly influential. They weren't people one would cross swords with. On the other hand, there was a hospital at Lichfield called Burntwood where Clegg, the medical superintendent, believed in including his consultants in all important decisions.

I think your question is really asking, though, what it availed for the patients. It was difficult to understand. At that time, extremes of psychopathology were the striking features of a walk through a mental hospital. There was a lot of established catatonia but also other very strange behaviours and emotional outbursts. I remember those phenomena as characteristic of much that I saw, though they're now much rarer, fortunately. As a visitor, it would have been unusual to have a coherent conversation with a patient, yet whenever I did try to do so, I was always a bit surprised by the amount of sanity which existed in even the most dishevelled or bizarre person. I don't think the lessons from that era have been fully learnt. For example, community care with inadequate provision must be pretty pathogenic, and there may still be a place for total care in a residential setting for a small number of mentally ill people. But have the rational principles for effective community care and modern treatment in institutions really been worked out? So often, they seem to be based on *a priori* assumptions rather than experience.

Let me ask you what else has influenced you in becoming the kind of psychiatrist you are.

From psychiatrists, there has always been the opportunity to learn how *not* to do things as well as what to take as exemplary. I have learnt a lot about techniques and styles from observing, not only my seniors, but also my contemporaries and juniors. There have been, of course, tremendous advances in the scientific disciplines and knowledge underlying psychiatry – the psychosocial and biomedical

sciences – as well as impressive advances in clinical psychiatry itself. So it has become a clinical/academic subject of distinction, though the best people academically aren't always the best psychiatrists clinically. I don't wish to imply that high academic achievement necessarily rules out high clinical competence, but the association is by no means complete. Often, I have met junior trainees and predicted to myself that this or that person is going to be a first-class psychiatrist, but I'm not sure what gives me this feeling of confidence. Knowledge of the scientific basis of psychiatry is absolutely essential, but by no means sufficient for high clinical competence.

I think there is at least one special patient you might want to mention.

There is one I can mention by name, because she has mentioned me by name in some of her writings, and that is Janet Frame, now a distinguished New Zealand writer and poet. Her autobiography, in three volumes, has been condensed into a very good film called *An Angel at my Table*. She was certainly a most amazing person to have as a patient. She had spent years in mental hospitals in New Zealand, and had seen the worst side of psychiatry. Consistently (and mistakenly) diagnosed as having chronic schizophrenia she had received more than 200 applications of ECT unmodified. But she survived this and remained a superbly intelligent, articulate, imaginative person, an original thinker, whose scope and confidence increased over the years. She has achieved many prizes, fellowships and honours. I was her registrar during a long admission to the Maudsley and I subsequently continued to see her and correspond with her, but I'm glad to say she needed no further psychiatric treatment. She was really quite ill but certainly did not have schizophrenia. She showed a most interesting interplay of original thought and imaginative awareness of her very rich inner experiences. She was a most instructive and rewarding patient, who publicly made generous (though not uncritical) attributions to myself and others, and to the Maudsley. She is in no way to blame for the fact that in the film, I was played by a New Zealand actor, whose name I can't remember but who seemed to take the view that I must have been a classy existential psychiatrist of the 1960s, with a shaggy beard and a rug over my shoulders, eating chocolates and drinking tea while talking to my patient. Fortunately I appear for no more than two minutes. I like recognition, but not of that sort.

In the mid-1960s, you wrote a chapter with Thomas McKeown, on the balanced hospital community and psychiatry, for a book I was editing with Jim Farndale. It seemed at the time that this was a very important idea for the future development of hospitals, but somehow nothing much ever came of it.

This was Tom McKeown's idea. His vision was, as you say, called the "balanced hospital community", and he applied it to the Queen Elizabeth Hospital site in Birmingham, where there was a lot of room for development, contiguous with a university campus. Tom's idea was that all hospital specialties should be represented on this site, through substantial building programmes, and that their structure and function should reflect the age-structure and the kinds of illness, acute and chronic, in the community. With that notion, there was another one – that hospital is only one phase of treatment for any disorder, and that the community services should be developed at the same time as the building of new hospitals. In this way, the whole campus would become a centre of operations for hospital and community work. He was always a bit vague about where general practice came into it, but this was in the days before the Royal College of General Practitioners. At that time, general practice was not a well organised specialty, and I think he failed to recognise that it would inevitably play a leading part in any comprehensive medical developments.

