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THE ASSUMPTIONS UNDERLYING THE ANALYSIS OF VARIANCE*

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1. *Introductory Remarks.* The statistical technique known as "analysis of variance," developed more than two decades ago by R. A. Fisher to facilitate the analysis and interpretation of data from field trials and laboratory experiments in agricultural and biological research, today constitutes one of the principal research tools of the biological scientist, and its use is spreading rapidly in the social sciences, the physical sciences, and in engineering. Numerous textbooks (or, should I say "manuals"?) have been published—and, I dare say, many more are being written—that aim to provide their readers with a working knowledge of the steps of analysis-of-variance procedure with a minimum exposure to mathematical formulas and mathematical thinking. Designed expressly for the "non-mathematical reader", whose mathematical equipment is presumed to be a reasonable competence in arithmetic and elementary algebra—mere previous exposure to these subjects is not enough. The method of instruction adopted in these books consists chiefly in guiding the reader by easy stages through a series of worked examples that are typical of the more common problems amenable to analysis of variance that arise in the scientific or engineering field with which the author of the book concerned is conversant.[1]

* An expository address delivered at a joint session of the Biometrics Section of the American Statistical Association and the Institute of Mathematical Statistics, held on December 28, 1946, in conjunction with the 113th Annual Meeting of the American Association for the Advancement of Science, Boston, Massachusetts.

[1] The author has found the discussions and examples of analysis-of-variance procedures given in the following four books especially valuable both for reference and for purposes of instruction: C. H. Goulden, *Methods of Statistical Analysis;* G. W. Snedecor, *Calculation and Interpretation of Analysis of Variance and Covariance;* G. W. Snedecor, *Statistical Methods;* L. H. C. Tippett, *The Methods of Statistics.* See full bibliographical references at the end of this paper.

These introductions to analysis of variance have been definitely worthwhile in at least three respects: first, they have acquainted a larger audience with the procedures of analysis of variance and its value as a research tool than probably would have been achieved by more mathematical expositions of the subject (even unfavorable reviews of some of these books have focused attention on the analysis of variance itself as a research tool "that needs further looking into"); second, by studying the worked examples provided and by carrying through analogous steps with data of their own, readers of these books have developed an amazing proficiency with the arithmetical steps involved, even in the cases of analyses associated with fairly complicated experimental designs which probably would not have been attempted or, if attempted, almost certainly would not have been analyzed correctly without the aid of these books; and third, since the worked examples given in these books have generally illustrated statistically sound experimental designs which were more efficient than the designs previously used by their readers, these readers have frequently adopted analogous designs in their own research (in order to be able to follow the book when the data are in and are crying for analysis), with a resulting general improvement of research procedure.

The principal deficiency of these books has been their failure to state explicitly the several asumptions underlying the analysis of variance, and to indicate the importance of each from a practical viewpoint. The mathematical treatments of analysis of variance have shared this deficiency to some extent, for, while they have posited the necessary and sufficient conditions[2] for strict validity of the entire set of analysis-of-variance procedures and associated tests of significance, they have not generally indicated in sufficient detail the actual functions of the respective assumptions—1) which can be dispensed with for certain purposes; 2) which are absolutely necessary, and what are likely to be the consequences if these are not fulfilled; and 3) what can be done "to bring into line," for purposes of analysis, data which in their original form are not amenable to analysis of variance.[3] In this paper I shall go into these matters in some detail. My assignment is to enumerate the several assumptions underlying the analysis of variance and to point out the practical importance of each. As we shall see, these assumptions are quite simple to state, and the practical significance of each not difficult to grasp. Professor Cochran, in the second paper of this issue of *Biometrics*, tells of some of the consequences to expect when certain of these assumptions are not fulfilled. Finally, Professor Bartlett, in the third paper, indicates how, by the use of transformations, some of these consequences can be avoided and valid conclusions reached by analysis of variance, when the data in their original form are essentially intractable by analysis of variance.

2. *Two Distinct Classes of Problems Solvable by Analysis of Variance.* Turning now to my assignment, I am obliged at the outset to draw attention to the fact that analysis of variance can be, and is, used to provide solutions to problems of two fundamentally different types. These two distinct classes of problems are:

(2.1) *Class I: Detection and Estimation of Fixed (Constant) Relations Among the Means of Sub-Sets of the Universe of Objects Concerned.* This class includes all of the usual problems of estimating, and testing to determine whether to infer the existence of, *true* differences among "treatment" means, among "variety" means, and, under certain conditions, among "place" means. Included in this class are all the problems of univariate and multivariate regression and of harmonic analysis. With respect to the problems of estimation belonging to this class, analysis of variance is simply a form of the method of least squares: the analysis-of-variance solutions are the least-squares solutions. The cardinal contribution of analysis of variance to the actual procedure is the *analysis-of-variance table* devised by R. A. Fisher, which serves to simplify the arithmetical steps and to bring out more clearly the significance of the results obtained. The analysis-of-variance tests of significance employed in connection with problems of this class are simply extensions to small samples of the theory of

[2] The conditions here referred to are certainly sufficient: they may be necessary in the mathematical sense, but no proof of this is known to the writer. For a variety of reasons he believes them to be "necessary in practice" in the same sense that, if there are exceptions, the circumstances required would be regarded by the practical man as "pathological."

[3] See, for example, the discussions of analysis of variance in H. Cramér, *Mathematical Methods of Statistics;* in M. G. Kendall, *The Advanced Theory of Statistics,* Volume II; and in S. S. Wilks, *Mathematical Statistics.* Somewhat more complete discussions have been given by J. O. Irwin in his paper entitled "Mathematical Theorems Involved in the Analysis of Variance" and in his note "On the Independence of the Constituent Items in the Analysis of Variance."

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least squares developed by Gauss and others—the extension of the theory to small samples being due principally to R. A. Fisher.

(2.2) *Class II: Detection and Estimation of Components of (Random) Variation Associated with a Composite Population.* This class includes all problems of estimating, and testing to determine whether to infer the existence of, components of variance ascribable to random deviation of the characteristics of individuals of a particular generic type from the mean values of these characteristics in the "population" of all individuals of that generic type, etc. In a sense, *this is the true analysis of variance,* and the estimation of the respective components of the over-all variance of a single observation requires further steps beyond the evaluations of the entries of the analysis-of-variance table itself. Problems of this class have received considerably less attention in the literature of analysis of variance than have problems of Class I.[4]

The failure of most of the literature on analysis of variance to focus attention on the distinction between problems of Class I and problems of Class II is very likely due to two facts: first, the literature of analysis of variance deals largely with tests of significance in contrast to problems of estimation; second, when analysis of variance is used merely to determine whether to infer (a) the existence of fixed differences among the true means of the sub-sets concerned or (b) the existence of a component of variance ascribable to a particular factor, the computational procedure and the mechanics of the statistical tests of significance are the same in either case—the same test criterion (F *or* z) is evaluated and referred to the same "levels of significance" in either case. On the other hand, in the estimation of the relevant parameters, and in the evaluation of the efficiency or resolving power of a particular experimental design, the distinction between these two classes of problems needs to be taken into account, since in problems of Class I the parameters involved are *means* and the issues of interest are concerned with the interrelations of these means, i.e., with the differences between pairs of them, with their functional dependence on some independent variable(s), etc.; whereas in problems of Class II the parameters involved are *variances* and their absolute and relative magnitudes are of primary importance. In other words, the mathematical models appropriate to problems of Class I differ from the mathematical models

[4] R. A. Fisher gives a brief discussion of estimating, and testing for the existence of components of variance in Section 40 of his *Statistical Methods for Research Workers.* Tippett considers such problems in Sections 6.1–6.2, 6.4, 10.11, and 10.3. Snedecor's treatment is somewhat more complete: *Statistical Methods,* Sections 10.6–10.12, 11.4, 11.7–11.8, 11.14, 11.16. The most complete discussions of problems of Class II are H. E. Daniels, "The Estimation of Components of Variance," and S. Lee Crump, "The Estimation of Variance Components in Analysis of Variance."

appropriate to problems of Class II, and consequently, so do the questions to be answered by the data.

