analysis (*Journal*, February 1972, pp. 143–5), in which papers by Pilowsky *et al.* (*Journal*, 1969, **115**, 937) and Paykel (*Journal*, 1971, **118**, 275) are referred to, it was certainly not my intention to accuse any of these authors of naivety.

However, in both papers only one method of cluster analysis was used, and although the groupings found may represent a stable solution there is also the distinct possibility that other clustering techniques might lead to considerably different solutions. The main difficulty is that each clustering technique is based on a certain set of assumptions, usually different for each method and mostly not clearly stated, and if the data fail to meet these assumptions spurious grouping will almost certainly be obtained. For example, the clustering criterion used by Dr. Paykel, namely minimization of /W/, assumes that all the clusters present have the same shape, an assumption which may or may not be reasonable. Dr. Paykel's reply to my paper (letter in this *Journal*, June 1972, pp. 695–6), points out that cluster analysis techniques have considerable advantages over factor analysis when one is seeking diagnostic categories. With this I agree, although ordination methods such as principal components may allow the data to be visually examined and clusters found, since when the data have not been forced into clusters the observer can assess better whether clusters exist.

The point of my paper was to try to make potential users of these techniques more cautious. A paper by Strauss *et al.* to appear in a forthcoming issue of this *Journal* shows clearly why they should be so, by describing the results of applying several different clustering techniques to a set of artificially constructed data. Different methods obtained widely different solutions although the data were constructed to be reasonably well structured.

B. S. Everitt.

Biometrics Unit,
Institute of Psychiatry,
De Crespigny Park,
London, SE5 8AF.

'RESULTS IN A THERAPEUTIC COMMUNITY'

Dear Sir,

We noted Dr. David Abrahamson's letter (*Journal*, April 1972, **120**, pp. 473–4), in which he criticizes the ward chosen as a control for our therapeutic community for disturbed patients. It seems that he has misunderstood us or that we expressed ourselves badly.

First, we were at pains to distinguish between therapeutic community approach and therapeutic community proper so that there should be no doubt about the organization we were examining. Second, the control ward was chosen particularly because it was conducted humanely and hopefully; we saw a number of wards but deliberately chose this one because it had its doors open, the majority of the patients went off to work every day, and there were none of those feelings of tension, degradation or hostility which many of us know so well from the bad old locked wards. Nevertheless, it provided a good contrast with our therapeutic community ward because it still maintained the medical model's social distinctions.

We are sorry if we did not state these points clearly enough, but we can assure Dr. Abrahamson that the control ward was carefully chosen, and that it represented the best that can be achieved so long as the traditional social structure is unchanged.

K. Myers.

'Southwood' Psychiatric Unit,
Middlewood Hospital,
(P.O. Box 134),
Sheffield, S6 1TP.

Dear Sir,

There are a number of peculiarities in the statistical treatment of the data in the paper by Myers and Clark, which appeared in the January 1972 issue of the *Journal* (**120**, pp. 51–8.)

First, Table III shows a significant Fisher exact probability of 0·029. I do not know how this was calculated, but it is inaccurate. A Fisher *exact* probability is extremely tedious to compute if none of the cells is zero, and it is much easier to use Table I in Siegel, which gives fixed levels of significance for the Fisher test. This shows that P in this case is less than 0·05. This means that there is no significant difference between the two patient groups in spontaneity of interaction.

Secondly, it is not made clear that the P of 0·029 in Table II (in which the bottom right hand cell should read 4 not 1) is in fact one-tailed. Using the more usual two-tailed criterion this P is not significant. It is difficult to understand why a one-tailed criterion was applied here when a two-tailed one is used in Table V. Strangely enough, the size of the χ^2 in Table V indicates that Yates' correction has been needlessly applied.

Thirdly, the inter-judge contingency coefficient of 0·28, despite being significant at the 0·05 level, is *much* too low for the mental assessments to be accepted as reliable, and suggests possible assessor bias.

Contrary to the authors' conclusions, therefore, there is only one area, that of discharge direct into the outside community, in which the therapeutic ward produces significant change. The authors say that this was not due merely to ward policy but that the degree of interaction in the ward community suggested that this was the appropriate move. Since the latter has not been demonstrated, one can only assume that there has been some degree of bias in discharge decisions.

