the human mind, including language and theories as well as works of art, science, and technology. The crux of the matter lies in the interaction between these worlds, and I do not see that the argument is altered by making the unit an extended system of modules rather than "centres" or single cells. The analysis of fully conscious intelligence as an interaction between worlds 2 and 3, with the use of artefacts and speech to amplify memory and permit model building at mental as well as physical levels, is illuminating and the reconstruction theory of learning is thought-provoking. The relationship between evolution of speech and self-awareness is important. The body image concept and the results of callosal section are discussed in an interesting way; are the parietal lobes more important than the frontal for self-consciousness? Mountcastle seems to be moving in that direction.

A goal-directed brain which learns from experience by action and selection is certainly a better bet for further evolution than one depending on conditioned reflexes and associations (*pace* the behaviourists), and it can create its own evolutionary pressures. Both authors agree that the organisms only learn from experience if they are active. They are particularly impressed by Held and Hein's cats. It is also agreed that organisms actively try to impose guessed regularities (and, with them, similarities) upon the external world as it is seen by the sense organs and the self-conscious mind scanning whatever cerebral modules happen to be "open" (Eccles calls it the liaison brain), coupled with memory stores and affective drive from the limbic system. The argument leads them to conclude that the "self" can actively modify the activity of "its" brain. For this reason they reject parallelism in favour of a ghost in the machine. It is not the Ego, and the Soul is an embarrassing term: the emperor is wearing new clothes! So Eccles follows Sherrington and Granit into the dualist camp but cannot suggest a mechanism for a wind without a functioning brain unless it has a supernatural origin. Back to the Golden Bough. As to the question of *what* is the self-conscious mind, Popper does not think that the question is important. You must make up your own mind (actively of course).

J. A. SIMPSON

Historical Explorations in Medicine and Psychiatry Edited by Hertha Riese. (Pp. 232; $22.95.) Springer-Verlag: New York. 1978.

This volume of historiographic studies is published as a tribute to the work of Dr Walther Riese, the distinguished philosopher and neuropsychiatrist who died in 1975. The contents reflect the wide-ranging interests of this leading thinker and, although somewhat diffuse in their materials, are unified by a sustained thread of concern with the evolution of ideas. Dr Hertha Riese, his widow, and herself an accomplished psychiatrist, has assembled an international team of 20 authors to provide a group of loosely related essays.

In the opening section of histiography, methodology, and classification, Dr Jeanne Brand (USA) usefully describes the modus operandi of the research orientated historian and claims that the history of psychiatry is best understood against a background of social history. Professor P. W. Harkins (USA) contributes a chapter in a section concerning the historical roots of modern medicine on the scope and limitations of Hippocrates' views in regard to the present day practice of medicine. Dr I. Galdston (USA) draws parallels, which are not always convincing, between some remarkable problem-solving dreams of patients in psychotherapy and the supplicatory purification rituals of the ancient Greeks. In a section frankly devoted to the past, Dr M. Schachter (France) presents a biographical study of the personality and obscure neurological malady of Heinrich Heine. The last section is devoted to a historical-philosophical review of mind-brain relationships. Here Dr W. Gooddy (UK) ponders the question of cerebral adaptation to the fast-moving four-dimensional world in whose terms man is progressively thinking. In the same context, Professor H. Baruk (France) applauds the emergence of psychodynamic insights during the past 75 years but sourly criticises Freud for his indifference both to neurological and spiritual dimensions.

Readers with a flair for wisdom rather than knowledge will find much to interest them in this unusual publication. It should appeal to many neurologists, psychiatrists, and psychologists with a penchant for the philosophical side to their professional life.

A. B. SCLARE

Therapy Options in Psychiatry Edited by J. Connolly. (Pp. 375; £6.95.) Pitman Medical: Tunbridge Wells. 1978.

If the reader is prepared to look elsewhere for advice on drug therapy this is, by and large, a very readable and enlightening book on most aspects of psychiatric treatment. Minor reservations concern the journalistic use of single word subheadings which bear no relation to the text in the chapter on addictions, and the rather scant attention given to treatment of adolescents despite there being four chapters with psychotherapeutic orientation. The disarray of headings, subheadings, and type size in the section on drugs accurately mirrors the content and presentation therein.

J. A. G. WATT

Electroconvulsive Therapy Task Force Report 14. (Pp. 200; illustrated; $7.50.) American Psychiatric Association: Washington DC. 1978.

This 200 page report deals with all aspects of ECT as did the shorter memorandum by the Royal College of Psychiatrists in September 1977, and both have been prompted in part by the increasing and critical interest taken by the public in this treatment. ECT is usually effective in the treatment of "major" or "severe" (psychotic) depression, this being the main indication for use. Its value in schizophrenia and hypomania is less certain. The development of unilateral ECT, delivered to the nondominant hemisphere, has greatly reduced the memory dysfunction associated with the procedure but there is still room for doubt as to whether this mode is quite as effective as bilateral treatment. The active anti-ECT lobby in USA resulted in the passing of a Bill in California in 1976 controlling use of the treatment because of the alleged division of opinion about its efficacy. However, as the Task Force rightly states, there is no division of *informed* opinion. Nevertheless, replies to a questionnaire indicated that some practitioners have ceased to employ ECT "because of legal issues" and because "insurance rates are so excessive."

