Perspective

An occasional series in which contributors reflect on their careers and interests in psychiatry

A Contribution by F. Kräupl Taylor

I was 34 when I arrived in England in May 1939, having left behind me a career as a specialist in internal medicine in Czechoslovakia and still smarting from my experiences in the concentration camp of Sachsenhausen and some later encounters with the Gestapo. The relief of finding some temporary security in London was, however, soon tempered with the boredom and frustrations of a purposeless existence. My efforts to escape from this limbo were eventually successful, when the Quakers at The Retreat in York offered me hospitality and I undertook to look after their pathological laboratory. This was my first opportunity to mingle with psychiatric patients and to form ties of friendship with some of them, learning about their problems and difficulties, and witnessing the ups and downs in their clinical conditions. Thus my introduction to psychiatry started on a very personal level.

My activities in the laboratory were not too arduous so that I had plenty of time on my hands. I decided to engage in a study of statistics and psychology with glimpses into some areas of logic and philosophy. Mathematics had always had a special attraction for me, and I soon found that I spent most of my time in the laboratory reading Yule and Kendall's *Introduction to the Theory of Statistics*, and enjoying it greatly.

These studies came to an end in 1941 when the authorities put me on a Temporary Medical Register so that I could look out for work as a doctor. As there was no opening for me at The Retreat, I obtained a post at the Crichton Royal, Dumfries, where I came in contact with such well-known psychiatrists as Willi Mayer-Gross and Erwin Stengel. I could concentrate on psychiatry now and in 1942 took the London DPM. My interests then turned to psychotherapy and psychoanalysis, and to obtain experience and instruction in these fields I moved in 1943 to Netherne Hospital Coulsdon. This was near enough to London so that I could start a training analysis with Willi Hoffer and attend evening discussions at Anna Freud's house. However, these activities came to an early end, leaving my psychoanalytic training in mid-air. I then became interested in group therapy which offered the promise of coping more adequately with the therapeutic demands of a busy psychiatric hospital and its out-patient clinics. I tried to model my approach to group therapy on the lines advocated by S. H. Foulkes. But I was not fully satisfied with a merely psychodynamic evaluation of group processes. There were other influences on the groups which had a potential therapeutic value, such as those that emerged from the network of interpersonal reactions and from the patient-audience relationships. These group events had the advantage that they were amenable to measurement by adapting the sociometric procedures originated by J. L. Moreno. There were problems here which I found absorbing. Yet to obtain acceptable results from my sociometric investigations, I needed more therapeutic groups than were available to me at Netherne. The opportunity to expand my studies came after my translation to the Maudsley in 1947. I could then organize seminars on group therapy and examine sociometrically the groups conducted by others. In this way I eventually obtained data from 28 groups consisting of 208 members. I managed to refine the mathematical approach needed for my group assessments. Among my innovations was the derivation of a formula for a coefficient of concordance among group data, which I published in 1951. It was a modification of a similar coefficient that had been worked out originally by M. G. Kendall in 1947. A psychologist in Minnesota published the same modification, though in a less general form, in 1956. I could, and did, claim priority.

The sociometric findings highlighted the social characteristics of patients within this particular group setting, their interpersonal relations, and their status in certain public hierarchies. Judged by the coefficients of concordance, the public hierarchies of dominance and popularity could be measured with the greatest reliability. Public dominance correlated highly with verbal initiative; in male-female groups it was slightly, but significantly, greater in men than in women; and it had no relation to therapeutic outcome. Public popularity correlated highly with group attendance; it was equally shared by men and women in male-female groups; in all-male groups, patients were significantly less aware of their popularity status than was the case in all-female and male-female groups (which was in accordance with some other findings and suggested that homosexual fears inhibited the open expression of feelings among men); and finally there was a positive correlation of 0.70 with therapeutic outcome. The correlation between dominance and popularity was +0.51 which was not high enough to obviate discrepancies of status. One of these discrepancies was clinically important. It concerned patients with high dominance and low popularity. They tended to leave the group prematurely for reasons that seemed genuine at the time. Yet it was found that the likelihood of their departure could be reduced when the therapist predicted early enough

that such a patient would find good reasons for leaving the group. Such 'prophylactic predictions' have since proved to have a wider therapeutic applicability.