I was asked, while I was a lecturer at the Maudsley, if I would produce a project on the psychiatric component of the balanced hospital community. This entailed planning for services which included in-patient, day hospital, and out-patient care, as well as reaching out into the community, and which had to be multiprofessional and integrated as fully as possible with the other aspects of both hospital medicine and community services. I wrote several of the memoranda for that project, and I think it was at that time that you invited me to write a chapter for your book. But the idea didn't come to anything. If you go to the QE now, you see an enormous development, with the new specialist hospitals and units and a postgraduate centre, and it's certainly an impressive group of buildings, but I think it has failed to exploit community links, or hospital care for the less severe acute illnesses, or the problems of chronic disabilities. Perhaps this was because there wasn't the necessary staffing and leadership from the community, and because the GPs remained very much outside the hospital centre. There were loopholes in the plan, although McKeown was a man of great originality and flair – a pioneer. But he wasn't close enough to the practice of any kind of clinical medicine to be able to deal with some of the very difficult aspects. What community services can do and what they can't do nowadays is, of course, a very live topic and a difficult problem to work out.

In his later writings, McKeown played down the contribution that technical progress in medicine had

https://doi.org/10.1192/pb.17.5.260 Published online by Cambridge University Press

made to the improvement of health, as opposed to the advantages that come from high standards of living. As someone who has been very much at the receiving end of technical medicine, would you accept his view? Was his balance of opinion right on the developments of medicine?

As one who owes his survival to high technologies, I have to say no! But I would certainly accept his point that a higher standard of living and improved public health measures did a great deal to eliminate infectious disease, reduce certain deficiency disorders, and therefore to prolong life. The arguments he produced are absolutely valid, but he didn't foresee any of what we now regard as the medical problems associated with a higher standard of living or with longer duration of life, or the consequences of high-technology medicine, or the effects of cultural shifts. Ischaemic heart disease, for instance, has become more prevalent because people are living longer, and perhaps also because some are living a sort of life which makes them particularly prone to that kind of disorder. This is one of many new public health problems of the present day. Longer survival and altered social mores are causes of different patterns of morbidity in the population. Disease does not disappear when conventional public health measures become standard, and when modern treatments become gradually available. This was believed when the NHS was introduced in 1948, but it now seems absurd. I think Tom got some of it right, but he missed some of the rather big tricks in the game.

One of the main principles he expounded about hospital was that the structure should be flexible because needs would change and change quite quickly. Yet if one looks at the hospital buildings of the last 20 years, it seems that what has been done has, in fact, been quite the opposite. They are highly technical structures which are very difficult to change.

That is absolutely right. He wasn't able to foresee the technological revolution, if that's the right word, in medicine and nursing – the developments which require very expensive accommodation and staff and sometimes, as in intensive care units, expensive machinery. The idea of hospital architecture changing to represent functional change doesn't seem to have much said about it nowadays. You could mention mental hospital wards being converted into day hospitals, even the introduction of day surgery, but otherwise it's difficult to see how the structure should be reflected in the design or in planning for the future. Tom was associated with Llewelyn Davis, the architect; the whole of his department of architecture at University College at that time seemed to be dedicated to adapting structure to function. They used some of the biological models of growth such as could be found in D'Arcy Thompson's book *Growth & Form*, where you could use coordinate transformations to demonstrate how living things change in shape – their relative dimensions – as they get larger. It was a very interesting attempt at biological analogy, but I doubt whether it really has any pay-off in the foreseeable future.

Let's go now to 1967, when you left Birmingham. What did you do next?

