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Dr. Speed believes that his definition of analysis of variance "is a mathematically fruitful definition, ..., and that its generality and simplicity is both pedagogically and scientifically helpful." I want to consider how far his belief is justified.

I think he has shown beyond doubt that his definition is mathematically fruitful. He has been able to embed the statistical approach based on patterned sets of random variables in an abstract mathematical apparatus of considerable generality. The abstract structures are the same whether we begin with notions of independent random variables or randomization or exchangeability or symmetry. In Nelder (1965), I used randomization to produce covariance structures for a class of structures whose members are formed from combinations of nested and crossed classifications. I gave rules for deriving separately the three representations of V discussed, in Section 6, i.e.,

$$\Gamma = \sum_a \gamma_a \mathbf{A}_a = \sum_b f_b \mathbf{R}_b = \sum_\alpha \xi_\alpha \mathbf{S}_\alpha.$$

It is good to have a complete account in this paper of the relations between them, and in a wider context. I also like bringing together classical anova ideas and those of harmonic analysis. (Lanczos, who said, in effect, "Give me Fourier analysis and you can have the rest of applied mathematics," would have been pleased.)

Does the author make a case for his definition being scientifically helpful? Here I am much less sure. It is significant that he quotes only the first of my two 1965 papers, the one that dealt with the null analysis of variance in the absence of treatments; in the second, I introduced treatment structures and derived the condition of general balance of a treatment structure with respect to a block structure. In my view, designs showing general balance have a well-defined anova which makes both mathematical and statistical sense, and I cannot see any good reason for excluding them. While it is true that arguments based on symmetry cannot, in general, be employed for treatment terms (it matters which level is which in a treatment factor), nonetheless, the sums of squares and their associated subspaces of contrasts are well defined, and indeed unique.

With unbalanced structures most of the nice mathematics becomes unavailable and the statistical problems mount. The statistician must still cope. He may have to examine several sequential fits, producing sequential anovas, in order to make inferences. The sequencing of terms may be guided by the existence of a minimal model, sizes of effects, marginality relations between main effects and their interactions, and functional marginality between terms in a polynomial response [Nelder (1977) and McCullagh and Nelder (1983)]. None of these statistical ideas, all of which are important in scientific applications, can be expressed in the framework of this paper. There seems to me a good case for accepting as a possible anova one deriving from a sequential fit, provided only that the error components of all the terms are homogeneous.

I have recently had my say elsewhere [Nelder (1986)] on the relations between statistics, mathematics, science and technology. If I had to make choices as a teacher I would be forced to give first priority to the statistical ideas underlying the analysis of variance (in my general sense) and its generalization, the analysis of deviance. This is not to say that the student should not be aware of the mathematics of balanced random structures; rather that (s)he should also be aware that the scientist and technologist will be interested primarily in the systematic part of the model (signal), and will regard the random part (noise) as an unavoidable nuisance. I believe that the analysis of variance, in its existing wider sense, has a useful part to play in the interpretation of the signal, so I should not like to see its use restricted to the description of the noise only.

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First of all, let me congratulate the author on an interesting paper. Though I find his use of the phrase analysis of variance somewhat unusual, it is certainly important to be aware that the mathematical tools in such seemingly different statistical procedures as that of analyzing a split plot experiment and that of analyzing a time series are, in fact, aspects of the same algebraic theory for spectral decompositions of covariance matrices.

I would like to stress a point related to the discussion of infinite arrays. What is said below can be read between the lines of Section 5, but I think it deserves more attention that the concept of an infinite array constitutes the ideal framework for discussion and clarification of the classical (and, unfortunately, slightly controversial) problem of negative variance components.

Very briefly, there are two kinds of variance component models:

(1) random effect models stated in terms of sums of independent random effects (error terms); and
(2) models stated in terms of covariances (as in Section 4).