3. *The Algebra of Analysis of Variance.* It was remarked above that the computational steps leading to an analysis-of-variance table are the same for problems of Class I and Class II. This is due largely to the fact that the decomposition of the (total) sum of squared deviations of the individual observations from the general mean of the observations into two or more "sums of squares" is based in every case upon an algebraic identity (appropriate to the case concerned) that is valid whatever the meanings of the numbers involved. To demonstrate this in complete generality would render the substance of this paper somewhat complicated, and these complexities would, I fear, distract attention from the main theme. Accordingly, I shall restrict myself to consideration of the algebra of the decomposition for the case of rc numbers arranged in a rectangular array of r rows and c columns. In order to be able to identify the various numbers, let us denote by x_{ij} the number occurring in the i^{th} row and j^{th} column of this array. If we border the rectangular array with a column of row means and a

TABLE 1

Row \ Column	1	2	3	·	·	·	·	·	j	·	·	C	Row Means
1	X_{11}	X_{12}	X_{13}	·	·	·	·	·	X_{1j}	·	·	X_{1C}	$X_{1.}$
2	X_{21}	X_{22}	X_{23}	·	·	·	·	·	X_{2j}	·	·	X_{2C}	$X_{2.}$
·	·	·	·	·	·	·	·	·	·	·	·	·	·
·	·	·	·	·	·	·	·	·	·	·	·	·	·
·	·	·	·	·	·	·	·	·	·	·	·	·	·
i	X_{i1}	X_{i2}	X_{i3}	·	·	·	·	·	X_{ij}	·	·	X_{iC}	$X_{i.}$
·	·	·	·	·	·	·	·	·	·	·	·	·	·
·	·	·	·	·	·	·	·	·	·	·	·	·	·
r	X_{r1}	X_{r2}	X_{r3}	·	·	·	·	·	X_{rj}	·	·	X_{rC}	$X_{r.}$
Col. Means	$X_{.1}$	$X_{.2}$	$X_{.3}$	·	·	·	·	·	$X_{.j}$	·	·	$X_{.C}$	$X_{..}$

row of column means, then we have a situation such as that portrayed in Table 1, where $x_{i.}$ denotes the arithmetic mean of the c values of x in the i^{th} row, $x_{.j}$ denotes the arithmetic mean of the r values of x in the j^{th} column, and $x_{..}$ denotes the arithmetic mean of all the rc values in the array.

It is evident that the following is an algebraic identity *whatever the interpretation of the numbers x_{ij} involved:*

(1) $\quad (x_{ij}-x_{..}) = (x_{i.}-x_{..}) + (x_{.j}-x_{..}) + (x_{ij}-x_{i.}-x_{.j}+x_{..}).$

Remembering that by definition the arithmetic mean of m values of a quantity y is (sum of the m values)$/m$ we see that

(2) $\quad$ Arithmetic Mean of $y \equiv \bar{y} \equiv \dfrac{1}{m}S(y)$ implies $S(y-\bar{y}) = 0,$

and

$$(3) \qquad S(y-\bar{y})^2 = S(y^2) - \frac{[S(y)]^2}{m},$$

where S denotes summation over all the values of y involved. Squaring both sides of (1) and summing over all rc observations, the algebraic identity

$$(4) \quad \underset{(A)}{S(x_{ij}-x_{..})^2} = \underset{(B)}{S(x_{i.}-x_{..})^2} + \underset{(C)}{S(x_{.j}-x_{..})^2} + \underset{(D)}{S(x_{ij}-x_{i.}-x_{.j}+x_{..})^2}$$

results, where S denotes summation over all the values in the entire array; the cross-products involved sum to zero by virtue of (2), *on account of the fact that $x_{i.}$, $x_{.j}$, etc., and $x_{..}$ are means.* The (A), (B), (C), and (D) sums of squared quantities in (4) are what are usually referred to in an analysis-of-variance table as the "total", the "between-row-means", the "between-column-means", and "residual" sums of squares, respectively. Since $(x_{i.}-x_{..})^2$ is identically the same for each of the c observations in the i^{th} row, and $(x_{.j}-x_{..})^2$ is the same for each observation in the j^{th} column, it is sometimes convenient to write (4) as

$$(5) \quad \underset{(A)}{\sum_{i=1}^{r}\sum_{j=1}^{c}(x_{ij}-x_{..})^2} = \underset{(B)}{c\sum_{i=1}^{r}(x_{i.}-x_{..})^2} + \underset{(C)}{r\sum_{j=1}^{c}(x_{.j}-x_{..})^2} + \underset{(D)}{\sum_{i=1}^{r}\sum_{j=1}^{c}(x_{ij}-x_{i.}-x_{.j}+x_{..})^2}$$

where $\sum_{i=1}^{r}\sum_{j=1}^{c}$ denotes summation over all the observations in the array, $\sum_{i=1}^{r}$ denotes summation only over i, $i=1$ to $i=r$, and $\sum_{j=1}^{c}$ denotes summation only over j, for $j=1$ to $j=c$.

(3.1) *"Practical" Formulas.* With the aid of the identity (3), it is easy to derive the "practical" formulas used for calculation:

$$(A) \quad \sum_{i=1}^{r}\sum_{j=1}^{c}(x_{ij}-x_{..})^2 = \sum_{i=1}^{r}\sum_{j=1}^{c}(x_{ij}^2) - \frac{[\Sigma\Sigma(x_{ij})]^2}{rc} =$$

$$\text{Sum of the squares of all observations} - \frac{[\text{Sum of all observations}]^2}{\text{Number of observations}},$$

$$(B) \quad c\sum_{i=1}^{r}(x_{i.}-x_{..})^2 = \sum_{i=1}^{r}\frac{(cx_{1.})^2}{c} - \frac{\sum_{i=1}^{r}(cx_{i.})^2}{cr}$$

$$(6) \quad = \text{Sum with respect to } i \text{ of } \frac{(i^{\text{th}}\text{-row Total})^2}{c} - \{\text{Correction Term, given above}\},$$

$$(C) \quad r\sum_{j=1}^{c}(x_{.j}-x_{..})^2 = \sum_{j=1}^{c}\frac{(rx_{.j})^2}{r} - \frac{\left[\sum_{j=1}^{c}(rx_{.j})\right]^2}{rc} =$$

$$\text{Sum with respect to } j \text{ of } \frac{(j^{\text{th}}\text{-Column Total})^2}{r} - \textit{idem},$$

$$(D) \quad \sum_{i=1}^{r}\sum_{j=1}^{c}(x_{ij}-x_{i.}-x_{.j}+x_{..})^2 = (A)-(B)-(C).$$

I repeat: All of the familiar formulas and procedures for evaluating component "sums of squares" that add up to the "total sum of squares" are based on algebraic identities, and are valid as descriptions of properties of the data whatever the interpretation of the numbers involved. Indeed, the fact that the "components" add up to the total is an algebraic (or, should I say a "geometric") property and means that (and will only happen when) the respective component "sums of squares" are themselves the squares of, or sums of the squares of, linear combinations of the observations that summarize mutually distinct properties of the data, or, as a geometer would say, linear combinations that define mutually orthogonal vectors in the N-dimensional sample

space.[5] Similarly, all of the familiar formulas and procedures for evaluating regression coefficients and the sum of squared deviations from the fitted regression, when the fitting is by the method of least squares, are based upon algebra and calculus, and the results obtained are valid as descriptions of properties of the data in hand, whatever the interpretation of the numbers involved.

In summary, when the formulas and procedures of analysis of variance are used merely to summarize properties of the data in hand, no assumptions are needed to validate them. On the other hand, when analysis of variance is used as a method of statistical inference, for inferring properties of the "population" from which the data in hand were drawn, then certain assumptions, about the "population" and the sampling procedure by means of which the data were obtained, must be fulfilled if the inferences are to be valid.

4. *The Assumptions Underlying the Use of Analysis of Variance as a Method of Statistical Inference.* As was remarked earlier, analysis of variance can be, and is, used to provide solutions to two fundamentally different types of problems: On the one hand, it can be used to detect the existence of, and to estimate the parameters defining, fixed (constant) relations among the population means. These were referred to as problems of Class I. On the other hand, analysis of variance can be used to detect the existence of, and to estimate, components of variance. These were termed problems of Class II. To formulate with complete generality the mathematical models upon which the solutions of problems of Class I and Class II by analysis of variance are based would render the substance of this paper somewhat complicated from this point on, and would, I fear, divert attention from the really important distinctions between the two different models, and from the differences between the assumptions required in order to be able to draw valid inferences by analysis of variance in the two cases. Therefore,

[5] To see what we mean by "mutually distinct" and by "orthogonal" in practical language, let us note that, if in the case of numbers arranged as in Table 1 we add a single arbitrary constant to each of the numbers in the first column, a different arbitrary constant to each of the numbers in the second column, and so forth through the c^{th} column, then the several row means will be altered by different amounts, which will be determined by the actual constants added, but the several row means will all be altered by the same amount, so that the *difference* between any pair of row means, $(x_{i_1\cdot} - x_{i_2\cdot})$, will be unchanged. Similarly the values of such quantities as $(x_{i\cdot} - x_{\cdot\cdot})$ and $(x_{ij} - x_{i\cdot} - x_{\cdot j} + x_{\cdot\cdot})$ will be unchanged by this tampering with the columns, so that the "between-row-means" and the "residual" sums of squares will be unchanged also. This is because *differences among row means* (or *differences of row means from the general mean*) and the *residuals* are *orthogonal* to *differences among column means* (or *differences of column means from the general mean*), that is, summarize mutually distinct properties of the actual numbers involved. This little trick of adding arbitrary constants in accordance with a definite pattern is a convenient practical way of checking whether particular combinations of the observations are mutually orthogonal.

two different models appropriate to data arranged as in Table 1 will be considered in detail and the relation of each assumption to the inferential steps indicated:

(4.1) *Model I, Special Case: Parameters Are Population Means.* Numbers x_{ij} arranged as in Table 1 do not lie within the province of mathematical statistics, nor can any statistical inferences be based upon them, unless it is assumed that they are (observed values of) *random variables* of some sort. Therefore, in order to bring the discussion within the province of statistical inference we must make

> *Assumption 1* (*Random Variables*): The numbers x_{ij} are (observed values of) random variables that are distributed about true mean values m_{ij}, $(i = 1, 2, \ldots, r;\ j = 1, 2, \ldots, c)$, that are fixed *constants*.