I went back to the Maudsley as a consultant, and did a lot of clinical work during the following eight years. I had a unit in general psychiatry at the Maudsley and one at Bethlem, and also half of the psychogeriatric unit at Bethlem, which I shared with Felix Post, before Raymond Levy was appointed. I also became heavily involved in a certain amount of administration, and eventually became Chairman of the Medical Committee, a member of the Board of Governors, and so on. In those days, it was all transacted in a most gentlemanly fashion. It was before the managerial revolution, and I suppose still the sort of job it had been in the earlier days of Carlos Blacker, Brian Ackner, Denis Leigh, and Felix Post. I had a very satisfying eight years as a full-time consultant. When I returned to the Maudsley, Denis Hill had just been appointed to succeed Aubrey Lewis and he (Hill) was keen to involve some of the more junior consultants in teaching in his department. He appointed Alwyn Lishman and myself as First Assistants, with defined teaching responsibilities. That was all very satisfactory, but in the early 1970s, there was a move to establish a Chair of Psychiatry at King's College Hospital across the road. The Institute was very interested in seeing this Chair established, but the process went through several stages, one of which revealed that King's didn't have enough money to do it, but said they might be able to pay for half of the total expenses of a small department. So in late 1974, it was settled that there would be a joint Chair and that two senior lecturers, a lecturer, and secretarial staff should be a joint responsibility. It was suggested that I might apply for the Chair. Of course, I had observed academic departments growing up elsewhere, and had really set my face against a return to the academic world, feeling that my line was to be a practising psychiatrist and post-graduate teacher. But rather to my surprise, when this join Chair came up, I found myself interested by the unique opportunities it seemed to offer. So I applied and was appointed. I started in October, 1975 and then it was that I really began to know the meaning of trouble.

In what way?

The department at King's had been a small one in a general teaching hospital. It was called the Department of Psychological Medicine, and when I took

https://doi.org/10.1192/pb.17.5.260 Published online by Cambridge University Press

office, the senior consultant explained that it was altogether superior to the Maudsley which was only a *mental hospital*. This was a Department patients attended if they didn't want to go to a mental hospital, so that we saw more interesting cases – sensible people not lunatics – and practised a better standard of psychiatry. No mention was made of the fact that it was not at all representative in its clientele. This harked back to 1967, when the Maudsley had first taken responsibility for a district service. Before that, all the long-term patients who might otherwise have been admitted to King's or the Maudsley had gone to Cane Hill in Coulsdon, Surrey. The Maudsley's new district commitment meant that it would take responsibility for the acute management of patients living in the old London Borough of Camberwell, with a population of about 130,000. The other part of the health district was in the Borough of Lambeth, with about 100,000 people. In 1967, that remained part of the responsibility of Cane Hill, but already it was recognised that King's too would shortly become responsible for its own district.

So I moved into a situation where a self-satisfied little hospital department regarded itself as very superior to the Maudsley, and relied on the out-of-town mental hospital for providing the bulk of the psychiatric services. The psychiatric beds at King's had dwindled to a token number – four – on the neurological unit. However, a consultant from Cane Hill, John Hutchinson, had just been appointed to King's, as a first step towards King's taking on a district commitment. I was also told on my first day by the senior consultant that I would get the support of everyone in the department, but must recognise that nobody would go anywhere on the coat-tails (sic) of the Maudsley. A stony silence answered my protest that I had been appointed to forge academic links between the two places and was in the business of trying to integrate, rather than compete. So I was not popular at King's, and on crossing the road back to the Maudsley, I found I was not at all popular there either. Wherever I turned, I was a quisling. My friends weren't particularly inclined to support what I was trying to do and were suspicious of my motives. Neither King's nor the Maudsley wanted any kind of integration (Denis Hill, Jim Birley and Tony Isaacs were exceptions at the Maudsley, along with Nicky Paine the House Governor; Steven Greer was the only exception at King's). The arguments were that integration for teaching and research must inevitably lead to joint development of services, so that for King's, the much-valued identity of the department would be lost. For the Maudsley, it would lead to a larger district commitment at the expense of funds, staff, and accommodation for the 'special' services for which it was rightly famed.

