

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.

*St. Loman's Hospital,
Palmerstown,
Co. Dublin.*

T. J. FAHY.

REFERENCE

1. FAHY, T. J., BRANDON, S., and GARSIDE, R. F. (1969). 'Clinical syndromes in a sample of depressed patients: a general practice material.' *Proc. Roy. Soc. Med.*, **62**, 331.

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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Lucknow,
India.*

V. R. THACORE.

REFERENCE

1. THACORE, V. R., AHMED, M., and OSWALD, I. (1969). 'The EEG in a case of periodic hypersomnia.' *Electroenceph. clin. Neurophysiol.*, **27**, 605-606.

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 answers 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