not buy anything in the end. Still, it should not be ignored that the *symmetries* of a collection of data—that is, the permutations with respect to which it is invariant, in the sense of the statistical information it contains—are often the most basic way of getting at the structure of the collection. Certainly the dispersion model basis matrices A_a do not appear intuitively in the mind of the experimenter; the individual factors as equivalence relations might, but not their nesting structure, at least not for structures of any complexity. But, I assert, what the experimenter should always be able to answer, upon a little reflection, is the question, "In which ways can we arbitrarily swap around the data without affecting the conclusions we should make?" Thus, the group theory approach may have useful ramifications for practical statistical consulting, in discovering in the first place from the experimenter what the structure of the data is.

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We must respect the many long steps that Speed has taken to understand, focus and describe a mathematical structure for what R. A. Fisher may have sensed in introducing the analysis of variance before 1925. But we dare not regard it as telling us why the analysis of variance deserves the great practical importance that it has held throughout recent decades.

I am not equipped to comment adequately on the mathematical niceties and careful craftmanship of Speed's paper. I do have an obligation, however, to point out why what he describes as the analysis of variance is *not* the core of what is practiced in so many areas of application.

The work of Pitman (1937, 1939) showed clearly that the practice of analysis of variance need not depend on Gaussian (normal) distributions, that it made sense in much broader circumstances than those "close to Gaussian." In my experience, most applications of the practical analysis of variance are exploratory in nature, and deserve an inherently flexible approach. Informally, those who have made, or handled, many such analyses have learned to include flexibility in such matters as how many interactions to include—and when to combine two (or more) factors into a composite factor and when to leave them separate.

The formalization of such flexibility has lagged. Green and Tukey (1960) illustrated an approach that has not been widely followed. Johnson and Tukey (1987) have now taken this flexibility several steps further. (In his nearly completed Ph.D. thesis, Johnson is taking still further steps.)

There need be no conflict between Speed's important improvements in the mathematical description of a narrower process and the clarification and exposition of a broader one. I trust there will be none.

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REJOINDER

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> ... I maintain that any writer of a book is fully authorised in attaching any meaning he likes to any word or phrase he intends to use. If I find an author saying, at the beginning of his book, "Let it be understood that by the word '*black*' I shall always mean '*white*,' and that by the word '*white*' I shall always mean '*black*,'" I meekly accept his ruling, however injudicious I may think it.
>
> Lewis Carroll [cited in Gardner (1960)]

In writing about models for the dispersion matrices of arrays of random variables, which are defined by equality constraints on the entries, and calling it anova, I tried to emphasise a unity between parts of theoretical statistics which I felt was not immediately apparent. Of course, the very wide variety of sums of