In statistical language this assumption states that, if some particular type of experiment leading to numbers arranged as in Table 1 were repeated indefinitely, then the numbers occurring in the i^{th} cell of the j^{th} column would vary at random about an average value equal to m_{ij}, which is, therefore, a parameter that characterizes the expected value of the number x_{ij}. If, for example, the several rows of Table 1 correspond to different "varieties" and the several columns to different "treatments," then m_{ij} is the so-called *true* (or expected) *yield* of the i^{th} "variety" when subjected to the j^{th} "treatment," under certain growing conditions.

Clearly the parameters m_{ij} can be arranged in a table analogous to Table 1, and bordered by the row-wise means $m_{i\cdot}$, $(i = 1, 2, \ldots, r)$, and the column-wise means $m_{\cdot j}$, $(j = 1, 2, \ldots, c)$ of these parameters, to which may be added, in the lower right corner, the mean $m_{\cdot\cdot}$ of all rc of these parameters. If one is merely interested in obtaining unbiased estimates of mean differences such as $m_{12} - m_{52}$, e.g., of the mean difference between variety 1 and variety 5 under treatment 2, then Assumption 1 is sufficient, and $x_{12} - x_{52}$ provides the desired estimate. More generally, Assumption 1 implies that an unbiased estimator of any linear function of the m_{ij} with *known* coefficients is provided by the same linear function of the x_{ij}. Furthermore, if the variances of the x_{ij} about their respective means and their intercorrelations are known, then the variance of any linear function of the x_{ij} can be evaluated, and provides a measure of the precision of this linear function of the x_{ij} as an unbiased estimator of the corresponding linear function of the m_{ij}.

On the other hand, when the entries m_{ij} of such a table of *true*

means are simple *additive functions* of the corresponding marginal means and the general mean, that is, when

$$m_{ij} = m_{..} + (m_{i.} - m_{..}) + (m_{.j} - m_{..}), \tag{7}$$

for $i = 1, 2, \ldots, r$ and $j = 1, 2, \ldots, c$, then the statistical inferences that may be based upon the x_{ij} are of a much more satisfactory sort. For instance, when (7) is satisfied, the difference between an arbitrary pair of row-wise marginal means, e.g., $m_{1.}$ and $m_{2.}$, is a *comprehensive* measure of the average difference in effectiveness of the factors identified with these rows. When (7) is not satisfied, then $m_{1.} - m_{2.}$ is merely a measure of the average difference between the effects of the corresponding row factors *when the column factors are as in the experiment concerned.* In other words, when additivity, as defined by (7), does not obtain, then it is not possible to define *the* mean difference in effectiveness of any given pair of the row factors, since the actual mean difference in effectiveness of these row factors will depend upon the column factor(s) concerned; and, conversely, the actual mean difference in effectiveness of a pair of column factors will depend upon the row factor(s) concerned. Hence, when additivity does not prevail, we say that there are *interactions* between row factors and column factors. Thus, in the case of varieties and treatments considered above, additivity implies that, under the general experimental conditions of the test, the true mean yield of one variety is greater (or, less) than the true mean yield of another variety by an amount—an additive constant, not a multiplier—that is the same for each of the treatments concerned, and, conversely, the true mean yield with one treatment is greater (or, less) than the true mean yield with another treatment by an amount that does not depend upon the variety concerned; which is exactly what is meant when we say that there are no "interactions" between varietal and treatment effects.

Therefore, in order to dispense with interactions and thus make possible the drawing of general inferences from the x_{ij}, let us make

Assumption 2 (Additivity):[6] the parameters m_{ij} are related to the

[6] In its most general form Model I involves N random variables $x_1, x_2, \ldots, x_N$ with mean values m_i, $(i = 1, 2, \ldots, N)$, and it is assumed that the m_i are linear functions of $s < N$ *unknown* parameters θ_j, $(j = 1, 2, \ldots, s)$, with *known* coefficients, c_{ij}, the matrix of which is non-singular; thus

$$m_i = c_{i1}\theta_1 + c_{i2}\theta_2 + \ldots + c_{is}\theta_s \cdot \qquad (i = 1, 2, \ldots, N),$$

and non-singularity of the matrix $||\, c_{ij} \,||$ signifies that from this set of N equations it is possible to select at least one system of s equations that is soluble with respect to the θ's.

This is known as the *general linear hypothesis.* For details, see the papers by F. N. David and J. Neyman, by S. Kolodziejczyk, and by P. C. Tang cited in the list of references.

means $m_{i.}$, $m_{.j}$, and $m_{..}$ as specified by (7), for $i = 1, 2, \ldots, r$ and $j = 1, 2, \ldots, c$.

When *Assumption 1* and *Assumption 2* are satisfied, then the difference between any pair of row-wise means of the observations x_{ij}, e.g., $x_{2.} - x_{5.}$, is an unbiased estimator of the *general* average difference in effectiveness of the row factors concerned, i.e., of $m_{2.} - m_{5.}$ in this case; and, similarly, the difference between any pair of column-wise means of the observations is an unbiased estimator of the *general* average difference in effectiveness of the column factors concerned. Furthermore, since such estimators are linear functions of the x_{ij}, the variances of these estimators can be evaluated readily when the variances and intercorrelations of the x_{ij} are known. On the other hand, if these variances and intercorrelations are unknown—the usual case in practice—then it is not possible to derive from the observed values of the x_{ij}, $(i = 1, 2, \ldots, r;\ j = 1, 2, \ldots, c)$, an unbiased estimate of the variance of any single x_{ij}, or of any particular linear combination of them, unless certain additional conditions are fulfilled by the x_{ij}. For instance, if the x_{ij} are mutually uncorrelated,[7] and if variance of the x_{ij} are given by

$$\text{variance of } x_{ij} = \frac{\sigma^2}{w_{ij}} \tag{8}$$

where the relative "weights," w_{ij}, are *known* constants, $(i = 1, 2, \ldots, r;\ j = 1, 2, \ldots, c)$, and σ^2 is an *unknown* constant, then an unbiased estimate of σ^2, and thence unbiased estimates of the variances of linear combinations of the x_{ij}, can be derived from the observations x_{ij} by the method of least squares. For details see, for example, the paper by F. N. David and J. Neyman cited in the list of references. They assume the x's to be mutually independent, whereas it is sufficient to assume that they are mutually uncorrelated.

It should be noted here that thus far the only motivation that has been given for the making of *Assumption 2* is the more general nature of the inferences that may be drawn from the observed means $x_{i.}$ and $x_{.j}$, $(i = 1, 2, \ldots, r;\ j = 1, 2, \ldots, c)$, when it is satisfied. We shall now show that, in general, it is not possible to derive from the observations x_{ij} by the *usual analysis-of-variance procedures,* unbiased estimates of the variances of the x_{ij}, and thence of any particular linear combinations of them, unless *Assumption 1, Assumption 2,* and *Assumption 3,* given below, are *all* satisfied.

[7] That is, if the *covariances* $\mathcal{E}\{[x_{ij} - \mathcal{E}(x_{ij})]\ [x_{pq} - \mathcal{E}(x_{pq})]\}$, where $(i, j) \neq (p, q)$, (i and $p = 1, 2, \ldots, r$; j and $q = 1, 2, \ldots, c$), and $\mathcal{E}$ denotes "expected value of," are *all* equal to zero.

Assumption 3 (*Equal Variances and Zero Correlations*) : The random variables x_{ij} are *homoscedastic* and *mutually uncorrelated,* that is, they have a common variance σ^2 and all covariances among them are zero.

