

Remembering Robert Kendell

BY JOHN COOPER

The purpose of this piece is to pay a personal tribute to Bob Kendell, with special emphasis on those parts of his work that have been in an international setting. He carried out a variety of such studies as an individual researcher, and also was a member of the UK half of the team that carried out the first phase of the US–UK Diagnostic Project from 1966 to 1971. In another section of this tribute, Norman Sartorius comments upon Bob's contributions to the programmes of the World Health Organization.

Bob Kendell was a man of exceptional ability and diverse interests, but perhaps the overriding impression he created in his colleagues was one of calm confidence and competence, expressed in comparatively few words. Very few psychiatrists enjoy dealing with data analysis and statistics, but Bob seemed to have a natural affinity for the analysis of large sets of data. This made him a particularly valuable member of the team of the US–UK Diagnostic Project.

Two themes are prominent in his publications, one being a search for points of discontinuity between symptom patterns (the basis of both his own MD thesis on depressive syndromes, and several of the papers that he wrote with Jane Gourlay using the data from the Project) and the other being the investigation of influences upon the diagnostic process – such as identification of sources of potentially harmful bias, and the speed of decision-making. His interest in sources of bias in clinical decision-making was evident from one of his earliest studies, done before joining the Project. He did a neat study showing that the preconceptions of psychiatrists about the existence (or not) of depressive syndromes can have a powerful effect upon whether they rate the constituent symptoms of the syndromes as present. But he was frustrated to find that his paper was

rejected out of hand by the then Editor of the *British Journal of Psychiatry*, who was a man of rather definite opinions. The paper was eventually published elsewhere, but it deserved a more prominent place in the international literature. I think that this experience was one reason why the prospect of joining the Project team was specially attractive for Bob, because we knew from the start that it would be necessary to do studies on a sufficiently large scale for it to be impossible to ignore the findings just because they went against the received truths of that time.

Bob and I were several years apart in the training programme at the Institute of Psychiatry, so at first I did not know him well personally. But we both enjoyed playing squash in the courts at King's College Hospital across the road from the Maudsley Hospital, and over a drink during the recovery phase after our games, he told me about his study of patients with depression. In this, it had been necessary for him to do hundreds of correlations using the then standard electric (but not electronic) machines, which required the data to be entered by hand. It was actually in the changing room of the squash courts that I asked him whether he would be interested in joining the Project team. Soon after this, Michael Shepherd suggested that an interesting young visitor to the Institute called Norman Sartorius might also be usefully employed, and so the UK team for the first Brooklyn–Netherne study began to take shape. To handle the data from the large-scale studies that were needed, there could be nobody better than Bob Kendell, fresh from many solitary months of number-crunching on the data of his own MD thesis. But now he could have a full-time programmer to help, and unlimited access to the main-frame computer. The cooperation between Bob and our next recruit, computer programmer Jane Gourlay, proved to be one of our success stories.

A few words on the Project itself are justified here, to show what a valuable member of the team he was because of his special skills. The US–UK Diagnostic Project had its origin in several years of negotiations in the early 1960s between the New York State Psychiatric Institute (Joseph Zubin), the Institute of Psychiatry (Aubrey Lewis) and the National Institute of Mental Health (Morton Kramer). Anybody who had worked in both the UK and the USA in the 1950s and early 1960s quickly formed a strong impression that most American psychiatrists had, by European and particularly by British standards, a remarkable propensity to give almost all seriously ill psychiatric patients a diagnosis of schizophrenia. Comments on how this was reflected in admission statistics to mental hospitals had appeared in the literature, but isolated comments and personal impressions are quite different from information that might change the diagnostic habits of clinicians. A few papers had also been published comparing the diagnostic habits of British and American psychiatrists, but they had had little impact, mainly because they were on a small scale.

Morton Kramer had set the scene in detail by his paper in 1961 showing that in the admission statistics to state mental hospitals in the USA, the rates for schizophrenia for most age groups were about five times those in the UK, and for some age groups other differences were as much as twenty times. The probability that this was due to differences in the patients was very low, so our tasks were simple and obvious. First, the sources of the differences in hospital admission statistics had to be identified, and second, any differences in the diagnostic habits and biases of the psychiatrists needed to be described in detail. All of us on the Project naturally hoped that our results would be accepted as important, but I do not think that anyone anticipated the far-reaching effects that became evident in the following years. My own view is that our results gave an important final impetus to a process that had been building up for some time. Psychiatry, particularly in the USA, was due for a sea-change because of frustration with the then dominant psycho-analytic model. The Project team just happened to be the right people in the right place at the right time. With a team that contained Bob Kendell and Norman Sartorius, and later John Copeland, it would surely have been surprising if the Project had not been a success.

