HANDBOOK OF CLINICAL NEUROLOGY Vol. 18: 

This volume of the Handbook, the third on tumours of brain and skull, continues the high quality of its companion volumes. Clinical parts are somewhat dated by being pre-EM1 scan. Unnecessary duplication, criticised in previous volumes, is reduced to a minimum and we are given a series of excellent monographs which will be the definitive account for a number of years.

J. A. SIMPSON

THE FUNDAMENTALS OF PSYCHOLOGICAL MEDICINE

This is a remarkable attempt to compress the basic data of psychiatry into 282 pages. The authors state (p. 1) that the ‘contents were originally formulated by the needs of postgraduate colleagues who have attended our courses at the Oxford Institute of Psychiatry’. Looking at chapters in sequence one must say that anatomy (chapter 2) is uninspiring with too much emphasis on the anatomy of the spinal cord. Chapter 3 ‘physiology and biochemistry of the nervous system’, is clear, dogmatic and relevant. Chapter 4 is too long and too general for the psychiatric postgraduate. Chapter 5, ‘man as an individual’ is very readable and pertinent, but chapter 6, ‘social man’, consists of a mass of sociological platitudes. Chapters 7 and 8 were reasonable and chapter 10 ‘critical ages of man’ was most interesting. It was chapters 9 and 12 which provoked some anxiety. Chapter 9 ‘psychiatric disorders and their classification’ includes the outdated subdivision of schizophrenia into simple, hebephrenic, catatonic, etc., and fails adequately to present either Schneider’s views on the first rank symptoms, or the more recent emphasis on operational definitions. Again (p. 198), there is a quite antiquated reference to paranoia and paraphrenia, surely concepts chiefly of historical interest. There is reference to depression and mania in this chapter but no adequate description of bipolar or unipolar manic-depressive illnesses or their management. In chapter 11 we have again reference to ‘historical’ drugs—for example, amphetamines, reserpine, and LSD, whereas neither fluphenazine decanoate or flupenthixol decanoate gets even a commendation.

Having made all these critical comments, I would add that the book reads easily and is indeed well written. If I had a nephew who was due to sit the MRC Psych examination and had, to my knowledge, done no work at all I would send him this book for his birthday.

ALISTAIR FORREST


It is a commonplace to say that our present society has maximised the role and status of young people. The problems and stresses of adolescence are discussed in detail in these two volumes written by a group of experts at the Tavistock Clinic in London. They provide a valuable source of specialist information.

The first volume discusses the nature of puberty, family relationships of adolescents, aspects of personality and sexuality, the adolescent’s view of authority, protest, creativity, and adolescent groups. Two important chapters on the phenomena of normal adolescence are contributed by Dr A. Hyatt Williams.

In the second volume attention is devoted to various psychopathological manifestations of adolescence—emotional and social conflicts, educational dropouts, delinquency, acting-out, drug abuse, and depression. Aspects of treatment, including family therapy are ably reviewed by Dr J. Byng-Hall.

These two companion volumes will go far in guiding the psychiatrist, psychologist or social worker engaged in helping disturbed young people.

A. BALFOUR SCLARE


This is an excellent book; a good review of the literature and a most interesting account of some of the authors’ own research.

Experimental evidence suggests to psychiatrists that the parents of schizophrenics are sometimes odd, or eccentric, and at times use communication forms which are obscure and hard to follow. These observations are not confirmed to any significant extent by the research described in this book. The reasons for this are not clear, but problems of sampling, selection, and diagnosis must be considered. Many of the American studies relate to whole families, each member of whom has been under therapy and investigation for years; they must be considered statistically and financially
unusual. Again, the psychiatrist in the clinic seeing the chronic patient with his parents must remember that some of the family tensions and abnormalities may well be the result of the patient’s illness (see Brown, 1967).

The present volume reviews all the research on the supposed ‘schizophrenogenic’ effect of the family experience from 1934 to 1974. There is detailed analysis of the ‘double-blind hypothesis’, the family interaction hypothesis of Lidz and the Yale Group and Alenen’s family studies. Pages 63–169 represent an excellent review of all the work on abnormal thought processes and communication found in the parents of schizophrenics, starting with McConaghy (1959) on object sorting, and ending up with an account of the authors’ own attempt to replicate the Wynne-Singer findings (pp. 113–169).

