Dr. Snaith asks what is the value of our diagnosis, once made. Dr. Snaith has missed the point; that, perhaps, is our fault. In each of our reports we have attempted to stress the value of the diagnostic criteria in prediction of clinical outcome. We consider our findings useful, first of all, to physicians who are confronted with patients who report symptoms which are unexplainable. These patients are frequently labelled "hysteric", "psychogenic", or "functional". Physicians need a systematic way of approaching such patients in order to organize management. Our studies represent an attempt to provide such a systematic approach. We need not recapitulate the result of the various studies here, except to say that the more closely a patient meets the objective criteria for hysteria the more his physician may be confident that the symptoms are medically benign. On the other hand, unexplained symptoms among patients who do *not* meet the objective criteria for hysteria are associated with a medical outcome involving the most diverse medical, neurological, and psychiatric illnesses (1, 3, 4).

The question of "mild hysteria" needs clarification. We have not been particularly vocal about mild hysteria because our understanding of it is incomplete. If mild hysteria exists, it will be demonstrated by data provided by the follow-up of patients who have symptoms fewer than we require for our present diagnosis; also by the study of the family members of those patients. We are engaged in such studies at this point.

We have commented previously on the importance of family data (2). We refer the reader to those comments, rather than recapitulate them in the same journal after this brief period of time.

It is characteristic that hysteria begins early in life. The age of 35 as a formal requirement is somewhat arbitrary. The ages of 30 or 25 might have been chosen just as well. In fact, we suspect that the choice of a younger age of onset might improve our criteria; probably even fewer false positives would occur. Preliminary data also suggest that a requirement for younger age of onset may allow us to dispense with the awkward business of deciding when individual symptoms are or are not medically explainable. Out of context, the diagnostic criteria appear arbitrary; they were actually derived from studies of the natural history of hysteria (5). It was on the basis of those studies that our more quantitative approach to diagnosis was organized.

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UNILATERAL ELECTROCONVULSIVE THERAPY

DEAR SIR,

I should like to comment on the double-blind trial of unilateral E.C.T. by Raymond Levy (*Journal*, April, 1968, p. 459).

Clinical psychiatrists may agree that the duration and intensity of memory impairment after each E.C.T. varies a great deal from treatment to treatment, and although temporary may last more than six hours. It becomes a serious matter, however, if it lasts more than 24 hours. Perhaps there were practical reasons for retesting only six hours after the last E.C.T., but it would have been more interesting to have waited for 48 hours. The author finds significantly less impairment on "general orientation" and "memory for general events" of the Gresham Test Battery with unilateral treatment, and more relief from depression with bilateral treatment (but not significantly more).

By a curious coincidence, three days after reading this paper, one of my patients (male, aged 45, suffering from endogenous depression with obessional features) volunteered the information that his present E.C.T. (three treatments in one week) had caused much less memory disturbance than his treatment two and a half months ago (two treatments) which was discontinued because of severe memory impairment. He had come back for completion of the course of E.C.T. as he felt that in spite of the memory disturbances the previous two treatments had done him more good than antidepressant drugs.

He did not know it, but the previous E.C.T. was given bilaterally and the present treatment unilaterally. Subjective relief of depression is now almost complete.

I for one shall continue to use unilateral E.C.T. as I have done in the past, not as a routine procedure,

but for patients such as this (a subordinate judge) in whom memory disturbance can be incapacitating. It is just as safe and almost as simple.

I fully agree, however, that further inquiry is needed, and we are working on this.

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GLUCOSE TOLERANCE IN DEPRESSION

DEAR SIR,

What Dr. van Praag writes in his letter about "averages" in clinical investigations (*Journal*, September, 1968, p. 1195) is, I believe, of fundamental importance to all those actively engaged in psychiatric research.

It has long been my conviction that the study of sub-groups, as these are at times discerned in the collection of research information, merits the greatest possible care and detailed analysis. Yet the very existence of such sub-groups is often missed, either because of inappropriate statistics which "iron out" the meaningful clusters of data or (which has the same practical effect) because of the attitude of the research worker who analyses them.

I had the opportunity to participate in the discussion of this problem in a symposium during the last International Congress of Psychiatry in Madrid, and was delighted to see that at least the mathematicians are fully aware of the difficulties in medical research; they are making use of electronic devices to help in the extensive analysis of amassed research data, with the very aim of recognizing significant clusters. Welcome as this new development might be, it cannot, however, replace the inquiring eye of the clinician who has an intimate knowledge of his material. Dr. van Praag's letter is a typical proof of this.

My own experience is in line with Dr. van Praag's findings about a category of patients whose psychiatric disorder is probably correlated with disturbance of metabolism. Some of this research work done at the Bethlem Royal Hospital under W. Linford Rees was with puerperal depression, where the tendency in certain sub-groups to emerge was obvious in a number of metabolic parameters studied—including glucose (Jacobides, 1957); although the deviations from the norm were never deemed great, especially as they were gauged by the single methods of Glucose Tolerance Test and Insulin Sensitivity Test and not by the elaborate biochemical assessments of Dr. van Praag. It is hoped that the findings from puerperal psychotics will be published in some detail and that they will give support to the conclusion of Dr. van Praag, viz. that "averages can be deceptive in biological psychiatry". I can only add to this important epigram that averages can be *a menace* when they let meaningful information from a sub-category or even from a single patient be drowned and irrecoverably lost!

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TREATMENT OF PREMATURE EJACULATION

DEAR SIR,

In reply to the letter of Dr. Ahmed (*Journal*, September, 1968, p. 1197), I would like to make the following points: Dr. Ahmed reports that patients receiving a 1 per cent. solution of methohexitone sodium feel intense pain along the site of the vein. Having regularly used a 2·5 per cent. solution of methohexitone sodium, as recommended by Friedman (1966), I have never received any complaint of pain. It has been my experience that patients may complain of unpleasant giddiness during the induction period, but this can be avoided by the anaesthetist injecting the drug very slowly (Kraft, 1967).

The first patient's symptoms of enuresis, frequency of micturition and premature ejaculation, which Dr. Ahmed attributes entirely to anxiety, might also be interpreted in terms of passive male urethral eroticism (Fenichel, 1946). During his treatment, as the duration of erection increased, there was a parallel decrease in his frequency of micturition, which is of theoretical importance, as it provides a link between analytic and behaviour-orientated approaches to treatment in this field (Kraft, 1969).

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