Most puzzling of all is Matthews's assumption that the sorting method he has used to produce Table 4 is necessarily the same as the unpublished programme technique used in my figures, even though they may both be *R*-matrices. Both *R* and *Q* matrices provide useful information about the data fed into them; they merely provide different aspects of the same material.

The mistaken view underlying the whole of Matthews's approach to my work seems to be his belief that I am relying entirely on this one experimental approach to classification. In fact, my whole thesis is devoted to the integration of various unrelated approaches to the same set of material in order to define the measure of agreement and the likely area of the 'real' situation. Any one of the methods used may be, and probably will be, biased by its line of approach. Another basis for misunderstanding is Matthews's apparent lack of comprehension of the pyramidal structure of archæological definitions so that the relationship trait/type is of the same nature as type/assemblage; thus matrices can be used at two different but similar levels (compare Clarke and Tugby with Brainerd). Hence the interchange as compared with Robinson's articles which was concerned with assemblages, the traits defining types show an exactly similar lenticular development and are subject to the same arguments. Just as identical assemblages of types are believed to define a single group or cultural facet so identical sets of traits are believed to define single types.

Finally, I would like to stress that the useful classification of data is the outstanding problem in many different disciplines providing, as it does, the model with which the student is taught and suggesting aspects for research.

The constant revision of classification is an essential task if old teaching models are not to conceal and condemn as ridiculous those lines of thought that imperil their validity and promise useful advance.

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Peterhouse,
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[1] Matthews, J., *Nature*, 198, 930 (1963).
[2] Brainerd, G. W., *Amer. Antiquity*, 16, 4, 301 (1951).
[3] Robinson, W. S., *Amer. Antiquity*, 16, 4, 293 (1951).
[4] Clarke, D. L., *Proc. Prehist. Soc.*, 28, 371 (1962).
[5] Tanimoto, T. T., "An Elementary Mathematical Theory of Classification Prediction", I.B.M. (1958).

I WAS interested to read the preceding communication commenting on my recent article in *Nature*. There are one or two points that I should like to make. The numbered paragraphs refer to the original paragraphs in Clarke's letter.

I think that anyone who is interested can read the papers by Brainerd and Robinson to which Clarke refers, and so judge the validity of Clarke's statements about the length and accuracy of my summary.

(1) Clarke is wrong. Brainerd[1] writes of "the criteria of *constant* combination of attributes in the defining of types". Neither Brainerd nor Robinson, in the articles to which Clarke refers, mentions types in terms of "consistently" recurring groups of traits.

(2) Clarke misquotes me here. I wrote[2], "The chronological purpose is only part of Clarke's endeavour. He is primarily concerned to define kinds and ranges of pottery". But if Clarke's intention is not to place kinds of beaker pottery in a chronological order, why has he concerned himself with Robinson's statistical technique ? This, in Robinson's words, is a method for chronologically ordering archæological deposits. The first part of Clarke's article[3] is a description of the method devised by Robinson, and the second part describes an application of this method to a collection of beaker pottery.

(3) In his article, Clarke states that he has made use of Robinson's method. He does not describe any adaptations that he may have made. He does mention the use of "an electronic brain" (*sic*) to sort his Tables, but this does not entail any modification of the statistical technique. Clarke does not mention either Dr. Needham or Messrs. Easterbrook and Grant in his article.

(4) On reading Clarke's article, I assumed that no coefficient had been entered in the unsorted matrix, and that the numbers of individuals observed to possess two characteristics had been entered directly. There are many suitable coefficients which can be used in this sort of situation[4]. Robinson has a very good coefficient, specially devised for his own method.

(5) This paragraph neatly summarizes Clarke's basic misunderstanding. The assemblages to which Robinson refers are archæological deposits which are compared in terms of the proportions of the various types which they contain. If Clarke were to arrange the beakers into assemblages from various archæological deposits, and then compare these assemblages in terms of the proportions of the various traits they contain, then he might be able to place those deposits in a chronological sequence.

(6) I am not impressed by the agreement which Clarke mentions here. The presentation of the data is in question, not the possible sorting techniques.

(7) Clarke misrepresents me here. I allege nothing in this context. I point out that there is no indication of the number of beakers used in the analysis in either of his two matrices. I also point out that the numbers of individual pots which possess each trait (which would occupy the main diagonal) are also missing from the Tables.

(8) Clarke is right and I am wrong. There is information contained in the matrix which demonstrates that the example to which I refer, concerning the number 141, is not appropriate for the point that I wish to make. I wish to submit the following errata to my article in *Nature*: p. 933, lines 35, 37, 38, 40, p. 934, line 1, for "141" read "149" ; p. 933, line 39, for *E* read *A*, for *F* read *E*; p. 934, line 1, for "six" read "five".

If my suggestion (it is only a suggestion) is inappropriate, then I shall be pleased to examine the beakers which demonstrate its inappropriateness.

(9) Is Clarke wise in his comment about imaginary figures and matrices ? The matrix which he gives in his article as Fig. 2, on which pivot the arguments substantiating his method, is filled with imaginary figures.

Clarke's comments on the matrices which I use as an illustration and which are included at the end of my article are quite interesting—although I am not sure that the point that Clarke is trying to make is very clear; but I am sure that Clarke has missed the point of my illustration.

In his article Clarke describes the statistical technique devised by Robinson for the chronological ordering of archæological deposits; he then applies this method to a collection of beaker pottery, and proceeds to arrange this collection in types and ranges of types which are presumed to have a chronological order. In my article[3] I have mentioned some of the reasons why Robinson's technique cannot be applied to collections of beaker pottery in this way. If Clarke is using some other method to analyse the collection of pottery in terms of significantly recurring groups of traits, then perhaps he could explain the principle on which this method is based and how this method can be expected to achieve his stated purpose.

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[1] Brainerd, G. W., *Amer. Antiquity*, 16, 4, 305 (1951).
[2] Matthews, J., *Nature*, 198, 933 (1963).
[3] Clarke, D. L., *Proc. Prehist. Soc.*, 28, 371 (1962).
[4] Sneath, P. H. A., *Nature*, 193, 855 (1962).