Much attention was devoted to the verbal interchanges in a group and to the topics discussed. For this purpose, some group sessions took place in a sound-proof room with microphones hanging from the ceiling and with observers behind a one-way screen surveying the patients and tape-recording their discussions. In the end we were deluged by yards and yards of tape. Sifting through them was a time-consuming and exhausting job. Still, with stringent editing we succeeded in slimming down the discussions to the size of a brief novel, and this was originally incorporated in the manuscript for my Maudsley Monograph on *The Analysis of Therapeutic Groups*. But Aubrey Lewis very wisely insisted on a further compression to the size of a short story. This was just as well as it seems that the sociometric content of the Monograph attracted most attention. In a pirated Spanish edition which appeared in Buenos Aires, the sociometric Appendix of the Monograph was fortified by copious footnotes by an Argentine professor of statistics.

There was yet another fascinating aspect of group activities, namely the phenomena of collective emotions, crowd behaviour and mental epidemics. J. H. Rey and I explored an occasion when a scapegoat motif was enacted in one of his groups. There was also an instance of a minor mental epidemic which occurred among the female patients of a Maudsley ward and was analysed by R. C. A. Hunter and myself. The patients showed an excited preoccupation with ideas of pregnancy and childbirth, and there were some transient conversion-hysterical symptoms. At a later time, when I gave a talk to the British Association on collective emotions and mental epidemics, I mentioned that certain fashions, fads and crazes could assume epidemic proportions. I gave as one of my examples the study by Richardson and Kroeber on how the length of skirts had varied periodically in the course of 300 years of female fashion. This inspired the *News Chronicle* of the next day to the headline 'Short Skirts are Mental Epidemic'.

For various reasons my interest in therapeutic groups was supplanted by an interest in the tricky problems posed by certain young female patients who alleviated their feelings of tension by repeated acts of self-damage, such as cutting and burning themselves, and risking death in suicidal ventures. They prompted me to review the rationale of analytic psychotherapy. It was obvious that the truth of analytic interpretations could not be objectively verified. It rested only on theoretical beliefs which varied with fashion, factional allegiance and personal predilection. But the lack of an objective truth criterion did not rule out the possibility of these interpretations having therapeutic merit. To my mind, they seemed to have two potentially therapeutic functions. They could be guiding interpretations inciting patients to search for a theory-inspired understanding of their symptoms. They could also be emotionally challenging interpretations which linked symptoms with disreputable or revolting motivations so that the patients would have a nagging incentive to rid themselves, if possible, of phenomena with such an unsavoury background. I deliberately began to use a variety of upsetting interpretations in the treatment of our self-damaging patients, thus challenging them to discontinue practices that had such shocking connotations. I was, however, disinclined to speak of 'challenging' psychotherapy, as I regarded psychotherapy as challenging in the first place to the therapist who had to find the right timing and phrasing for his interventions. 'Challenge' psychotherapy also seemed off-key, for it sounded like a battle-cry of the anti-psychiatry lobby. So I went to the Greek dictionary and christened this form of psychotherapy 'prokalectic' (from *prolaleo*, to challenge or provoke). One of our staple interpretations designed to halt the practice of skin incisions was to stigmatize them as perverted forms of masturbation. This was usually fairly successful, if we had chosen the right time when a positive transference had been established. Even then the interpretations had to be repeated and there were emotional upheavals which needed all the tact and devotion of our Ward Sister Winicki to keep in check.

In 1949, I had come across the fifth German edition of Karl Jaspers' *Allgemeine Psychopathologie*. It was printed on poor post-war paper and had a flimsy binding which was soon in tatters as I ploughed my way through the difficult jungle of Jaspers' views. It left me with a permanent interest in descriptive phenomenology which eventually bore fruit in my book on *Psychopathology*. Among the phenomena mentioned in the book, some were unlikely to come to the attention of psychiatrists, even though they could have troublesome consequences. One of these was the phenomenon of cryptomnesia, which could cause much embarrassment when it issued in unconscious plagiarism. Another phenomenon was that of *penis captivus*. I began to wonder whether it had ever occurred or was just a titillating hearsay story. It turned out to have an exceedingly rare basis in fact, as emerged from my search of the literature and a letter to the *British Medical Journal* by Brendan Musgrave, a London doctor, in response to a paper of mine on the subject. The phenomenon of pseudo-hallucinations only found a place in the second edition of my book, but needed further clarification later to disentangle two incompatible meanings it had received which I distinguished as the English and German meanings, referring respectively to perceived and imaged manifestations.

