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.

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'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.

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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

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