Finally, the authors say that many other statistical calculations were computed but none proved significant, suggesting that they have selected the choicest of their results for publication. Perhaps if these had been reported, a fuller picture of the therapeutic inefficacy of the community ward might have emerged.

J. G. Greene.

Psychology Department,
Gartnavel Royal Hospital,
Glasgow, G12 0XH.

MMPI PERFORMANCE IN CHRONIC MEDICAL ILLNESS

Dear Sir,

Goldstein and Reznikoff in their recent article in the *Journal* (February 1972, **120**, 157–8) report significantly higher mean scores on the neurotic triad of the MMPI for haemodialysis patients as compared to general medical patients convalescing from minor medical conditions. Elsewhere their report states: 'The finding of significant elevations of Scales 1, 2 and 3, the neurotic triad, confirms results of other studies on haemodialysis patients employing the MMPI' (p. 157). Apparently the authors have equated 'significantly higher mean scores' with 'significant elevations' although the latter expression in MMPI parlance has the specific meaning of 'Scale elevations at or above T-score 70', i.e. scores significantly above the MMPI standard population mean (T-score 50). They do not say how they are warranted in making this equation.

The distinction is important, because only when T-scores reach or exceed the T-score 70 level does conservative interpretation indicate the possible presence of psychiatric illness.

Failing to state unequivocally that all or most of the haemodialysis patients obtained scores at or above the T-score 70 level, Goldstein and Reznikoff have left open the possibility that although the haemodialysis patients as a group obtained higher mean T-scores than the controls, none or only some of the individuals in the haemodialysis group obtained triad scores of significant elevations.

That haemodialysis patients would show *some* elevation on the neurotic triad (particularly on Scales 1 and 2) is of course to be expected: such non-critical elevations would accurately reflect the physical and psychological stress effects of their condition, without suggesting at the same time the presence of a neurotic condition. Alternatively, it is possible that the unpublished data of Goldstein and Reznikoff show that *some* of the haemodialysis *and some* of the control patients obtained significant neurotic triad elevations. Subject to the outcome of individual psychiatric evaluation one would have to assume that those individuals, whether haemodialysis or control patients, were in fact true neurotics. Obviously, neither the presence of kidney disease nor that of any other medical condition bestows immunity from neurotic illness.

Only if it were shown that neurotic triad elevations at or above T-score 70 were significantly more common amongst haemodialysis patients than amongst their matched controls would one have to face the possibility of mislabelling.

With reference to the computer statement frequencies presented by Goldstein and Reznikoff in Table I (p. 158), Fisher exact probabilities show that only three of the statements occur more frequently (at or beyond the 5 per cent level) in the computer-derived MMPI interpretations of the haemodialysis groups than in the control group: 'Normal male interest pattern for work, hobbies, etc.' ($p = \cdot 0345$); 'Moderately depressed, worrying and pessimistic' ($p = \cdot 0153$); Considerable number of physical complaints. Prominent concern with bodily functions' ($p = \cdot 0442$). In view of the haemodialysis patients' objective condition, the latter two statements appear to have at least face validity. They give little support to Goldstein and Reznikoff's contention that 'Computer-derived statements may erroneously label patients as 'hypochondriacs' when in fact they are chronically physically ill' (p. 158).

As for the first statement, it seems more parsimonious to look for reasons why so few of the controls are said to have normal male interest patterns than to speculate, as Goldstein and Reznikoff do, about denial of physical weakness and reduction in sexual potency on the part of the haemodialysis patients.

T. J. P. Verberne.

Parkville Psychiatric Unit,
35–37 Poplar Road,
Parkville,
Victoria 3052,
Australia.

OXAZEPAM (SERENID D) DEPENDENCE

Dear Sir,

I would refer to Dr. S. M. Hanna's article in the *Journal* (1) concerning oxazepam (Serenid-D) dependence. This occurrence is sufficiently uncommon (2) to indicate an alternative explanation.

https://doi.org/10.1192/bjp.121.2.234-a Published online by Cambridge University Press