When I started at King's, I decided to adopt three major objectives, which would take perhaps three years to complete. Firstly, there should be a joint training scheme; there were about 70 registrars and senior registrars at the Maudsley and 12 at King's. Creating a joint rotational training scheme would bring King's, with its general hospital psychiatry component, into the Maudsley circuit. This I thought should take about a year; in the end, the prejudices (on both sides of the road), together with the institutional barriers (the Maudsley was a Special Health Authority and King's was at the time part of an Area Health Authority, under the aegis of the SE Thames Region), caused it to take nine years.

The second objective was to have medical students coming over to the Maudsley on a regular basis; I thought that might take a couple of years. But it took ten years of pleading, memoranda, committees, and clandestine meetings.

My third objective was to have a joint service, rather than two separately administered ones. This I thought might take a little longer than the others – say three years. In fact, it had not been achieved when I retired in 1989, after 14 years, despite help from a number of sources, including the Health Advisory Service. So my plans can hardly be said to have got off to a flying start.

I'd hoped, of course, that the Camberwell Register, which had been developed by John Wing's MRC Unit, could be expanded to include the East Lambeth population about to be served by King's, but my preliminary proposals for that arrangement were most unwelcome. My friends were not interested in any of my proposals. You mentioned the downside of the Maudsley, and for me this was it. I was very surprised and disappointed. In the course of time, I had a lot of moral support from King's and made a lot of friends there, but there was no new money for these developments. Yet they had to take place before the joint Department could function as it should, providing a comprehensive service for its own District and a basis for teaching and research in general hospital psychiatry, as part of the Maudsley/King's post-graduate training programme. It was certainly the unhappiest phase of my career. I still enjoyed clinical work, did a lot of post-graduate teaching at both the Maudsley and King's, and reorganised the undergraduate teaching. There were ways in which the Department was successful, but in what one might have thought the essential prerequisites, it was still grievously underdeveloped when I retired.

Of course an unsurmountable problem was the Maudsley's near-total preoccupation with its 'excellence'. The lesson is that the pursuit of 'excellence' is on all fours with elitism and isolationism – both enemies of progress. It has nothing to do with meeting the challenges and opportunities of the day. Therefore, the concern with 'excellence', as opposed to impeccable practice, awareness of context, and

imaginative planning, can never succeed. The place had moved far from the Maudsley of Aubrey Lewis. As a result, in recent years the Maudsley has been forced to incorporate King's psychiatry, on very unfavourable terms.

We need to come now to one of your biggest areas of interest, which is your involvement in research. Of the many research areas you've been associated with, you said the first was human growth and development.

That was when I was a zoology student in my final year. Lancelot Hogben had designed a study and gathered a lot of data about human growth and development during the years from 10 to 16. The project was called Studies on Puberty, and my first assignment was to help with sorting and analysis of data for the first paper, which was on the qualitative changes during puberty. This was quite a complicated bit of analysis, my first venture into serious research, and as a reward for much labour, I was mentioned in a footnote. That was the way things were done at that time: I was very proud of this acknowledgement.

The second study was on the quantitative aspects of puberty – the growth spurt, along different dimensions of the body. The analysis of these anthropometric data was the main content of my PhD thesis – my very own project. What was hitherto not well recognised was that there is differential growth at puberty, not at the same rate along different dimensions of the body. For example, arm length and leg length don't increase at the same rate as height, while neck girth, shoulder girth and pelvic width don't change at the same rate, and so on. Not only was there variation between individuals, and different axes of the body, there were also striking contrasts between males and females. One was able to combine that with the previously published qualitative data and produce a sort of calendar of sexual maturation and its variations. The measurement principle I used was a development of a theory which Julian Huxley had made use of in animals – the 'allometric equation' – which is a way of relating growth along one dimension with growth along another. We were still in the era of desk calculators and Hollerith sorting machines, long before computers. So dozens of hours had to be spent, and careful thought had to be applied *before* setting out a detailed regression analysis rather than, as nowadays, afterwards. If you made what Bacon called a radical error in the first concoction, woe betide you, because you were liable to find you'd wasted three weeks' effort.

