the models; in particular, as the author states, procedures based on conventional asymptotic theory can go wildly wrong if many parameters are used. All aspects except one of the primary models have been generalized in the paper: the restriction is to Euclidean geometry. Is it worth considering putting the points on a (gently curved) Riemannian manifold using geodesic distance as the key characteristic? This would probably make sense only if drastic simplification of other aspects were achieved and the curvatures had some viable interpretation.

The following contributions were received in writing, after the meeting.

Professor A. P. M. COXON (University of Wales, University College Cardiff): Professor Ramsay has pinpointed very nicely a number of important statistical shortcomings in current MDS procedures. In particular. I found the use of monotone splines, for transforming dissimilarity data, a fascinating development and one which is potentially very far-reaching in its applications. It certainly offers a far more justifiable and powerful alternative to the centring and standardizing options usually employed in the attempt to overcome idiosyncratic individual differences in subjects' response styles. The extent of such differences needs emphasis, not least because most users of MDS show a marked reluctance to report them or deal systematically with them. In my experience, it is not uncommon to find a third of one's subjects using the "totally dissimilar" category of a similarity rating scale. Not surprisingly this proportion increases monotonically but in a highly non-linear fashion with the scaled separation of the points. Hence the importance of the monotone spline transformation.

There are two further, but more minor points. First, the presentation of the paper might give the impression that Dr Ramsay's estimation procedures are primarily psychological in motivation. Certainly, the use of aggregate similarity coefficients typically produces rather different values of individual differences and nuisance parameters, but can often be as revealing as those based on individual ratings. Secondly, whilst I can understand the desirability of enclosing point locations with confidence regions, there is a danger that the unsophisticated user will either mistake these for isotonic regions or else be misled into thinking that the problem of local instability of point location in MDS configurations has been effectively solved. Unfortunately this is not so.

Dr FRANK CRITCHLEY (University of Glasgow): As Professor Ramsay indicates, this interesting paper raises a lot of unanswered questions and highlights many inferential difficulties.

I was unfortunately prevented from attending by transport difficulties and would value elucidation of the theoretical and empirical basis of: (a) the sentence in Section 3.1 beginning: "Assuming some prudence..." and (b) the approximation (4.2), in particular its range of application.

I agree entirely with the author's comments on the need for software development and on the desirability of considering monotone transformations that are smooth. Although currently restricted to the special case $R = T = 1$, Critchley (1978) has introduced a method in which parameterised transformations are used and shown how an appeal to parsimony (essentially, to low dimensionality) leads to a method which requires *only* the optimization of a nonlinear function of the transformation parameters, the solution configuration being given in terms of the data and the fitted parameters. The method is thus computationally efficient, the power law requiring a single univariate optimization, and quite general in that any parameterised relation can be accommodated. Transformations of the form:

$$f(d) \propto \sum_{i=1}^{k} \lambda_i\, g_i(d), \quad \text{where } \lambda_i \geqslant 0,\ g_i(0) = 0,\ g_i(\,.\,) \text{ increasing},$$

are appealing and are *linear* in the parameters $\{\lambda_i\}$, simplifying the accompanying theory.

Professor JAN DE LEEUW (University of Leiden): Professor Ramsay's presentation is an impressive and comprehensive review of his approach to MDS. Its three main ingredients are rating scale data, the lognormal distribution, and classical asymptotic maximum likelihood theory. One remark that I have is that the scope of the theory is consequently quite limited. MDS has been used, and will be used, in other sciences than the behavioural sciences, even in the behavioural sciences rating scale data are certainly not the only possible type, and even for rating scale data lognormality and independence of residuals will usually only be rough approximations. And finally, as Ramsay indicates, even for rating scale data for which the models are approximately true classical maximum likelihood theory does not apply. The problem following from this remark is that computer programs such as MULTISCAL do not indicate in any way that their basic assumptions are violated, because they only test additional specifications such as dimensionality within the saturated parametric model. They are consequently used in many cases with the same degree of justification as "non-statistical" approaches to MDS. Nevertheless they tend to induce a false sense of confidence in their users, by providing interval estimates and tests of hypotheses, which look sophisticated and satisfactory but may actually be quite useless or even misleading. The same argument applies to *LISREL*, by the way. My thesis would be that in many cases MDS is not a statistical, in the sense of inferential or inductive, technique, but simply a graphical method. My second thesis is that there is nothing inferior about graphical methods.