The foregoing pronouncement can be demonstrated readily by considering the analysis-of-variance table shown as Table 2. This repre-

TABLE 2

ANALYSIS OF VARIANCE
(Non-Additive Case)

Variation	Degree of Freedom	Sums of Squares	Mean Square	Expected Value of Mean Square
Between Row Means	$r-1$	$S(X_{i\cdot}-X_{\cdot\cdot})^2$	$S(X_{i\cdot}-X_{\cdot\cdot})^2/(r-1)$	$\sigma^2+S(m_{i\cdot}-m_{\cdot\cdot})^2/(r-1)$
Between Column Means	$c-1$	$S(X_{\cdot j}-X_{\cdot\cdot})^2$	$S(X_{\cdot j}-X_{\cdot\cdot})^2/(c-1)$	$\sigma^2+S(m_{\cdot j}-m_{\cdot\cdot})^2/(c-1)$
Residual	$(r-1)(c-1)$	$S(X_{ij}-X_{i\cdot}-X_{\cdot j}+X_{\cdot\cdot})^2$	$S(X_{ij}-X_{i\cdot}-X_{\cdot j}+X_{\cdot\cdot})^2/(r-1)(c-1)$	$\sigma^2+S(m_{ij}-m_{i\cdot}-m_{\cdot j}+m_{\cdot\cdot})^2/(r-1)(c-1)$
Total	$rc-1$	$S(X_{ij}-X_{\cdot\cdot})^2$	$S(X_{ij}-X_{\cdot\cdot})^2/(rc-1)$	$\sigma^2+S(m_{ij}-m_{\cdot\cdot})^2/(rc-1)$

sents the situation when *Assumption 1* and *Assumption 3* are both satisfied, but *Assumption 2* is not. We notice that under these conditions each of the "mean squares" customarily evaluated in such cases will have, in general, an expected value larger than σ^2. If, on the other hand, *Assumption 2* is satisfied also, then the "residual" mean square will be an unbiased estimator of σ^2, the variance of any single observation x_{ij}. This situation is portrayed in Table 3. Hence, when *Assumption 1, Assumption 2,* and *Assumption 3* are all satisfied, an unbiased estimate of the variance of the difference of two *observed* row means can be evaluated from 2(residual mean square)$/c$; and an unbiased estimate of the variance of the difference of two *observed* column means, from 2(residual mean square)$/r$. Furthermore, under these conditions the between-row-means mean square in general will *tend* to exceed the residual mean square, and this tendency will be greater when the true row means, the $m_{i\cdot}$, differ markedly in magnitude than when they differ only slightly. Similarly, the between-column-means mean square in general will tend to exceed the residual mean square by an amount that depends upon the degree of "scatter" of the true column means, the $m_{\cdot j}$, about $m_{\cdot\cdot}$, the mean of all the m_{ij}. Thus we have yardsticks for

judging whether there exist real differences among the true means for the row factors, and for the column factors. Unfortunately, however, our yardsticks have no scales, i.e., probability levels, marked on them, so that with them we cannot conduct exact tests of significance corresponding to previously agreed upon probability levels. In order to be able to do this, the form of the joint distribution of the x_{ij} must be specified. To this we shall return in a moment.

TABLE 3

ANALYSIS OF VARIANCE
(Additive Case)

Variation	Degree of Freedom	Sum of Squares	Mean Square	Expected Mean Square
Between Row (Variety) Means	$r-1$	$S(X_{i\cdot}-X_{\cdot\cdot})^2$	$S(X_{i\cdot}-X_{\cdot\cdot})^2/(r-1)$	$\sigma^2+S(m_{i\cdot}-m_{\cdot\cdot})^2/(r-1)$
Between Column (Cultivation) Means	$c-1$	$S(X_{\cdot j}-X_{\cdot\cdot})^2$	$S(X_{\cdot j}-X_{\cdot\cdot})^2/(c-1)$	$\sigma^2+S(m_{\cdot j}-m_{\cdot\cdot})^2/(c-1)$
Residual	$(r-1)(c-1)$	$S(X_{ij}-X_{i\cdot}-X_{\cdot j}+X_{\cdot\cdot})^2$	$S(X_{ij}-X_{i\cdot}-X_{\cdot j}+X_{\cdot\cdot})^2/(r-1)(c-1)$	σ^2
Total	$rc-1$	$S(X_{ij}-X_{\cdot\cdot})^2$	$S(X_{ij}-X_{\cdot\cdot})^2/(rc-1)$	$\sigma^2+\dfrac{S(m_{i\cdot}-m_{\cdot\cdot})^2+S(m_{\cdot j}-m_{\cdot\cdot})^2}{rc-1}$

At this juncture let us pause for an instant to note that *it has not been necessary to postulate mutual* INDEPENDENCE *of the x_{ij} in order to achieve Table 3 and the results deducible from it*—for these, *existence of the mean values of the x_{ij}* (Assumption 1), *additivity* (Assumption 2), *and equal variances and zero covariances* (Assumption 3) *are sufficient.*

Also, let us examine the situation where *Assumption 1* and *Assumption 2* are satisfied, but *Assumption 3* is not. In this case the four values of σ^2 that appear in the last column of Table 3 must be replaced, in general, by four *different* quantities, which we may denote by σ_1^2, σ_2^2, σ_3^2, and σ_4^2. In general these will be complex weighted means of the variances and covariances of the x_{ij}, and the neatness of Table 3 is lost.

In summary, if *Assumption 1* is satisfied, but if either *Assumption 2,* or *Assumption 3,* or both, is (are) not satisfied, then the strict validity of analysis of variance as a method of solution of problems of Class I vanishes out the window.

Finally, even when *Assumption 1, Assumption 2,* and *Assumption 3*

are satisfied, it is still not possible to conduct exact tests of significance based on the x_{ij} alone, e.g., tests of significance based upon Fisher's z- or Snedecor's F-distributions. Fortunately, *normality,* in addition to *Assumptions 1–3,* is sufficient for exact tests of significance. Therefore let us make

Assumption 4 (Normality): The x_{ij} are jointly distributed in a multivariate normal (Gaussian) distribution.

It may be noted that when *Assumption 4* is satisfied, *Assumption 1* is partially redundant, and serves principally to define the parameters m_{ij}. Furthermore, zero covariances, as postulated in *Assumption 3,* taken in conjunction with normality, postulated in *Assumption 4,* imply mutual independence of the x_{ij}. Thus independence finally sneaks in by the back door, so to speak.

When *Assumptions 1–4* are all satisfied, then all of the usual analysis-of-variance procedures for estimating, and testing to determine whether to infer the existence of, *fixed linear relations,* e.g., non-zero differences, among population *means,* are strictly valid. In particular, an unbiased estimator of any given linear function of the parameters m_{ij} is provided by the identical linear function of the observations x_{ij}, an unbiased estimate of its variance can be derived from the "residual" mean square and exact confidence limits for the value of the given linear function of the parameters can be deduced with the aid of Student's t-distribution. Furthermore, when the row-wise population means, the $m_{i.}$, are all equal, then the quotient ("between-row-means" mean square)/("residual" mean square) will be distributed according to Snedecor's F-distribution for $n_1 = (r-1)$ and $n_2 = (r-1)(c-1)$ degrees of freedom, respectively, which is the basis of the customary *test* of the hypothesis that the $m_{i.}$ are all equal, and the *power* of the test can be evaluated from the tables provided by P. C. Tang, and by Emma Lehmer—see references. An analogous statement can be made with respect to the column-wise population means, the $m_{.j}$.

Therefore, we can summarize the foregoing by the following theorem:

THEOREM I: *The necessary*[8] *and sufficient conditions for the strict validity of analysis-of-variance procedures for solving problems of Class I with respect to data arranged as in Table 1 are that*

$$x_{ij} = m_{..} + (m_{i.} - m_{..}) + (m_{.j} + m_{..}) + z_{ij}, \qquad (i = 1, 2, \ldots, r;\ j = 1, 2, \ldots, c) \tag{9}$$

[8] See footnote 2.

where the $m_{i.}$, $m_{.j}$, and $m_{..}$ are constants with

$$m_{..} = \sum_{i=1}^{r} m_{i.}/r = \sum_{j=1}^{c} m_{.j}/c \tag{10}$$

and the z_{ij} are normally and independently distributed about zero with a common variance σ^2.

(4.2) *Model II: Parameters are Components of Variance.* The preceding discussion of the application of analysis of variance as a method of drawing statistical inferences about the parameters involved in the mathematical model of an experiment leading to numbers arranged as in Table 1 has been concerned entirely with the problems of Class I, where the parameters are *means* and the object of the analysis is to estimate these means or to infer whether certain differences among them are or are not zero. We shall now consider the application of analysis of variance as a method of statistical inference with respect to *components of variance* involved in the mathematical model of an experiment leading to numbers arranged as in Table 1.

For the sake of concreteness, let us suppose for the moment that r animals are drawn at random from the available (large) stock of a given species and that some characteristic of each, say its body temperature, is measured on each of c days randomly located throughout some period of time. Such measurements could be arranged as in Table 1. Furthermore, let us suppose that our ultimate objective is to determine very precisely *the* body temperature characteristic of this species. By *the* body temperature characteristic of this species we mean that value about which the body temperatures of individual animals from the species will vary as a result of *biological variation,* this variability being accentuated, possibly, by day-to-day vicissitudes in the case of each animal. Under these circumstances it will clearly be of interest (a) to ascertain whether there is a component of variation assignable to day-to-day changes in the body temperature of a single animal, and (b) to compare its magnitude with the component of variation assignable to animal-to-animal variability within the species, in order to have a basis for deciding whether in collecting further data a few animals examined on each one of many days, or many animals examined on each one of only a few days, will lead to a more precise estimate of the mean body temperature characteristic of the species.