Although the matter of relative contraindications is dealt with rather nonspecifically and is difficult to locate because of the absence of an index, the report is excellent in all other respects

and should be required reading for all psychiatrists.

J. A. G. WATT

Developmental Dysphasia Edited by Maria A. Wyke. (Pp. 179; £7.80.) Academic Press: London. 1978.

The child who is slow to talk makes his parents anxious and induces a sense of frustration in his doctors and teachers. Reassurance and encouragement are fortunately sufficient in most cases. But in those instances where there is a true dysphasia, every stage of the child's management from diagnosis to treatment is difficult. For the doctor it is often made more difficult by the scarcely penetrable maze of jargon used by some aphasiologists. Zangwill's introduction to this study is a model of comprehensibility, and the other authors are generally successful in conveying their meaning without recourse to idiosyncratic terminology.

The closely allied condition, developmental dyslexia, has gone through various phases of acceptability to gain neurological respectability. Perhaps this short and essentially practical book will do the same for developmental dysphasia.

IVAN T. DRAPER

An Introduction to the Psychotherapies Edited by Sidney Bloch. (Pp. 224; £8.50 cloth, £3.95 paper.) Oxford University Press: Oxford. 1979.

One of the aims of this textbook is to make the concept of psychotherapy understandable to beginners and in attempting this task the editor has incorporated contributions from seven skilled practitioners of psychotherapy and behaviour therapy.

Despite its title, the book is not essentially about different schools of psychotherapy but rather about various models of intervention in different settings, using a psychodynamic or behavioural approach or a combination of both, and, since practice cannot be divorced from theory, each contributor also offers an outline of his theoretical model.

The quality of contributions is generally high, especially those chapters dealing with behaviour therapy, individual long-term psychotherapy, family therapy, and sex therapy. Although brief or focal psychotherapy is discussed, this topic perhaps deserved a chapter in its own right. Each contributor offers an excellent list of references.

This textbook will certainly be of use to beginners and those colleagues who are interested in knowing something about psychotherapy.

J. D. TEMPLETON

Human Neuropsychology By Henry Hécaen and Martin L. Albert. (Pp. 509; illustrated; £16.20.) John Wiley and Sons: Chichester. 1978.

This relatively small book has 10 chapters on the major topics in neuropsychology relevant to the neurologist's practice. Each one is broken up into short, easily accessible sections so that a brief reference allows one to grasp the essentials of that subject with an outline of anatomy, clinical considerations, and animal experiments, and a generous list of references. In the preface the authors acknowledge a lack of comprehensiveness—for instance, there is no section on dementia—saying that its scope reflects their particular interests. This alone is a monument to their industry. Unlike many books on neuropsychology, this one is easily read and there is a commendable absence of specialised language.

IVAN T. DRAPER

Letters

F discharge method in measurement of proximal conduction times

SIR,—In a recent report on the characteristics of the F response Yates and Brown (1979) concluded that "caution should be exercised before the F discharge is accepted as a method for measuring proximal conduction times in human motor nerves."

This statement, based on only one partially examined observation of the above authors and a theoretical argument, is unjustified. Furthermore, the literature review in their discussion is limited to those reports which favour their conclusion.

Observation "Trains of 100 to 200 stimuli result in F discharges from less than one half of motor units", thus they might not excite the motor units antidromically with the shortest conduction time as the method requires. This observation is of value only when the F wave motor nerve conduction velocity (MNCV) estimations are based on "less than 10 stimuli" as in some of the reports mentioned by the above authors. Reports on F wave MNCV measurement based on more than 20 to 100 F waves, which is our practice, are not cited (Panayiotopoulos *et al.*, 1977; Panayiotopoulos and Scarpalezos, 1977).

It has been shown that one to three out of 20 F waves have the shortest latency. Therefore, accurate F wave MNCV measurements are achievable provided that adequate numbers of F waves are used (Panayiotopoulos *et al.*, 1977, see also references in Panayiotopoulos, 1978). Moreover, the argument of the authors that "if the stimulus trains are too long, the test may be too uncomfortable and require too much time to be practical" does not prove to be justified upon closer examination. One hundred stimuli, for example, given at one stimulus per two seconds, require 3.3 minutes and are much less uncomfortable than the universally applied multistimulus test in myasthenia. It has also been shown that the MNCV in the distal segment of the nerves is practically the same in both the M and F wave methods (Panayiotopoulos *et al.*, 1977; Kimura, 1978; Panayiotopoulos, 1979).

Theoretical argument "All motor units should have a more or less uniformly short central delay in health and disease." This has recently been dealt with (Kimura, 1978; Panayiotopoulos, 1978) in view of the same argument raised by Young and Shahani (1978). I should like to add in this letter that the margin of error introduced by theoretically possible differences in the central delay (which is the time required for the antidromic activation of motor neurones) would be negligible particularly in F wave MNCV measurements of the peripheral nerves of the lower limbs. This is better understood in the following example: a 2 ms difference in the central delay, which is nearly impossible (Kimura, 1978) would introduce a 6 m/s difference in MNCV estimations of the tibial nerve in a subject with an L1-knee distance of 700 mm, F wave and M response latency of 38 and 9 ms respectively, from stimulation at the knee. However, errors of this order are quite common in the classical M response method (see Panayiotopoulos, 1978): a difference of 0.5 ms between onset of M response at