A second remark I have is also related to uncontrolled use and development of complicated iterative computer programs, although I also have indulged in this respect. It seems to me that in MDS very complicated and very mysterious functions are being minimized, more precisely, that relatively flat spots in very complicated surfaces are being found (not necessarily local minima, almost certainly no global minima). Taking logarithms in MDS loss functions, as Ramsay does, may be very sensible from an error theory point of view, but it is clear that it introduces nasty discontinuities and is not very wise from a computational point of view. We do not know enough to find the appropriate balance of these two arguments. Surely the programs are not going to teach us what is best. It could very well be true that the simplicity and nicety of classical scaling make it possible to derive useful additional theoretical results (such as Sibson's perturbation theorems) which counterbalance the unwise squaring of residuals.

Dr J. F. P. HUDSON (UI Management Consultants): The author urges model builders and computer programmers to give us a quiver full of MDS tools. But the techniques of fitting MDS data are already ahead of our understanding of what these analyses really mean. The sad fact is that when the "statistician conveys.... to the investigator... how much the data has to tell him" it is often disappointingly little.

The contrast between science and hunch can be seen in the Brahms example; on the one hand we have the careful estimation of confidence ellipses and on the other hand we have the intuitive identification of the factors with *simplicity* and *tempo*. This identification is a conjecture (unless it is intended as a redefinition of the terms). The comment "a number of subjects seem to pay attention only to tempo" is a hypothesis about which the present analysis cannot give us any quantitive evaluation.

We are shown how dramatically the fit can be improved by the introduction of additional parameters, all of which are theoretically acceptable. But I should like to introduce an additional criterion for choosing between models; "How much information does this give us?" We need to distinguish between *internal* and *external* information. By *internal* information I mean information about the modelling technique; by *external* information I mean information about the external world which we are trying to understand. I realize that this is not a strict dichotomy; the question of whether the relationship between similarity data and euclidean distances is linear or nonlinear, continuous or discontinuous, may be to the psychologist an important question about how people really think, whereas to the market researcher it might be a technical detail of the modelling process with no direct bearing on predicting buying behaviour. Nonetheless I believe that this distinction between internal and external information can help us to keep our feet on the ground.

To return to the Brahms example, it would be interesting to see how the final configuration looked under the simpler analyses using lognormal or linear transformations; does the improved fit really tells us more? Personally I was much more interested in the diagram of subject specific dimension weights (fig. 6). This points to some wide variations between individuals responses to the musical passages. Would this diagram have been significantly different without the spline transformations? While continuous transformations have intuitive advantages over the general monotone transformations, do they make any difference to the conclusions?

MDS methods can help to produce an interesting "display... in an intuitively appealing fashion", and can help the formation of interesting hypotheses. The ability of MDS to test useful hypotheses depends on the development of its relationship with other statistical techniques, and on the constant insistance on relevance to the external world.

The AUTHOR replied later, in writing, as follows.

I am very grateful for the many interesting and important points raised in the discussion. A number of critical remarks pertain to how a program such as MULTISCALE might be used in practice. I am largely in agreement with these comments and would merely like to add a few of my own.

Are the models available in MULTISCALE too complex and sophisticated for the quality of data analyzed or the theoretical knowledge available? The answer is certainly yes in many applications. I have indicated in various places in the paper that the user should proceed with caution in extending a baseline model possibly consisting of a two-dimensional fit with identity metric, power transformations, and subject-wise variance components. Only those additional features should be introduced which are either known in advance to be important or are very strongly supported by the data. As Professor Roskam points out, there is seldom a body of theory which argues specifically for a Euclidean distance model.