The next one, I think, was pedigree analysis.

That didn't occupy a very great deal of my time, but in those days, human genetics was at a relatively primitive stage. In that kind of genetics, a lot of the work is in the assignment of probabilities of particular family constellations, and thus making predictions regarding genetic mechanisms. It also provided the basis for an early form of genetic counselling.

What about the work you did for the MRC?

In about 1973, I was invited to be a member of the Project Grants Committee of the Medical Research Council's Neurosciences (Neurobiology & Mental Health) Board. An awful lot of the work was in basic sciences, but such projects as the MRC funded in clinical psychiatry also went through it. After about three years, I became Chairman and a member of the Board – the parent Committee. In due course, I became its Chairman, and a member of the Council itself. Those were interesting times, because they provided me with some rather high-powered seminars on a lot of subjects in neurobiology and basic medical sciences. It gave me an opportunity to affect developments in a very small way, and it opened my eyes to the range of basic sciences which might have a bearing on psychiatry. There was always a very large pile of documents to study before these meetings. I doubt whether this work really paid off in terms of the advancement of psychiatry at King's or the Maudsley, but that wasn't the object. I spent in all about eight to ten years with those activities with the MRC before I finally came to the end of my stint with the Council in 1981.

It sounds like a good time to have left.

That's absolutely right. The day had already arrived when many grant applications were highly rated and yet not funded, so that applicants naturally became very discouraged. The decisions and chances of success have become immensely more difficult in the last ten years.

One study that might be a bit more controversial was the MRC randomised controlled trial of treatments of depressive illness.

That was an earlier venture. The MRC had established the practice of multi-centre randomised controlled trials with streptomycin in tuberculosis, and in 1959, the hope was expressed by its Psychiatry Committee that the same method could be carried out on some of the newer treatments in psychiatry. The obvious candidates for this sort of process were the antidepressant drugs, since one needed to have an acute, time-limited condition. I was brought into this trial as an assistant, and it became my function to co-ordinate it and to carry out a good deal of the statistical analysis, during 1961–64. I usually spent a day a week in or around London attending to this.

The first problem was to recruit patients for the trial. They had to be in-patients in psychiatric hospitals or units, suffering from clear-cut depressive illness according to closely-defined criteria. There were meetings of large numbers of consultants, the first of which I remember was held in the Westminster Public Baths, and the attempt was made to secure their co-operation. The next stage was for visits to the mental hospitals, nearly all by myself, in order to go over the detailed procedures and listen to the objections of some of the prospective participating consultants. In that way, I was able to visit pretty well every psychiatric unit in all the four London regions and also in some of the hospitals in East Anglia, Cambridge, and Oxford. In due course, we collected enough patients for the trial, went through the procedures, obtained the data, analysed these, and presented the results to the Secretary of the Committee, Michael Shepherd. They were also presented to the main Committee, the Chairman of which was Sir George Pickering, who understood psychiatry as might a general physician. The paper was published in the *BMJ* in 1965. There were no names at the head of the paper but several, including my own, in a footnote. The results were that for moderate to severe depressive illness, the most effective of the treatments on trial was ECT, imipramine following a close second and thirdly, placebo and phenelzine, differing very little for that particular kind of illness in that setting. Naturally, there were many people who didn't find this result very palatable, and William Sargant was quite tireless in his attempts to discredit our methods. Nevertheless, the trial was accepted as at least the first plank in the modest edifice of multicentre controlled studies of treatment in psychiatry in this country.

Your conclusion about phenelzine was not that held by most clinicians who had used it. Do you think there might have been some problem about the dosage or the selection of patients?

The selection of patients is a big limitation in any clinical trial, and this was no exception. We had to select patients with moderate-to-severe depressive illness, requiring admission to hospital, and for whom ECT could be a form of treatment. So they were pretty ill people, and yet of course, they had to be people who might start with no treatment other than a placebo. It was a restricted group, by no means representative of all depressive disorders. If we had been able to construct a stratified trial with more people and a wider range of illnesses, we might have seen some differentials according to the severity and type of disorder, and phenelzine might well have proved to be more effective than it was in our particular series of patients.