My interest in psychopathological phenomena was coupled with an interest in the concepts of health and disease. Past generations have often seen health as God's gift to men in a state of grace. Diseases were then frequently viewed as malevolent or maleficent physical entities of autonomous existence which could impinge on human beings and convert them into hidden or open 'disease carriers'. Yet neither health nor disease are physical entities of autonomous existence. They are only attributes of such physical

entities as living organisms. There is therefore no question of searching for the essential nature of the non-entities of good health and disease. The task before us is to find such criteria as will differentiate the attributes of health and disease, and such criteria are bound to be attributes of attributes, i.e., attributes of the second degree. In tackling this task I restricted myself to the physical entities that are human beings and enlisted the help of the logical theories of classes or sets. The problem then turned into the question: by which criteria can the class of human beings be divided into two mutually exclusive and complementary subclasses consisting respectively of healthy and diseased persons. Because of the unavoidable existence of borderline persons in whom the differential diagnosis of health and disease cannot be definitely decided, it is impossible to make our subclasses complementary. However, it should be possible to find criteria which will achieve mutual exclusiveness for the two subclasses of definitely healthy and definitely diseased persons. There have been a few relevant suggestions which have often given preference to attributes of the second degree which are objectively ascertainable. It seemed to me, however, that this preference has not yielded acceptable results. One has to lower one's sights and content oneself with subjective criteria. In this spirit I recommended as a solution that the criteria should consist of attributes which are abnormal by population and/or individual standards, and that they should also exhibit at least one of the following features: (a) therapeutic concern for himself experienced by a person; (b) therapeutic concern for him experienced by his social environment; and (c) medical concern for him experienced by his doctors. Whatever the shortcomings of this solution it has the advantage that it mirrors what happens in actual practice.

Mental Handicap: Observations on Current Discussion

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Current discussion on the problems of mental handicap include the frequent repetition, as if new, of material accepted as commonplace and non-controversial in the past fifty years. A summary of such material is produced and used to investigate the phenomenon.

The phenomenon

For the past several years the staff of mental handicap hospitals have been obliged to study and discuss a great quantity of advice and instruction from a variety of sources. These include documents of policy, Departmental advice and the publications of the National Development Group. Each hospital will have been visited by the Hospital Advisory Service, and in more recent years by the National Development Team. In addition, when events lead to a formal Committee of Inquiry into a particular hospital, the resulting report is studied by every other hospital, often by the request of Area or District Management requiring written conclusions and recommendations resulting from such study. The flow of 'advice' continues without there being any critical investigation of the overall character and content.

The past few years have indeed seen a number of technical advances in the field of mental handicap. Examples include the growing use of 'sign language' techniques to foster improved communication ability in the most severely handicapped, the increasing use of behaviour modification, a widening scope and effectiveness of genetic counselling, and the general availability of anticonvulsant blood level monitoring. These advances anticipate, rather than follow, any directives that come from 'on high'. There seems, then, to be a stream of progress quite separate from the stream of Good Advice. I have resisted the temptation to use inverted commas, that is 'good advice', as the advice is very rarely in any way bad, merely ineffective.

I believe certain analogies can be made that contribute some insight into the present futility of much current discussion regarding mental handicap.

Repetition

The amount of repetition is prodigious. Speaking technically, a neutral stimulus continually repeated without reinforcement induces cerebral inhibition. In this context I maintain that 'old advice', repeated frequently, cannot evoke a response other than boredom or resentment, and any gold will lie hidden in the dross.

Signal-to-noise ratio

Signal-to-noise ratio is a measurement familiar to many in its commonest context of domestic high fidelity sound equipment. The concept can be used in any communication system, and in ours anything that does not add to the quantity or clarity of a message is noise rather than signal. Our vast background of 'old hat' is noise, and where the signal-to-noise ratio is very low the message, or signal, is not heard at all.

The test

Where the signal-to-noise ratio falls below a certain level the addition of more noise will be entirely unnoticed. I tested this by ensuring that certain of my own contributions to the system were, in my terms, without meaning or value. However, I realized that, on its own, this proves nothing, and that I should have to introduce into the system a 'paper' that