research have failed to discover the essential cause, or even the essential factors, of schizophrenia, and Scharfetter, like Manfred Bleuler, clearly is of the opinion that this state of affairs will continue. Scharfetter concedes readily that for epidemiology and scientific communication a criterion-dependent delimitation of schizophrenia is essential. However, for a deeper understanding, for management, and for therapy we should think only in terms of schizophrenic people (the book's title).

In the light of these theories, which a review can present only very sketchily, schizophrenic symptomatology as well as the literature on aetiology, on features affecting course, and on possibilities of treatment are exhaustively dealt with. The varying views, and the ways in which they were arrived at, are concisely and objectively presented, and usually there is also an evaluation by the author, often critical but only rarely (and deservedly) abrasive. Treatment, according to Scharfetter, should be aimed at restoring ego integration, though usually only amelioration of the disruption is possible. Some 20% of schizophrenics make complete and lasting recoveries from their first attack, and the mechanisms of self-cure should be researched and therapeutically applied. Neuroleptic drugs are almost always required to suppress hallucinations and delusions and to make therapeutic communication with the patient possible, but in the great majority of patients therapies directed at individual aspects of the basic ego disruptions are essential. These include psychotherapeutic approaches of various kinds, behavioural therapies, family therapies, and education of the carers. Most of these tailor-made measures do not require full medical training, and should be in the hands of appropriate members of the team.

These days one has become rightly distrustful of system building and of hypothetical constructs, which are difficult to test, but this book does seem to crystallize much of what many of us have been thinking at some time or other. For this reason, and on account of the critique of the literature on schizophrenia research, translation should be seriously considered.

FELIX POST

Electrical Activity of the Archicortex. Edited by G. Buzsáki and C. H. Vanderwolf. (Pp. 404; £34.25). Akadémiai Kiadó: Budapest. 1985.

This book records the proceedings of a symposium, held in Pécs, Hungary, in September 1984, that dealt intensively with the electrical activity and behavioural functions of just one region of the brain, namely, the hippocampal formation. In spite of the time that has elapsed between the symposium and the appearance of this notice, the material remains very much up-to-date, testimony both to the excellence of the symposiasts (nearly all of whom had fresh and important laboratory findings to report) and to the longevity of the problems with which the field is faced. Most of the issues discussed have been around for at least a decade, and will probably still be around a decade hence, even though many elegant experiments, some reported here, have provided a mountain of rich and valuable data to digest.

What has all of this to do with readers of *Psychological Medicine?* The answer to this question will depend upon our eventual understanding of the functions of this region of the brain. But, if *any* of the leading hypotheses turns up trumps – those given most attention in this volume relate to spatial analysis and various forms of memory, – then the hippocampus will have much relevance to psychology and psychiatry. For the moment, however, it will require an unusual and devoted interest in this still mysterious organ to take one from cover to cover of the book. But, if readers happen to possess such an interest, they will find this volume an excellent way to satisfy it.

J. A. GRAY

Left, Right, Hand and Brain: the Right Shift Theory. By M. Annett. (Pp. 474; illustrated; £29.95.) Lawrence Erlbaum: New Jersey. 1985.

Since 1964, Dr Marian Annett has published some thirty papers on lateralization, the vast majority concerned with the inheritance of handedness and with its relationship to cerebral dominance. On several occasions the trail has been a false one. The first genetic model, published in *Nature* in 1964, has been long refuted, not least by Annett herself. A second speculation, advanced in 1967, that the categories of 'right, mixed and left handedness' are in binomial proportions can now be seen to be nothing but numerological mystification, without substantial empirical power, depending for its results on the arbitrary division of a continuous distribution. A third result, that handedness as a skill is approximately normally distributed, or is at least unimodal, has lasted longer, but now seems to be primarily dependent upon the particular choice of task.

Dr Annett's most substantive contribution to our understanding of handedness has been a genetic model with a number of original features, the 'right shift theory', and which is the principal subject of the present book. The model has already been widely published, and, as some readers will be aware, I have criticized it in detail elsewhere (McManus, 1985*a*), and have proposed an alternative genetic model in a monograph published by this journal (McManus, 1985*b*). Simple rehearsal of those criticisms in this review would be of little benefit, and I propose instead to try to stand back from the details of the right shift theory, and to ask instead what sort of questions in general are being asked about lateralization and its genetic basis, to ask why they are of general interest within psychology, and to ask how they might be construed by a philosopher of science (and, in particular, I will use the ideas of the late Imré Lakatos (1978), and his concept of a 'scientific research programme').

Laterality in general has shown a massive resurgence of interest over the past three decades, perhaps dating back to Kimura's paper of 1961 in which she discovered asymmetries in the dichotic listening tasks first described by Broadbent. Indeed, at present over 400 papers on laterality are published each year, and the number is increasing by nearly 9% per annum (McManus, 1986). On merely statistical grounds such a large-scale research effort would seem to deserve the name 'programme'. That would not be justified, however, for instead we may merely be dealing with the vagaries of what Lakatos calls 'mob psychology'; fashion takes many forms, and it might be that research into laterality is one of them. What makes the simple collection of facts into a scientific research programme is the existence of some central hard core of theory; a theory which is not a lone hypothesis, but is part of a broader effort such that it is surrounded by additional hypotheses which form a 'protective belt' around the core and prevent unduly premature rejection by empirical facts which could, to a naïve falsificationist, be the death of the theory. Developing theories are tender plants which cannot be exposed to the full blast of an east wind without being protected through their first hard winters. Research programmes therefore have a sense of direction, often containing what Popper has called a 'metaphysical research programme' at their base, the *real* reasons for the whole enterprise.

