

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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T. J. FAHY.

REFERENCE

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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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V. R. THACORE.

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 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

now becoming rapidly aware of their existence. This letter is not intended as a condemnation of such activities, but is written out of a developing uneasiness concerning the methods employed and assumptions made by those involved in the organization of these groups.

In all established disciplines which deal with people there are sets of normative standards, rules and codes of behaviour, and the professional usually has constrictions placed upon his conduct in the form of a peer group of similar professionals. At the moment 'T' groups are being widely organized, and people are being 'trained' by individuals who appear to have complete autonomy and lack any established code of conduct and remain immune from censure. This point would be superfluous and trivial if the groups under discussion were being 'trained' for a specific task with little self or ego involvement. However, considering that T groups encourage 'encounters', 'confrontations', 'honesty', etc., and the direct expression of both positive and negative emotions, then it seems that questioning of some of the basic assumptions of 'T' group leaders is not only desirable but essential.

In one of the very few comprehensive examinations of psychiatric perspectives on 'T' groups and the laboratory movement, Gottschalk and Pattison (1) point out that leaders may use groups for their own aggrandizement or neurotic needs. They add 'Leaders may be incompetent—either accomplishing little or allowing unnecessary and destructive group activity'. Berne (2), in his account of principles of group treatment with psychiatric patients, emphasizes, on the therapist's part, a self-critical, self-evaluative approach, with a specific appraisal of an examination of one's own motives for starting group therapy. No such qualifications are required for 'T' group work, and there appears to be a lack not only of a clearly defined goal but also of clearly defined responsibility. Crawshaw (3) describes three cases of emotional disturbance aggravated by sensitivity training. He did not detect quackery, but noted 'an absence of ethical standards and an apparent abrogation of responsibility by the trainers and their sponsoring institutions'. He reports the case of a teacher, in treatment with him, who was *ordered* by his supervisor to attend a sensitivity training experience. Crawshaw writes that 'as a result of the psychological evisceration, the patient was admitted to the State hospital, and as a result of the hospitalization lost his position'. He goes so far as to put the 'T' group to a modern test—the Nuremberg Rules, evolved after investigation of the Nazi medical experiments.

Perhaps this is extreme, but numerous relevant assumptions are questioned by Crawshaw. It seems

to be held that negative psychiatric history is sufficient as adequate participant selection criteria. This approach seems to demonstrate a naïvety about human psychodynamics which would be touching if the implications were not so serious. Even many behaviourists would concur, albeit in different terminology, with the view that individuals need and use ego defences. 'T' groups often seem to ignore this and place high value on 'honesty', 'exposure', 'attack' and 'vulnerability', with little attention paid to the need for support. The total theorizing regarding personal inadequacies, exposure, etc., ignores the basic fact that individuals vary markedly in personality adjustment and in their ability to tolerate stress. Psychotherapists would probably concur on the notion of establishing a rather protected, accepting environment for a patient before pointing to some of the patient's even peripheral defence mechanisms, and here we assume that the therapist's aims have as their goal the ultimate benefit of the patient. Considering the dubious motives underlying a 'T' group member's 'frank' appraisal of another's behaviour and personality, in an environment which may lack a responsible assessor of an individual's level of stress tolerance, there appear to be strong reasons for disquiet concerning the present concept of the 'T' group. As Gottschalk *et al.* stressed, 'the assessment of "T" group results has failed to consider seriously the psychotoxic or deleterious effects of group participation: without adequate training supervision and guidelines, a powerful instrument may be destructive, just as a valuable drug may have undesirable effects if used universally incorrect doses.'

Because the 'T' group is not intended for or in individuals with psychiatric abnormality, many of those most fitted to analyse and criticize have remained silent, and the 'T' group looks as though it is very much here to stay, with an increasing effect on the lives of the population. The use of training groups is becoming more widely accepted in this country and is being utilized by management and labour groups in industry, civic leaders, social scientists, educators, hospital authorities and clergy as an effective means of teaching the factors which assist or hinder human relationships. Commercial enterprises also seem to be taking advantage of the present climate of opinion and are making rather exaggerated claims of the therapeutic changes effected by these procedures.