The first main findings of the Project were published in a series of papers that were necessarily team efforts, and in which the New York members, led by Barry Gurland and Larry Sharpe, played a full part (Kramer, 1969). In all of these, and in the production of the eventual monograph summarising several years of work (Cooper *et al*, 1972), Bob played a major role. He was the only person I have known who could sit down at a tidy desk with a pen and a blank sheet of paper and write in a neat and legible hand what was a coherent first draft.

After leaving the Project, Bob continued to pursue a variety of topics with international ramifications, and among these his interest in puerperal psychiatric illness was unusually productive. Together with Ian Brockington and others, he was a founder member of the Marcé Society, and Bob's study on the epidemiology of puerperal psychosis is still regarded as one of the foundations of that whole topic. He also continued the theme of studying the diagnostic habits of psychiatrists in other countries in a cooperative study with Pierre Pichot (Paris) and Michael von Cranach (Munich). I remember meeting him one day while he was trying to work out what to say in a lecture in Paris, which he was proposing to deliver in French. A long-lasting interest in problems to do with alcohol was recognised recently by his election to the presidency of the Society for the Study of Addictive Substances.

In the USA he was highly regarded. Having met Robert Spitzer in New York during the years on the Diagnostic Project, he soon became an adviser to some of the task-forces of DSM-III. This link continued with DSM-III-R and DSM-IV. In a recently published book of reviews on research prospects for DSM-V, Bob was the only European academic psychiatrist among the 46 contributors. The award of

the Paul Hoch Medal also demonstrated his special reputation in American eyes.

It will be some time before the sad fact that Bob is no longer with us has sunk in, particularly for people who had periods of working closely with him. Even though the Diagnostic Project was a long time ago, I have had a useful habit over the years of saying to myself, when faced with a tricky problem about data, 'Now, what would Bob have said about that?' Some of our colleagues live on inside our minds, and Bob is surely one of these.

Kramer, M. (1969) Cross-National Study of Diagnosis of the Mental Disorders: origin of the problem. *American Journal of Psychiatry*, **125** (suppl), 1–11.

Cooper, J. E., Kendell, R. E., Gurland, B. J., et al (1972) *Psychiatric Diagnosis in New York and London*. Maudsley Monograph 20. Oxford: Oxford University Press.

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BY NORMAN SARTORIUS

I met Bob in 1966 at the Institute of Psychiatry in London, and had the pleasure of working with him on the US–UK Diagnostic project. This was, I think, Bob's first involvement in an international scientific investigation and I must have been the first foreigner with whom he worked in such an enterprise. I enjoyed the time we spent together and learned a great deal from him. He often used to pick me up and take me to Netherne Mental Hospital, the UK site of the first phase of the Diagnostic Project. He did this using his Vespa motor scooter. Most mornings of that winter were bitterly cold and I did not enjoy the ride in the slightest. When I cautiously complained, colleagues told me that Bob was a man born with the gift of

imperturbability, a gift no doubt valuable in his mountaineering and sailing endeavours in various parts of the world.

The gift of imperturbability did not apply only to meteorological conditions. It was equally present and valuable in his contacts with people from different parts of the world and of different professional backgrounds. Where most others would find it difficult to adjust to working in a new setting and with people from other cultures, he established productive and agreeable contacts and friendships. Although taciturn he had the gift of listening, and he had much respect for other cultures and traditions, always taking his time to learn how to work harmoniously with those steeped in them.

These qualities were equally evident subsequently in his many activities in collaboration with the World Health Organization. For several decades he was a member of the WHO Expert Advisory Panels on Mental Health and Drug Dependence and Alcohol Problems. When invited to chair meetings or to act as a consultant he invariably delivered faultless contributions marked by practical wisdom and understanding for the situations in which his advice was to be used. He made important contributions to the ICD-10, and also advised on the programmes dealing with alcohol and drug problems. In more recent years, his wider interests led to his being an adviser on mental health service development, and projects concerning the improvement of undergraduate and postgraduate psychiatric education. He will be remembered by psychiatrists and others in many countries across the world as an outstandingly knowledgeable person, a man of integrity, a reliable friend and a versatile and practical source of advice and help.

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