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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.