I think your next project was an attempt at a randomised controlled trial for the assessment of psychotherapy?

That was in 1969–70 and I think the first, last, and only research collaboration between the Maudsley and the Tavistock. The question was – can you assess the efficacy of dynamic psychotherapy by a controlled trial? The answer, in a word, was no. Our feasibility study was worthwhile in demonstrating this. There were a number of reasons why the method proved to be inapplicable, one of which was the problem of choosing a suitable control treatment. People included in the trial had to be deemed by psychotherapists as likely to benefit from dynamic psychotherapy, but they had also to be seen as at risk of receiving a control treatment which included only supportive treatment. This put rather a strain on the psychotherapists who, for regrettable reasons, start off with the firm conviction that their treatment is effective and therefore necessary for some people. That selection factor meant that it was possible only to include a very small and atypical series of patients in the study. It wouldn't have done at all for the very expensive business of a randomised controlled trial with long-term follow-up. One other limitation was in the large number of criteria of change and improvement which would have been necessary, including not only those of descriptive psychopathology – the symptoms and social adjustment – but also dynamic variables. These were agreed upon only after very difficult arguments among the research group, and they were less easy to establish in a reliable fashion than were the more descriptive criteria. So our pilot study was successful, but the answer was negative.

There were also some projects on psychiatry in general hospitals.

That was when I went to King's. The first that I myself was involved in was with Geoffrey Lloyd, looking at psychiatric sequelae of myocardial infarction; he was at that time working in my department, and wrote several papers on the topic. In other projects, my main role was as an initiator or adviser, and subsequently as a person mentioned in footnotes. I've been mentioned in quite a lot of footnotes in my time. Unlike many professors, I believe that authorship of papers should be restricted to those who have done really substantial parts of the work.

We should now come on to one of your most important areas of work – in connection with education and examinations. You were, of course, Chief Examiner for the College.

I became Chief Examiner and Chairman of the Examinations Sub-Committee in 1981. That was

https://doi.org/10.1192/pb.17.5.260 Published online by Cambridge University Press

when James Gibbons retired from that office, and it was already being promulgated that changes should be made to the examination. In fact, what was required was a root-and-branch revision of the whole thing, so as Chairman of the Examinations Sub-Committee, I became Chairman of the Working Party for reviewing the examinations, and our work took nearly five years. We had a very good group of about eight people (including a representative of the Collegiate Trainees Committee), all of whom worked very hard on reviewing the previous examination, and possible modifications for the new one. We produced our report, which then had to go through the Court of Electors and the Council. It was accepted, and the new examination was phased in from 1986, over a five-year period to 1991. I think the examination is due for further review, which I hope may not have to be as radical as the last one. We continued to have the clinical as the central part of the examination, though steps were taken to improve the reliability and uniformity of standards. As far as multiple-choice questions were concerned, we agreed that these were the best way of testing certain kinds of factual knowledge. So the MCQ was considerably revised, the MCQ bank was diversified and cleaned up, and certain rules were devised for setting MCQs and testing their performance. This provided opportunities to test reliability and validity. We set great store on the reliability of all other parts of the examination by introducing quite elaborate schemes for the training of examiners and monitoring the results.