Much – I might even be tempted to say most – of laterality research is not clearly part of any genuine programme. Workers often appear to published a paper or two and then to disappear from the field; experiments are rarely repeated and, when they are, often fail to replicate the original findings, resulting in a little theoretical '*ad hoccery*' to 'explain' the inconsistency; and, in many cases, the only justification for publication seems to be that some main effect or interaction has reached the magic '$P < 0{\cdot}05$' in a statistical analysis that has taken no note of Type I errors. To call this a research programme is falsely to glorify some uninspiring episodes in the history of psychology.

Nonetheless, laterality research *is* going somewhere. To my mind at least there *is* a genuine programme, with an implicit metaphysics at base. That metaphysics concerns the feasibility of understanding brains. People, and presumably also brains, differ one from another; and, to a large extent, these differences can be construed as being quantitative along a number of dimensions (as, for instance, in much personality theory). A far more worrying prospect is that there might be *qualitative* individual differences in cognitive processing or in brain structure and function. Such a finding could deal a near lethal blow to psychology as it is presently conducted, with its extensive modelling of individual differences by Anova type statistics which simply fail to give meaningful results in the presence of qualitative differences; the spectre therefore hangs over neuropsychology that many, if not most, of our empirical results might have to be discarded as uninterpretable. That such qualitative differences can and must exist has been a necessary commonplace in neuropsychology since the late nineteenth century, and Broca's demonstration of cerebral dominance for language. I wish to argue here that the metaphysical basis of laterality is that it might allow this Gordian knot to be cut, providing a *via media* between the Scyllan anarchy of qualitative differences, and the Charybdean failure of theoretical models based solely on quantitative differences. Laterality models, particularly those which specify a genetic basis for differences, allow a *finite* and, one hopes, a *relatively small* number of different types of cognitive or neural organization; analytical tractability is therefore restored. The high price of this tractability is the near inability to derive conclusions from the individual case study (so much beloved of the recent decade of neuropsychology), since the principle of double dissociation, the corner stone of most lesion studies, requires the powerful assumption of *ceteris paribus*, and with qualitative differences one cannot assume that all other things *are* indeed equal. A crucial exception would be if accurate genotyping of qualitatively different types were possible.

Within the pragmatic, empirical research programme the essential core contains arguments as to the precise form of the genetic model (and here I differ from Annett, preferring a conventional single gene model to her complexities – and the reader is referred to her recent review (Annett, 1986) for a fair description of our differences). The differences lie also in the additional hypotheses which we each find necessary to account for the manifest failure of any genetic model to be fitted directly to the extant data. To answer questions such as 'How do we measure and define handedness?', 'How do performance and preference relate?', 'Are twins and singletons similar?' etc. the theories make their own specific assumptions and predictions, and it is here, rather than at the core, where most of the battles are being fought at present. Should any of these hypotheses fail, the core theories can still survive. For *those* to be tested we must ultimately await inevitable advances in molecular biology, which surely will one day determine with precision whether or not the DNA of right-handers and left-handers is systematically different. For then one theory will clearly be proven beyond any reasonable doubt to be superior to all others.

Dr Annett's book is a long (probably overly long) summary of her work. It not only describes her model, which it does very fully, but also attempts to give introductory accounts of brain evolution, and of genetics: it is probably fair to say that anyone who requires these introductions should probably not be reading the book, especially given that the accounts are not particularly well done. The review of the right shift theory is comprehensive, and would be very suitable for a novice reader. For anyone who has followed the trail over the past decade there is little that is new, most having been more succinctly described in the many previous research papers. Many obscure aspects of the theory are, however, expanded upon, and therefore clarified.

In summary, Annett's right shift theory has been an important and influential attempt at making sense of the mystery of the inheritance of handedness and cerebral dominance, and that attempt is of broad significance for neuropsychology as a whole. This book will provide a useful source of reference for any one coming to the field afresh who wishes to understand Dr Annett's theories.

I. C. MCMANUS

REFERENCES

Annett, M. (1986). Review of I. C. McManus' 'Handedness, language dominance and aphasia'. *Psychological Medicine* **16**, 227–228.

Lakatos, I. (1978). *The Methodology of Scientific Research Programmes* (ed. J. Worrall and C. Currie). Cambridge University Press: Cambridge.

McManus, I. C. (1985*a*). Right- and left-hand skill: failure of the right-shift model. *British Journal of Psychology* **76**, 1–16.

McManus, I. C. (1985*b*). *Handedness, Language Dominance and Aphasia: a Genetic Model. Psychological Medicine*, Monograph Supplement No. 8. Cambridge University Press: Cambridge.

McManus, I. C. (1986). Review of A. Beaton's 'Left side, Right side'. *British Journal of Psychology* 77, 419–421.

Great and Desperate Cures: The Rise and Decline of Psychosurgery and Other Radical Treatments for Mental Illness. By E. S. Valenstein. (Pp. 340; illustrated; £14.95.) Harper & Row: London. 1986.

Eliot Valentstein's two earlier volumes on psychosurgery will have prepared many readers for his authoritative historical account of the rise and fall of the most radical method of treatment of mental disorder to be employed in the