These questions may be answered by analysis of variance by arranging the data as in Table 1 and making the following assumptions:

Assumption A (Random Variables): The numbers x_{ij} are (observed

values of) random variables that are distributed about a common mean value $m_{..}$, $(i=1, 2, \ldots, r;\ j=1, 2, \ldots, c)$, where $m_{..}$ is some fixed constant.

Assumption B (*Additivity of Components*) : The random variables x_{ij} are sums of component random variables, thus

$$(11)\quad x_{ij} = m_{..} + (m_{i.} - m_{..}) + (m_{.j} - m_{..}) + z_{ij}, \qquad (i=1, 2, \ldots, r;\ j=1, 2, \ldots, c)$$

where the $(m_{i.} - m_{..})$, the $(m_{.j} - m_{..})$, and the z_{ij} are random variables.[9]

It should be noted that *Assumption A* in conjunction with *Assumption B* implies that the mean values of the $(m_{i.} - m_{..})$, of the $(m_{.j} - m_{..})$ and of the z_{ij}, are all zero.

Assumption C (*Zero Correlations and Homogeneous Variances*) : The random variables $(m_{i.} - m_{..})$, $(m_{.j} - m_{..})$, and z_{ij} are distributed with variances σ_r^2, σ_c^2, and σ^2, respectively, and all covariances among them are zero.

By following a line of reasoning similar to that presented in detail in the preceding section for the case of Model I, it is clear that here, in the case of Model II, the principal function of *Assumption A* is to bring the problem within the province of mathematical statistics; of *Assumption B*, to give specific meaning to the concept of "components of variance"; and of *Assumption C*, to dispense with interactions and render each of the "components of variance" assignable to a distinct "factor." It should be noted, however, that *independence* of the respective component deviations $(m_{i.} - m_{..}$, $m_{.j} - m_{..}$, and $z_{ij})$ of an x_{ij} from the general population mean $(m_{..})$ is not assumed—it is merely assumed that all covariances among them are zero, i.e., that they are *mutually* uncorrelated.

Collectively, *Assumptions A, B, and C* imply that

(12)

$$\sigma_{x_{ij}}^2 \equiv \text{variance of a single observation} \equiv \mathcal{E}\,(x_{ij} - m_{..})^2 = \sigma^2 + \sigma_r^2 + \sigma_c^2$$

$$\sigma_{x_{i.}}^2 \equiv \text{variance of a row-wise mean} \equiv \mathcal{E}\,(x_{i.} - m_{..})^2 = \sigma_r^2 + \frac{\sigma^2}{c}$$

$$\sigma_{x_{.j}}^2 \equiv \text{variance of a column-wise mean} \equiv \mathcal{E}\,(x_{.j} - m_{..})^2 = \sigma_c^2 + \frac{\sigma^2}{r}$$

[9] In the example considered above, $(m_{i.} - m_{..})$ represents the deviation of the long-run mean body temperature of the i^{th} animal from the long-run mean body temperature of the species; similarly, $(m_{.j} - m_{..})$ is an adjustment for the j^{th} day, assumed applicable to the body temperature of any animal from the species on that day. The z_{ij} are "catch-alls" and represent errors of measurement, etc.

$$\sigma_{x_{..}}^2 \equiv \text{variance of the general mean} \equiv \mathcal{E}\,(x_{..} - m_{..})^2 = \frac{\sigma_r^2}{r} + \frac{\sigma_c^2}{c} + \frac{\sigma^2}{rc}$$

Whence the expected values of the several mean squares of the customary analysis-of-variance table are as shown in the last column of Table 4. In brief, when *Assumptions A, B, and C* are satisfied, the

TABLE 4

ANALYSIS OF VARIANCE

(Additive Case, Row- and Column-Factors *Random*)

Variation	Degree of Freedom	Sum of Squares	Mean Square	Expected Mean Square
Between Row (Animal) Means	$r-1$	$S(X_{i.}-X_{..})^2$	$S(X_{i.}-X_{..})^2/(r-1)$	$\sigma^2 + c\sigma_r^2$
Between Column (Day) Means	$c-1$	$S(X_{.j}-X_{..})^2$	$S(X_{.j}-X_{..})^2/(c-1)$	$\sigma^2 + r\sigma_c^2$
Residual	$(r-1)(c-1)$	$S(X_{ij}-X_{i.}-X_{.j}+X_{..})^2$	$S(X_{ij}-X_{i.}-X_{.j}+X_{..})^2/(r-1)(c-1)$	σ^2
Total	$rc-1$	$S(X_{ij}-X_{..})^2$	$S(X_{ij}-X_{..})^2/(rc-1)$	$\sigma^2 + \frac{c(r-1)}{rc-1}\sigma_r^2 + \frac{r(c-1)}{rc-1}\sigma_c^2$

residual mean square is an unbiased estimate of the "residual" variance, σ^2; subtracting the residual mean square from the between-row-means mean square and dividing this difference by c, the number of columns, yields an unbiased estimate of the "row-factor" component of variance, σ_r^2. By a similar procedure an unbiased estimate of "column-factor" component of variance, σ_c^2, can be obtained. It may be noted in passing that *the "naive" estimate of the over-all variance of a single observation, furnished by the "total" mean square, is a biased estimate,* and becomes unbiased only asymptotically as both r and c increase indefinitely.

In summary, when *Assumptions A, B, and C,* or their analogs in more complex cases, are satisfied, the customary analysis-of-variance procedures yield unbiased estimates of the respective variance components. Details of the procedures appropriate to situations differing in various ways from the situation considered here will be found in the papers by S. Lee Crump, and by H. E. Daniels cited in the list of references; and in the additional references that they cite.

Whereas *Assumptions A, B, and C,* or their analogs in more complex cases, are necessary[10] and sufficient for the validity of analysis-of-

[10] See footnote 2.

variance procedures for *unbiased estimation* of *components of variance*, it is not possible to conduct exact tests of significance with respect to these components of variance, nor to derive exact confidence limits for them or their ratios, unless the joint distribution of the several *deviations* in relations (11) is specified. Therefore, we shall make

Assumption D: The deviations $(m_{i.} - m_{..})$, $(m_{.j} - m_{..})$, and z_{ij}, $(i = 1, 2, \ldots, r;\ j = 1, 2, \ldots, c)$, are all normally distributed.

When *Assumption D* is satisfied, *Assumptions A* and *B* are partially redundant, and serve principally to define the "compositions" of the random variables x_{ij}, $(i = 1, 2, \ldots r,\ j = 1, 2, \ldots, c)$. Furthermore, zero covariances, as postulated in *Assumption C*, taken in conjunction with normality, postulated in *Assumption D*, imply mutual independence of the deviations $(m_{i.} - m_{..})$, $(m_{.j} - m_{..})$, and z_{ij}; and thence of the x_{ij} with respect to each other. So, once again, independence gets in by the back door.

When *Assumptions A–D* are all satisfied, then all of the standard analysis-of-variance procedures for estimating, and testing to determine whether to infer the existence of, *components of variance* are strictly valid. These are based on that fact that these assumptions are sufficient to insure that

(a) The quotient (Between-row-means *sum* of squares)$/(\sigma^2 + c\sigma_r^2)$ will be distributed as χ^2 for $(r-1)$ degrees of freedom,

(b) The quotient (Between-column-means *sum of squares*)$/(\sigma^2 + r\sigma_c^2)$ will be distributed as χ^2 for $(c-1)$ degrees of freedom,

(c) The quotient (Residual *sum of squares*)$/\sigma^2$ will be distributed as χ^2 for $(r-1)(c-1)$ degrees of freedom,

(d) The "quotients" referred to in (a), (b), and (c) will be independent in the probability sense, so that

(e) The quantity

$$\left[\frac{\text{(Between-row-means } \textit{mean square)}}{\sigma^2 + c\sigma_r^2}\right] \Big/ \left[\frac{\text{Residual } \textit{mean square}}{\sigma^2}\right]$$

will be distributed in Snedecor's F-distribution for $n_1 = (r-1)$ and $n_2 = (r-1)(c-1)$ degrees of freedom, and

(f) The quantity

$$\left[\frac{\text{Between-column-means } \textit{mean square}}{\sigma^2 + r\sigma_c^2}\right] \Big/ \left[\frac{\text{Residual } \textit{mean square}}{\sigma^2}\right]$$

will be distributed according F for $n_1 = (c-1)$ and $n_2 = (r-1)(c-1)$ degrees of freedom.

Thus (e), which obtains also in the case of Model I when *Assumptions 1–4* are satisfied, is the basis of exact tests of hypotheses regarding the value of σ^2, and of the derivation of exact confidence limits for the value of σ^2. Similarly (e) is the basis of exact tests of hypotheses regarding the value of σ_r^2/σ^2, e.g., that $\sigma_r^2 = 0$, and of the derivation of exact confidence limits for σ_r^2/σ^2. An analogous statement holds for (f) in relation to σ_c^2/σ^2.[11]

Unfortunately, aside from testing the hypothesis that $\sigma_r^2 = 0$ or that $\sigma_c^2 = 0$, it is not possible to conduct exact tests of hypotheses regarding the absolute values of σ_r^2 and σ_c^2, nor is it possible to derive exact confidence limits for their absolute values.