The biggest change was in the distribution of the examination between the first and the second parts, because hitherto, the first part had been devoted to basic sciences and only after passing that did the average student get on to serious reading on clinical psychiatry. That was the wrong way round – too reminiscent of the traditional procedure in the undergraduate curriculum – and we introduced a clinical in the first part of the examination, together with an MCQ on clinical topics. The second part includes the basic sciences, as well as the more advanced assessment of clinical knowledge and skills. One other big change we made was in the essay paper. Previously, this had included questions inviting a display of factual knowledge, and it had been very difficult to obtain a reliable way of marking these, because there was so much muddle between factual knowledge and opinion, between reasoning and presentation, and so on, in the answers. So it was agreed that factual knowledge should be tested by the MCQ or the newly introduced Short Answer Questions (SAQ), while the essay would address a broader topic, the candidate being required to answer one question out of a choice of six. This would display the standard of their presentation, argument, and reasoning. Another change was in the oral examination in Part II. Previously, this had been a random collection of thoughts which came into the examiners' heads, which the candidate would be questioned about. We introduced the ruling that the viva would focus on very specific problems of managing patients, and that examiners would prepare themselves for this by having their own card index of about a couple of hundred case vignettes. We talked for a long time about having a second clinical, not based, as the main clinical had always been, mostly on hospital in-patients, but to include patients seen in general practice, out-patients, or in wards in a general hospital, with a wider range of psychiatric disorders. However, with 300–400 candidates twice a year, it would not have been possible to organise second clinicals in all the centres or to recruit suitable patients for them. So the oral on patient management problems was the nearest we could get to the second clinical examination in Part II.

As Chief Examiner, I would imagine you have had some experience of other higher examinations. How do you think this College compares with the others?

I feel very optimistic. The conjoint DPM changed very little from its traditional pattern. Several University departments have introduced master's degrees, usually with emphasis on research methods and a project – but these are supplementary to the MRCPsych, not substitutes. The Australasian College of Psychiatrists has a very good high-level examination (MRANZCP), including case books and advanced knowledge and experience, which differs from the MRCPsych in being an exit qualification. After passing it, the candidate looks for a consultant post. After passing the MRCPsych, a further three or four years higher (senior registrar) training is required. However, I am glad to know that steps are to be taken to harmonise the two. Among other specialities, the MRCP (UK) hasn't very substantially departed from its old form, but of course, it can be said to serve its purpose as an examination in general medicine. An examination in psychiatry should be very different, and I think we have got away from any stereotype we might have taken over from the MRCP. A nearer one for comparison might be the MRCGP, which is obviously immensely difficult to arrange, because of the diversity of general practitioners' work and the types of decisions they have to make. They have worked very hard on their examinations, and now they have a very well constructed one. I would be confident in saying that the MRCPsych is the most appropriate examination feasible for its subject in today's climate.

I mentioned that on the review Working Party, we had a member of the Collegiate Trainees Committee, who was responsible for finding out what the trainees thought about various issues. One thing which

surprised me very much was that the trainees didn't want to have local examinations or rely on local assessments by consultants with whom they had worked. They wanted a central one, rather than to be assessed by their own tutors and teachers. I think that was an important principle, because it can so easily be argued that the exam should be based on local practice and cumulative performance, with either no test or only a simple one. Nevertheless, if such an arrangement might be introduced some time in the future, it is likely that standards would vary considerably from one locality to another.

As an individual, you have perhaps experienced being something of a surgical battlefield and you have had long periods of illness, from you adolescence onwards. Do you think this has influenced your general outlook in the way you practise medicine?

It must have done. I've mentioned the long period in hospital in my adolescence. In later years, I had a connected series of misfortunes. I had a partial gastrectomy of the Polya type when I was 33. Ten years later, I had to have my gall bladder removed and that became complicated by the scarring round the blind loop of the duodenum left by the gastrectomy. Although my surgeon could not have been more eminent in his field, he told me it had been a long and difficult operation and he believed he had left a stone behind. But I remained very well until, ten years later, he was proved correct when I had an obstructed pancreatic duct and acute pancreatitis, pancreatic abscess, pseudocyst, subphrenic abscess, and empyema – a whole lot of nasty things which kept me in hospital for more than six months. Some people write books during their illnesses, but I was never able to concentrate my mind for that. However, I've had plenty of opportunity to observe doctors, nurses, and patients, and I suppose I learnt a lot from my experience, though so far, I haven't experienced the psychiatric battlefield. I have probably become rather sensitive about what is good and bad practice in doctoring and nursing, but I don't know how my ideas can be generalised or how they might have affected my own practice. One learns something about oneself from being a patient, but perhaps not a great deal about how to practise medicine.

One of the subjects you've thought about is team work in psychiatry. Where do you feel that has got to at present?