The way in which many 'T' groups are conceived bears some relationship to cults which have developed in the past—assumptions are not tested, and unjustified claims are made, the motives of organizers are questionable, few limits on the scope of the theory are placed, belief is more important than

appraisal, and those likely to suffer personality disruption are not screened. Crawshaw feels that the medical profession has an opportunity, if not a responsibility, to ask institutions employing 'T' groups to re-examine the ethical basis when using humans as subjects. From a more practical angle, not only psychiatrists but all those dealing with personal problems should ask questions, if only because a number of writers have pointed to the probable contribution of sensitivity training to exacerbation of emotional problems in certain individuals, who are usually not followed up after their group experiences. What other body of individuals is in a position to question the place of sensitivity training in our society and assess critically what has been hailed with almost unreserved enthusiasm and unquestioning faith?

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J. R. MARSHALL.

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2. BERNE, E. (1966). *Principles of Group Treatment*. New York: Oxford University Press.
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THE RHYTHM OF 'POP' MUSIC

SIR,

Having, while on holiday, been reluctantly compelled to listen to a considerable amount of 'pop' music, I was struck by one aspect of its rhythm to which I myself have not seen any reference in discussions of the subject, but which seems to me to be of some significance. In books and papers on pop, there are repeated comments on the physically exciting and sexually stimulating effect of the loud fast beat, the 'jungle rhythm', and descriptions of the mind-deadening quality of the sheer volume of sound produced by some groups of players, leading to a semi-hypnotic state of the dancers, who are said to appear to be seeking the equivalent of a 'trip' on drugs by means of this overwhelming sensory bombardment alone.

All this is undoubtedly true, but the aspect to which I would like to draw attention is that of the double rhythm, the slow rocking (reminiscent of the 'Hill Billy' horse-back songs), coupled with the steady fast beat, as best heard in the quieter but continuous playing of some groups, building up to no climax but persisting throughout the entire evening. This is the type to which the devotees

usually dance separately, without a partner, not moving from one spot on the floor, but simply making slow writhing movements of the arms, hands and hips, and gradually attaining to a dreamy-trance-like state. About three-quarters of the dancers were girls, on an evening which I witnessed, and I am told that this is often so.

It occurred to me that this linked double rhythm was precisely that which is heard by the unborn baby in the womb. The slow rocking component is the sound of the mother's heart-beat, transmitted from her aorta, while the continuous rapid beat is that of the child's own heart, transmitted clearly by the intrauterine fluid. If I am right, then these young people are not seeking excitement, but a return to the pre-natal state, just as I suggested in an early paper a rocking and head-banging toddler is doing. (*J. ment. Sci.* (1950), **96**, 763.)

In an article on pop and drugs in the *Sunday Times* of 14th March, the writer said, 'What is involved here is a flight from boredom', and that is undoubtedly true, but in the type of pop dancing I have just described the quest seems to me to go even deeper, to be a flight from a separate, self-responsible existence to a pre-natal Nirvana, a rejection of both 'experience and discovery of self', the penultimate abnegation of life.

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FORTHCOMING CONFERENCES

Third Conference on Behavioural Modification

The Third Conference on Behavioural Modification will be held from 24-27 September 1971, in Wexford, Ireland. Topics to be covered include token economy systems, behaviour therapy with children, methods of assessing behavioural change. For further details write to: The Conference Secretary, Behavioural Engineering Association, c/o Department of Mental Health, The Queen's University of Belfast, Belfast City Hospital, Belfast, BT9 7AB.

Third International Conference on Social Science and Medicine

The Third International Conference on Social Science and Medicine will be held at Elsinore, Denmark, from 14-18 August, 1972. Further details, including the preliminary programme and registration forms, can be obtained from Dr. P. J. M. McEwan, Director, Centre for Social Research, University of Sussex, Falmer, Brighton, BN2 9QN.