In essence the Wynne-Singer approach has been to use the Rorschach test as a structured way of obtaining language samples from parents individually or in pairs. They have published a manual identifying 41 categories of communication defects and deviations for scoring transcripts of sessions of Rorschach testing. In brief they found they could distinguish, in a statistically significant way, the parents of schizophrenics from the parents of neurotics by studying these transcripts.

Hirsch and Leff drew their sample from hospitals serving a known catchment area in London and took consecutive patients. Their control sample was from the same hospitals but diagnosed as neurotic/depressive illness. The deviance scores for the parents of schizophrenics and neurotics did not differ significantly for pairs (father and mother), or for mothers. Fathers were distinguishable but only at the 0.02 level of confidence. They discuss in detail why their results differed from the group at NIMH and conclude that differences in diagnostic practice between the US and London was probably the important factor. What they did note positively was that their schizophrenic fathers’ high deviance score was associated with a high word count—that is, the schizophrenic fathers were more verbose than the neurotics.

A. D. FORREST

Letters to Editor

NEW METHODS OF ESTIMATING THE NUMBER OF MOTOR UNITS: THE PROBLEMS REMAIN UNSOLVED

SIR,—The two ‘New methods of estimating the number of motor units’ and ‘a potentially third method’ reported by Milner-Brown and Brown (1976) may be ‘important modifications of the original method’ (McComas et al., 1971) but the principle of the technique remains the same.

We wish to repeat briefly two main problems of the motor unit counting techniques, only the solution of which would justify the title ‘new methods’.

Problem 1 The principle of the technique, which is the same for the original and modified methods, we have considered faulty (detailed discussion based on previous reports and new findings can be found in Panayiotopoulos, 1976).

The principle is that the number of motor units (MU) is estimated by dividing the maximum compound potential (MCP) evoked in muscle by the mean motor unit potential (MMUP) of a few MU. The most essential requirement is that the MUP may be summated in a precisely additive manner. We have, however, postulated that the summation ‘would be affected even by small latency differences resulting from differences in diameter and length of activated nerve fibres’ (Scarpalezos and Panayiotopoulos, 1973) and have shown that the MUP do not summate in a peak-to-peak manner (Panayiotopoulos et al., 1974). Furthermore, ‘the amplitude or area of the first five individual MUP (resulting from graphical subtraction) was 5–20% less than the amplitude or area of the fifth incremental response’ (Panayiotopoulos, 1976). Ballantyne and Hansen (1975) reported that the mean value of MUP amplitudes, in 21 control subjects, was much greater when estimation was made from individual MUP (obtained by computer subtraction) than from increments to muscle action potential (61.2 ± 13.6 μV and 42.1 ± 12.2 μV). The above differences were much greater in patients with muscular dystrophies (Ballantyne and Hansen, 1975). The greater amplitude of isolated MUP than of incremental responses reported by Milner-Brown and Brown (1976) may be at least related to all the above findings.

Therefore, the ratio which gives the number of motor units ‘might result in an over- or underestimation of a number of motor axons depending on whether the percentage loss of amplitude or area of the first 10 MUP is greater or less than that of the whole population of MUP contributing to the maximal evoked response’ (Panayiotopoulos, 1976). In view of this argument, all findings obtained by the original (McComas et al., 1971) and modified methods (Panayiotopoulos et al., 1974; Ballantyne and Hansen, 1974; Milner-Brown, and Brown, 1976) are more or less vulnerable and cannot be of absolute value. The absolute value of our results concerning the numbers of MU have always been given with great reservations. ‘The similarity between our electrophysiological and the anatomical estimations of McComas et al. (1971) does not weaken our arguments that the motor unit counting technique does not provide accurate estimations. In some subjects an overestimation and in others an underestimation of motor axons might have occurred’ (Panayiotopoulos, 1976) or ‘it