I think psychiatric practice has reached the point at which it is recognised that at its best, it's a multiprofessional affair, so that the psychiatrist works best in association with a team of people. These include the psychiatric nurse, who is now much more than a hand maiden, the social worker, the clinical psychologist, occupational therapists, and specialists in rehabilitation. A psychiatric service of the best kind would be one in which all those people work together in assessment, management, and prognosis – identifying a patient's needs and doing as much as they can in meeting those needs. This means that they make decisions jointly, as well as practise in harmony with each other. I think that trend is one that many psychiatrists would agree with, as a most desirable move away from the hegemony of the psychiatrist in the management of mental illness. However, the trend has been rather tripped up by the fact that the other professions are now developing not only technically, and in their training and career structure, but also a need to go it alone and to be in competition with each other. The result is that social workers, for example, sometimes only very reluctantly work with psychiatrists; psychiatric nurses, if they are community nurses, may prefer to work with general practitioners rather than psychiatrists; clinical psychologists often compete rather than collaborate with psychiatrists, and the whole aspiration for team-work has tended to be thwarted. I feel sad that the opportunities for team work have diminished because of this fragmentation.

What can be done about it?

I would like to see some policy discussions at the highest level. Perhaps the College should talk about this with the other professions, in order to establish something like a code of multidisciplinary practice, emphasising the duties rather than the 'rights' of each profession. It would be a code for multi-professional division of labour and collaboration in the interests of the patient or client, and although one could foresee difficulties in agreeing details, it shouldn't be a hopeless task. Otherwise, we are going to continue to have a position where there's a lot of repetitiveness and fragmentation, rather than integrated effort. And that means a lot of wasted time, energy, and resources. It may also mean that a lot of the really important clinical decisions will be made by managers rather than practitioners, of whatever kind.

You are preparing a lecture, I believe on, 'Is psychiatry more than a science?' Is it?

It was a title I chose myself, and I have some misgivings about it. Psychiatry is very clearly based on the biomedical and psychosocial sciences. There's no doubt about psychiatry's scientific basis and about the huge advances that have been made in recent years, so it can certainly be dignified by being called an advancing clinical scientific discipline.

However, I do believe there's significantly more to psychiatric practice than science. What that extra is, though, I find difficult to characterise. But it seems to me important that somebody should be thinking about this.

Can you take it any further at this stage?

It's often said, of course, that medicine itself is an art as well as a science; that is rather obviously true. I believe, though, that in psychiatry, the non-science component – the X factor – is bigger than and different from what it is in the rest of medicine. Some people would claim that this X factor can be understood only by resource to psychoanalysis. I strongly dispute that. I suspect it can be defined as something more primary, more basic to human experience than psychoanalysis, which is a relatively late invention. There are those who would say that the X factor – if it is admissible to concretise it for the purpose of this argument – is that aspect of psychiatry which belongs to the humanities. I can see the point of that argument, but I think it should be taken further. For the sake of our understanding of mental illness and its management, I hope somebody will be able to crystallise it one day, and I believe it will remain outside the purview of science.

In passing, you mentioned psychoanalysis in relation to psychiatry. It has been said that psychoanalysis is not so much a doctrine or body of theory, but more a climate of opinion – something which is now an integral part of our culture. Would you accept that view?

Certainly. Whatever role psychoanalysis may have as a treatment in itself or in the general run of psychiatric problems, it's fundamentally a way of looking at human experience – a viewpoint which has become assimilated into our culture. Perhaps its importance to psychiatric disorder is small, compared with its significance in cultural and social anthropological studies and in the imaginative literature of successive periods of human history. Aside from that, though, it is sad that within psychiatry, even nowadays, there is so much antagonism between those whose practice is based on psychoanalysis (exemplified by the Tavistock Clinic) and those who foreswear it, believing it to be extinct (exemplified by those who base their practice on neuroscience alone). The dogmatism, on both sides, horrifies me. Perhaps the majority of psychiatrists are sensible people who occupy the middle ground and sometimes admit to uncertainty. I fervently hope this is the case.

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