5. *Which Model—Model I or Model II?* In practical work a question that often arises is: which model is appropriate in the present instance—Model I or Model II? Basically, of course, the answer is clear as soon as a decision is reached on whether the parameters of interest specify *fixed relations*, or *components of random variation*. The answer depends in part, however, upon how the observations were obtained; on the extent to which the experimental procedure employed sampled the respective variables at random. This generally provides the clue. For instance, when an experimenter selects two or more treatments, or two or more varieties, for testing, he rarely, if ever, draws them at random from a population of possible treatments or varieties; he selects those that he believes are most promising. Accordingly Model I is generally appropriate where treatment, or variety comparisons are involved. On the other hand, when an experimenter selects a sample of animals from a herd or a species, for a study of the effects of various treatments, he can insure that they are a random sample from the herd, by introducing randomization into the sampling procedure, for example, by using a table of random numbers. But he may consider such a sample to be a random sample from the species, only by making the assumption that the herd itself is a random sample from the species. In such a case, if several herds (from the same species) are involved, Model II would clearly be appropriate with respect to the variation among the animals from each of the respective herds, and might be appropriate with respect to the variation of the herds from one another.

[11] For detailed considerations of various aspects of planning and interpreting experiments for comparing standard deviations and components of variance, the reader is referred to the report by A. H. J. Baines and to Chapter 8 of the forthcoming book by the Statistical Research Group, Columbia University, which are cited in the list of references at the end of this paper.

The most difficult decisions are usually associated with *places* and *times:* Are the *fields* on which the tests were conducted a random sample of the county, or of the state, etc.? Are the *years* in which the tests were conducted a random sample of years?

When a particular experiment is being planned, or when the results are in and are being interpreted, the following parallel sets of questions serve to focus attention on the pertinent issues, and have been found helpful in answering the basic question of random versus fixed effects:

(1) Are the conclusions to be confined to the things actually studied (the animals, or the plots); to the immediate sources of these things (the herds, or the fields); or expanded to apply to more general populations (the species, or the farmland of the state)?

(2) In complete repetitions of the experiment would the same things be studied again (the same animals, or the same plots); would new samples be drawn from the identical sources (new samples of animals from the same herds, or new experimental arrangements on the same fields); or would new samples be drawn from the more general populations (new samples of animals from new herds, or new experimental arrangements on new fields)?

It is hoped that these queries will not only aid in reducing the reader's "headaches," but will lead him to the correct decisions.

Finally, it needs to be said—as the reader will no doubt discover for himself, when he considers some specific sets of data or some proposed experiments in the light of the above queries—that real-life investigations rarely fall entirely within the domain of Model I, or entirely within the domain of Model II, unless they are planned and conducted so as to achieve one or the other of these objectives, and then they may not be realistic. In consequence, some of the mean squares of the analysis-of-variance tables may be unbiased estimators of linear combinations of variance components; and others, of linear combinations of variance components *and* "mean squares" of *fixed deviations.* H. E. Daniels, in the paper cited in the list of references, has proposed a method of interpreting analysis-of-variance tables of this sort. His method consists essentially of looking at such an analysis-of-variance table through Model-II spectacles, and interpreting the "mean squares" of *fixed deviations* as variance components also. While this approach may be fruitful in situations of the type to which he has applied his method, it cannot be regarded as a general solution since, the objectives of problems of Class I and problems of Class II

are in general quite distinct. More general methods need to be devised for interpreting "mixed" analysis-of-variance tables, particularly in regard to tests of significance for individual factors.

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SOME CONSEQUENCES WHEN THE ASSUMPTIONS FOR THE ANALYSIS OF VARIANCE ARE NOT SATISFIED

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1. *Purposes of the Analysis of Variance.* The main purposes are:

(i) To estimate certain treatment differences that are of interest. In this statement both the words "treatment" and "difference" are used in rather a loose sense: e.g., a treatment difference might be the difference between the mean yields of two varieties in a plant-breeding trial, or the relative toxicity of an unknown to a standard poison in a dosage-mortality experiment. We want such estimates to be *efficient.* That is, speaking roughly, we want the difference between the estimate and the true value to have as small a variance as can be attained from the data that are being analyzed.

(ii) To obtain some idea of the accuracy of our estimates, e.g., by attaching to them estimated standard errors, fiducial or confidence limits, etc. Such standard errors, etc., should be reasonably free from bias. The usual property of the analysis of variance, when all assumptions are fulfilled, is that estimated variances are unbiased.

(iii) To perform tests of significance. The most common are the F-test of the null hypothesis that a group of means all have the same true value, and the t-test of the null hypothesis that a treatment difference is zero or has some known value. We should like such tests to be *valid,* in the sense that if the table shows a significance probability of, say, 0.023, the chance of getting the observed result or a more discordant one on the null hypothesis should really be 0.023 or something near it. Further, such tests should be *sensitive* or *powerful,* meaning that they should detect the presence of real treatment differences as often as possible.

The object of this paper is to describe what happens to these desirable properties of the analysis of variance when the assumptions required for the technique do not hold. Obviously, any practical value of the paper will be increased if advice can also be given on how to detect failure of the assumptions and how to avoid the more serious consequences.

2. *Assumptions Required for the Analysis of Variance.* In setting up an analysis of variance, we generally recognize three types of effect:

(a) treatment effects—the effects of procedures deliberately introduced by the experimenter
(b) environmental effects (the term is not ideal)—these are certain features of the environment which the analysis enables us to measure. Common examples are the effects of replications in a randomized blocks experiment, or of rows and columns in a Latin square
(c) experimental errors—this term includes all elements of variation that are not taken account of in (a) or (b).

The assumptions required in the analysis of variance for the properties listed as desirable in section 1 are as follows:

(1) The treatment effects and the environmental effects must be additive. For instance, in a randomized blocks trial the observation y_{ij} on the i^{th} treatment in the j^{th} replication is specified as

$$y_{ij} = \mu + \tau_i + \rho_j + e_{ij}$$

where μ is the general mean, τ_i is the effect of the i^{th} treatment, ρ_j is the effect of the j^{th} replication and e_{ij} is the experimental error of that observation. We may assume, without loss of generality, that the e's all have zero means.
(2) The experimental errors must all be independent. That is, the probability that the error of any observation has a particular value must not depend on the values of the errors for other observations.
(3) The experimental errors must have a common variance.[1]
(4) The experimental errors should be normally distributed.

We propose to consider each assumption and to discuss the consequences when the assumption is not satisfied. The discussion will be in rather general terms, for much more research would be needed in order to make precise statements. Moreover, in practice several assumptions may fail to hold simultaneously. For example, in non-normal distributions there is usually a correlation between the variance of an observation and its mean, so that failure of condition (4) is likely to be accompanied by failure of (3) also.

3. *Previous Work on the Effects of Non-normality.* Most of the published work on the effects of failures in the assumptions has been

[1] This statement, though it applies to the simplest analyses, is an oversimplification. More generally, the analysis of variance should be divisible into parts within each of which the errors have common variance. For instance, in the split-plot design, we specify one error variance for whole-plot comparisons and a different one for sub-plot comparisons.

concerned with this item. Writing in 1938, Hey (8) gives a bibliography of 36 papers, most of which deal with non-normality, while several theoretical investigations were outside the scope of his bibliography. Although space does not permit a detailed survey of this literature, some comments on the nature of the work are relevant.

The work is almost entirely confined to a single aspect, namely the effect on what we have called the validity of tests of significance. Further, insofar as the t-test is discussed, this is either the test of a single mean or of the difference between the means of two groups. As will be seen later, it is important to bear this restriction in mind when evaluating the scope of the results.

Some writers, e.g., Bartlett (1), investigated by mathematical methods the theoretical frequency distribution of F or t, assuming the null hypothesis true, when sampling from an infinite population that was non-normal. As a rule, it is extremely difficult to obtain the distributions in such cases. Others, e.g., E. S. Pearson (9), drew mechanically 500 or 1000 numerical samples from an infinite non-normal population, calculated the value of F or t for each sample, and thus obtained empirically some idea of their frequency distributions. Where this method was used, the number of samples was seldom large enough to allow more than a chi-square goodness of fit test of the difference between the observed and the standard distributions. A very large number of samples is needed to determine the 5 percent point, and more so the 1 percent point, accurately. A third method, of which Hey's paper contains several examples, is to take actual data from experiments and generate the F or t distribution by means of randomization similar to that which would be practiced in an experiment. The data are chosen, of course, because they represent some type of departure from normality.

