used in endocrine, cardiovascular and many other disorders, where drug treatment aims to restore and preserve the patient's functional adequacy.

Lithium does not abolish the clinical and psychosocial evidences of recurrent affective disorders. It diminishes the severity of symptoms to varying degrees and reduces the amplitude of mood swings to a point where hospitalization can be avoided. Actually, most of the reported statistics concerned with prophylactic effects concern reduction of frequency or length of hospitalization. While hospitalization may be taken as a global measurement of the severity of affective episodes, clinical evaluation must also take into account the less severe mood fluctuations which continue during lithium therapy.

It is well known that lithium is frequently combined with antidepressant drugs during depressive cycles to maintain patients' ambulatory status. Similarly, neuroleptic drugs may have to be added to control rapidly emerging manic disturbances to maintain the patients' functional balance. Neither clinical nor psychosocial patterns indicate that lithium prevents affective disorders to the point where evidence of illness disappears.

Whatever the pharmacological action of lithium may turn out to be, it appears to interact effectively with an ongoing biochemical disorder, counteracting its socially and clinically disruptive manifestations. I believe that this is best conceived of as compensatory therapy rather than as prophylaxis.

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CRIMINALITY AND VIOLENCE IN EPILEPTIC PRISONERS

Dear Sir,

Once again, by use of a biased sample, a paper has been published purporting to show that, 'it is clearly incorrect to think of epileptic prisoners as being especially violent' (*Journal*, March 1971, p. 337). Although reference is made to the possibility of the sample being unrepresentative in not including patients in Special Hospitals, broad conclusions have been drawn which, while strictly true for the sample chosen, do nothing to elucidate the problems of epilepsy and violence.

The definition of the Hospital Order (M.H.A., Section 60) includes the terms 'A patient convicted . . . of an offence punishable on summary conviction with imprisonment . . . etc.' Thus the Mental and Subnormality Hospitals, as well as the Special Hospitals, must contain many individuals who, but for the Hospital Order, would be in prisons.

In Rampton in 1968 there were 138 known male epileptics, representing 20% of the males in that institution. Of these, 11 committed property offences chiefly and 127 were violently aggressive and assaultive, leading to deaths on four occasions. It was apparent in reading the records that in many cases deaths had only narrowly been averted.

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CLASSIFICATION OF DEPRESSED PATIENTS: A CLUSTER-ANALYSIS-DERIVED GROUPING

Dear Sir,

In his paper (*Journal*, March 1971, page 275) describing cluster-analysis-derived groups of depressed patients, Dr. Paykel rightly observes that few studies using factor analysis have seriously explored the possibility of more than two groups. He also comments that previous factor-analytic studies suggested a simple division of depressives into two polar types rather than more complex classifications.

Since the implications for methodology could be considerable, I wish to point out that at least one previous factor-analytic study went further than a simple dichotomy and proposed a multiple-group classification. In a factor-analytic study of 126 depressed patients seen in general practice (1), two clear-cut groups of patients, one endogenous and one non-endogenous, were found when the patients were distributed on one factor; on another, virtually independent, factor there were *three* patient groups: phobic-anxious, (non-phobic) anxious, and non-anxious. Two other factors, identifying reactive depression and general severity respectively, did not serve to distinguish patient groups. These results acknowledge a diversity of neurotic sub-groups independent of a primary division of patients into endogenous (or psychotic) and non-endogenous.

This multiple-group classification was obtained by factor-analytic methods (including calculation of factor scores) only when the number of variables

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was reduced to five. It seems more than coincidence that the variables in Dr. Paykel's cluster analysis 'scores on principal components' numbered only six, whilst his principal components analysis, using 35 variables, showed no patient groups whatsoever. This may be because his variables were too numerous (or of insufficient relevance) for delineation of patient groups by the principal components method of factor analysis. The groups he demonstrated by cluster analysis, as he himself implies, derive to some extent from the statistical method itself, and his results do not necessarily indicate that his groups are in fact clear-cut. The factor-analytic approach, on the other hand, has the advantage that groups will only be shown by this method when they are genuinely distinct.

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A CASE OF THE KLEINE-LEVIN SYNDROME IN INDIA

Dear Sir,

In the November 1970 issue of the *Journal*, Drs. Prabhakaran, Murthy and Mallya have reported a case of Kleine-Levin Syndrome (p. 517), claiming it to be the first to be reported from India. I wish to point out that a case of Periodic Hypersomnia (1) conforming to descriptions of the Kleine-Levin Syndrome has been reported earlier from this country.

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India.

Reference

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AN EMPIRICAL STUDY OF RELIGIOUS MYSTICISM

Dear Sir,

I should like to make two brief comments on the interesting article by B. Douglas-Smith (*Journal*, May 1971, p. 549). I wonder if the author considered the possibility that members of the lower social classes have such experiences as he recounts but are unable to describe them because of a lack of facility with ideas and language. As for his comment when comparing Religious Mysticism and E.S.P. ('always on the assumption that E.S.P. exists at all') it seems to me that the existence of the former is more open to doubt on the grounds that no objective investigation is possible, whereas E.S.P. can be the subject of scientific inquiry.

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REMAKING AN ORGANIZATION

Dear Sir,

Dr. Schulman (*Journal*, April 1971, p. 487) believes I have misled your readers about his book. I do not agree with him and would like to discuss the most important point he raises.

I selected two examples of what I described as a 'parody of scientific method and argument'. The first concerned a questionnaire which provided the basis for conclusions on the question of 'value cleavages'. I pointed out that the answers to fourteen unidentified questions were discarded: an unspecified proportion of questionnaires were not returned; but the conclusions are applied to all. Some to whom the questionnaire was addressed gave such absurd answers that it was clear that the questions were misunderstood or the answers were lies; but there was no check on the truth of the anwers on which Dr. Schulman relied, although this is a problem with which sociologists are familiar. In the second example a firm conclusion was based upon a discrepancy between 43 per cent and 17 per cent of subjects, where 17 per cent represented one subject.

My objection here is not, as Dr. Schulman implies, that the method is sociological or ethological, but that the canons of scientific procedure, commonly acknowledged among social scientists, are flouted: in the instances I have given, among others, the evidence offered does not justify the conclusions Dr. Schulman has drawn; in publishing them he does a disservice to the branch of science he represents, and to psychiatry, which depends on it.

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TRAINING GROUPS

Dear Sir,

Following the growth of interest in training groups (T groups) in the U.S.A., the British population is

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