The consensus from these investigations is that no serious error is introduced by non-normality in the significance levels of the F-test or of the two-tailed t-test. While it is difficult to generalize about the range of populations that were investigated, this appears to cover most cases encountered in practice. If a guess may be made about the limits of error, the true probability corresponding to the tabular 5 percent significance level may lie between 4 and 7 percent. For the 1 percent level, the limits might be taken as $\frac{1}{2}$ percent and 2 percent. As a rule, the tabular probability is an underestimate: that is, by using the ordinary F and t tables we tend to err in the direction of announcing too many significant results.

The one-tailed t-test is more vulnerable. With a markedly skew distribution of errors, where one tail is much longer than the other, the usual practice of calculating the significance probability as one-half the value read from the tables may give quite a serious over- or underestimate.

It was pointed out that work on the validity of the t-test covered only the cases of a single mean or of the comparison of the means of two groups. The results would be applicable to a randomized blocks experiment if we adopted the practice of calculating a separate error for each pair of treatments to be tested, using only the data from that pair of treatments. In practice, however, it is usual to employ a pooled error for all t-tests in an analysis, since this procedure not only saves labor but provides more degrees of freedom for the estimation of error. It will be shown in section 6 that this use of a pooled error when non-normality is present may lead to large errors in the significance probabilities of individual t-tests. The same remark applies to the Latin square and more complex arrangements, where in general it is impossible to isolate a separate error appropriate to a given pair of treatments, so that pooling of errors is unavoidable.

4. *Further Effects of Non-Normality.* In addition to its effects on the validity of tests of significance, non-normality is likely to be accompanied by a loss of efficiency in the estimation of treatment effects and a corresponding loss of power in the F- and t-tests. This loss of efficiency has been calculated by theoretical methods for a number of types of non-normal distribution. While these investigations dealt with the estimation of a single mean, and thus would be strictly applicable only to a paired experiment analyzed by the method of differences, the results are probably indicative of those that would be found for more complex analyses. In an attempt to use these results for our present purpose, the missing link is that we do not know which of the theoretical non-normal distributions that have been studied are typical of the error distributions that turn up in practice. This gap makes speculation hazardous, because the efficiency of analysis of variance methods has been found to vary from 100 percent to zero. While I would not wish to express any opinion very forcibly, my impression is that in practice the loss of efficiency is not often great. For instance, in an examination of the Pearson curves, Fisher (4) has proved that for curves that exhibit only a moderate departure from normality, the efficiency remains reasonably high. Further, an analysis of the logs of the observations instead of the observations themselves has fre-

quently been found successful in converting data to a scale where errors are approximately normally distributed. In this connection, Finney, (3) has shown that if log x is exactly normally distributed, the arithmetic mean of x has an efficiency greater than 93 percent so long as the coefficient of variation of x is less than 100 percent. In most lines of work a standard error as high as 100 percent per observation is rare, though not impossible.

The effect of non-normality on estimated standard errors is analogous to the effect on the t-test. If a standard error is calculated specifically for each pair of treatments whose means are to be compared, the error variance is unbiased. Bias may arise, however, by the application of a pooled error to a particular pair of treatments.

We now consider how to detect non-normality. It might perhaps be suggested that the standard tests for departure from normality, Fisher (5), should be applied to the errors in an analysis. This suggestion is not fruitful, however, because for experiments of the size usually conducted, the tests would detect only very violent skewness or kurtosis. Moreover, as is perhaps more important, it is not enough to detect non-normality: in order to develop an improved analysis, one must have some idea of the actual form of the distribution of errors, and for this purpose a single experiment is rarely adequate.

Examination of the distribution of errors may be helpful where an extensive uniformity trial has been carried out, or where a whole series of experiments on similar material is available. Theoretically, the best procedure would be to try to find the form of the frequency distribution of errors, using, of course, any *a priori* knowledge of the nature of the data. An improved method of estimation could then be developed by maximum likelihood. This, however, would be likely to lead to involved computations. For that reason, the usual technique in practice is to seek, from *a priori* knowledge or by trial and error, a transformation that will put the data on a scale where the errors are approximately normal. The hope is that in the transformed scale the usual analysis will be reasonably efficient. Further, we would be prepared to accept some loss in efficiency for the convenience of using a familiar method. Since a detailed account of transformations will be given by Dr. Bartlett in the following paper, this point will not be elaborated.

The above remarks are intended to apply to the handling of a rather extensive body of data. With a single experiment, standing by itself, experience has indicated two features that should be watched for:

(i) evidence of charges in the variance from one part of the experiment to another. This case will be discussed in section 6.
(ii) evidence of gross errors.

5. *Effects of Gross Errors.* The effects of gross errors, if undetected, are obvious. The means of the treatments that are affected will be poorly estimated, while if a pooled error is used the standard errors of other treatment means will be over-estimated. An extreme example is illustrated by the data in Table I, which come from a randomized blocks experiment with four replicates.

TABLE I

WHEAT: RATIO OF DRY TO WET GRAIN

Block	Nitrogen applied			
	None	Early	Middle	Late
1	.718	.732	.734	.792
2	.725	.781	.725	.716
3	.704	1.035	.763	.758
4	.726	.765	.738	.781

As is likely to happen when the experimenter does not scrutinize his own data, the gross error was at first unnoticed when the computor carried out the analysis of variance, though the value is clearly impossible from the nature of the measurements. This fact justifies rejection of the value and substitution of another by the method of missing plots, Yates (11).

Where no explanation can be found for an anomalous observation, the case for rejection is more doubtful. Habitual rejection of outlying values leads to a marked underestimation of errors. An approximate test of significance of the contribution of the suspected observation to the error helps to guard against this bias. First calculate the error sum of squares from the actual observations. Then calculate the error when the suspected value is replaced by the missing-plot estimate: this will have one less degree of freedom and is designated the "Remainder" in the data below. The difference represents the sum of squares due to the suspect. For the data above, the results are

	d.f.	S.S.	M.S.
Actual error	9	.04729	.00525
Suspect	1	.04205	.04205
Remainder	8	.00524	.000655

Alternatively, the contribution due to the suspected observation may be calculated directly and the remainder found by subtraction. If there are t treatments and r replicates, the sum of squares is $(t-1)(r-1)d^2/tr$, where d is the difference between the suspected observation and the value given by the missing-plot formula. In the present case t and r are 3 and the missing-plot value is 0.7616, so that the contribution is $9(0.2734)^2/16$, or 0.04205.[2]

The F ratio for the test of the suspect against the remainder is 64.2, giving a t value of 8.01, with 8 degrees of freedom. Now, assuming that the suspect had been examined simply because it appeared anomalous, with no explanation for the anomaly, account must be taken of this fact in the test of significance. What is wanted is a test appropriate to the *largest* contribution of any observation. Such a test has not as yet been developed. The following is suggested as a rough approximation. Calculate the significance probability, p, by the ordinary t table. Then use as the correct significance probability np, where n is the number of degrees of freedom in the actual error.[3] In the present case, with $t = 8.01$, p is much less than 1 in a million, and consequently np is less than 1 in 100,000. In general, it would be wise to insist on a rather low significance probability (e.g., 1 in 100) before rejecting the suspect, though a careful answer on this point requires knowledge of the particular types of error to which the experimentation is subject.

6. *Effects of Heterogeneity of Errors.* If ordinary analysis of variance methods are used when the true error variance differs from one observation to another, there will as a rule be a loss of efficiency in the estimates of treatment effects. Similarly, there will be a loss of sensitivity in tests of significance. If the changes in the error variance are large, these losses may be substantial. The validity of the F-test for all treatments is probably the least affected. Since, however, some treatment comparisons may have much smaller errors than others, t-tests from a pooled error may give a serious distortion of the significance levels. In the same way the standard errors of particular treatment comparisons, if derived from a pooled error, may be far from the true values.

[2] This formula applies only to randomized blocks. Corresponding formulas can be found for other types of arrangements. For instance, the formula for a $p \times p$ Latin square is $(p-1)(p-2)d^2/p^2$.

[3] The approximation is intended only to distinguish quickly whether the probability is low or high and must not be regarded as accurate. For a discussion of this type of test in a somewhat simpler case, see E. S. Pearson and C. Chandra Sekar, *Biometrika*, Vol. 28 (1936), pp. 308–320.

There is no theoretical difficulty in extending the analysis of variance so as to take account of variations in error variances. The usual analysis is replaced by a weighted analysis in which each observation is weighted in proportion to the inverse of its error variance. The extension postulates, however, a knowledge of the relative variances of any two observations and this knowledge is seldom available in practice. Nevertheless, the more exact theory can sometimes be used with profit in cases where we have good estimates of these relative variances. Suppose for instance, the situation were such that the observations could be divided into three parts, the error variances being constant within each part. If unbiased estimates of the variances within each part could be obtained and if these were each based on, say, at least 15 degrees of freedom, we could recover most of the loss in efficiency by weighting inversely as the observed variances. This device is therefore worth keeping in mind, though in complex analyses the weighted solution involves heavy computation.

TABLE II

MANGOLDS, PLANT NUMBERS PER PLOT

Block	Control		Chalk			Lime			Total
	0	0	1	2	3	1	2	3	
I	140	49	98	135	117	81	147	130	897
II	142	37	132	151	137	129	131	112	971
III	36	114	130	143	137	135	103	130	928
IV	129	125	153	146	143	104	147	121	1068
Total	447	325	513	575	534	449	528	493	3864
Range	106	88	55	16	26	54	44	18	

Heterogeneity of errors may arise in several ways. It may be produced by mishaps or damage to some part of the experiment. It may be present in one or two replications through the use of less homogeneous material or of less carefully controlled conditions. The nature of the treatments may be such that some give more variable responses than others. An example of this type is given by the data in Table II.

The experiment investigated the effects of three levels of chalk dressing and three of lime dressing on plant numbers of mangolds. There were four randomized blocks of eight plots each, the control plots being replicated twice within each block.[4]

Since the soil was acid, high variability might be anticipated for the

[4] The same data were discussed (in much less detail) in a previous paper, Cochran (2).

control plots as a result of partial failures on some plots. The effect is evident on eye inspection of the data. To a smaller extent the same effect is indicated on the plots receiving the single dressing of chalk or lime. If the variance may be regarded as constant within each treatment, there will be no loss of efficiency in the treatment means in this case, contrary to the usual effect of heterogeneity. Any t tests will be affected and standard errors may be biased. In amending the analysis so as to avoid such disturbances, the first step is to attempt to subdivide the error into homogeneous components. The simple analysis of variance is shown below.

TABLE III

ANALYSIS OF VARIANCE FOR MANGOLDS DATA

	d.f.	S.S.	M.S.
Blocks	3	2,079	.....
Treatments	6	8,516	.....
Error	22	18,939	860.9
Total	31	29,534	.....

For subdivision of the error we need the following auxiliary data.

Block	Diff. between Controls	Total − 4 (Controls)	(C1–L1)	(C2 + L2 + C3 + L3) − 2(C1 + L1)
1	91	141	17	171
2	105	255	3	9
3	78	328	−5	−17
4	4	52	49	43
Total	.....	776	64	206
Divisor for S.S.	2	24	2	12

The first two columns are used to separate the contribution of the controls to the error. This has 7 d.f. of which 4 represent differences between the two controls in each block. The sum of squares of the first column is divided by 2 as indicated. There remain 3 d.f. which come from a comparison within each block of the total yield of the controls with the total yield of the dressings. Since there are 6 dressed plots to 2 controls per block we take

(Dressing total) − 3(Control total) = (Total) − 4(Control total)

Thus 141 = 897 − 4(140 + 49).

By the usual rule the divisor for the sum of squares of deviations is 24.

Two more columns are used to separate the contribution of the single dressings. There are 6 d.f. of which 3 compare chalk with line at this level while the remaining 3 compare the single level with the higher levels. The resulting partition of the error sum of squares is shown below.

TABLE IV

PARTITION OF ERROR SUM OF SQUARES

	d.f.	S.S.	M.S.
Total	22	18,939	861
Between controls	4	12,703	3,176
Controls v. Dressings	3	1,860	620
Chalk 1 v. Lime 1	3	850	283
Single v. Higher Dressings	3	1,738	579
Double and Triple Dressings	9	1,788	199

As an illustration of the disturbance to t-tests and to estimated standard errors, we may note that the pooled mean square, 861, is over four times as large as the 9 d.f. error, 199, obtained from the double and triple dressings. Consequently, the significance levels of t and standard errors would be inflated by a factor of two if the pooled error were applied to comparisons within the higher dressings.

In a more realistic approach we might postulate three error variances, σ_c^2 for controls, σ_1^2 for single dressings and σ_h^2 for higher dressings. For these we have unbiased estimates of 3,176, 283 and 199 respectively from Table IV. The mean square for Controls v. Dressings (620) would be an unbiased estimate of $(9\sigma_c^2 + \sigma_1^2 + 2\sigma_h^2)/12$, while that for Single v. Higher Dressings (579) would estimate $(2\sigma_1^2 + \sigma_h^2)/3$.

What one does in handling comparisons that involve different levels depends on the amount of refinement that is desired and the amount of work that seems justifiable. The simplest process is to calculate a separate t-test or standard error for any comparison by obtaining the comparison separately within each block. Such errors, being based on 3 d.f., would be rather poorly determined. A more complex but more efficient approach is to estimate the three variances from the five mean squares given above. Since the error variance of any comparison will be some linear function of these three variances, it can then be estimated.

To summarize, heterogeneity of errors may affect certain treatments or certain parts of the data to an unpredictable extent. Sometimes, as in the previous example, such heterogeneity would be expected in ad-

vance from the nature of the experiment. In such cases the data may be inspected carefully to decide whether the actual amount of variation in the error variance seems enough to justify special methods. In fact, such inspection is worthwhile as a routine procedure and is, of course, the only method for detecting heterogeneity when it has not been anticipated. The principal weapons for dealing with this irregular type of heterogeneity are subdivision of the error variance or omission of parts of the experiment. Unfortunately, in complex analyses the computations may be laborious. For the Latin square, Yates (12) has given methods for omitting a single treatment, row or column, while Yates and Hale (14) have extended the process to a pair of treatments, rows or columns.

In addition, there is a common type of heterogeneity that is more regular. In this type, which usually arises from non-normality in the distribution of errors, the variance of an observation is some simple function of its mean value, irrespective of the treatment or block concerned. For instance, in counts whose error distribution is related to the Poisson, the variance of an observation may be proportional to its mean value. Such cases, which have been most successfully handled by means of transformations, are discussed in more detail in Dr. Bartlett's paper.

7. *Effects of Correlations Amongst the Errors.* These effects may be illustrated by a simple theoretical example. Suppose that the errors $e_1, e_2, \ldots, e_r$ of the r observations on a treatment in a simple group comparison have constant variance σ^2 and that every pair has a correlation coefficient ρ. The error of the treatment total, $(e_1 + e_2 \ldots, + e_r)$ will have a variance

$$r\sigma^2 + r(r-1)\rho\sigma^2$$

since there are $r(r-1)/2$ cross-product terms, each of which will contribute $2\rho\sigma^2$. Hence the *true* variance of the treatment mean is

$$\sigma^2\{1 + (r-1)\rho\}/r.$$

Now in practice we would estimate this variance by means of the sum of squares of deviations within the group, divided by $r(r-1)$. But

$$\text{Mean } \Sigma(e_i - \bar{e})^2 = \text{Mean } \Sigma\, e_i^2 - r\,\{\text{Mean } \bar{e}^2\}$$
$$= r\sigma^2 - \sigma^2\{1 + (r-1)\rho\} = (r-1)\sigma^2(1-\rho).$$

Hence the *estimated* variance of the treatment mean is $\sigma^2(1-\rho)/r$.

Consequently, if ρ is positive the treatment mean is less accurate

than the mean of an independent series, but is estimated to be more accurate. If ρ is negative, these conditions are reversed. Substantial biases in standard errors might result, with similar impairment of t-tests. Moreover, in many types of data, particularly field experimentation, the observations *are* mutually correlated, though in a more intricate pattern.

Whatever the nature of the correlation system, this difficulty is largely taken care of by proper randomization. While mathematical details will not be given, the effect of randomization is, roughly speaking, that we may treat the errors as if they were independent. The reader may refer to a paper by Yates (13), which presents the nature of this argument, and to papers by Bartlett (1), Fisher (6) and Hey (8), which illustrate how randomization generates a close approximation to the F and t distributions.

Occasionally it may be discovered that the data have been subject to some systematic pattern of environmental variation that the randomization has been unable to cope with. If the environmental pattern obviously masks the treatment effects, resort may be had to what might be called desperate remedies in order to salvage some information.

The data in Table V provide an instance. The experiment was a 2^4 factorial, testing the effects of lime (L), fish manure (F) and artificial fertilizers (A). Lime was applied in the first year only; the other dressings were either applied in the first year only (1) or at a half rate every year (2). Two randomized blocks were laid out, the crop being pyrethrum, which forms an ingredient in many common insecti-

TABLE V

WEIGHTS OF DRY HEADS PER PLOT
(Unit, 10 grams)

Block 1				Block 2			
LA1	LF2	F2	L1	A1	L1	A2	0
84	66	70	81	63	97	56	64
1	1	1	1	1	1	1	1
LF1	A2	A1	FA2	F1	LA2	LA1	LFA1
148	137	146	171	168	158	189	152
0	0	0	0	0	0	0	0
LFA2	F1	LFA1	LA2	LF1	L2	LF2	FA2
179	218	247	228	191	195	189	179
0	0	0	0	0	0	0	0
0	L2	0	FA1	FA1	LFA2	0	F2
124	166	177	153	133	145	141	130
0	0	0	0	0	0	0	0

cides. The data presented are for the fourth year of the experiment, which was conducted at the Woburn Experimental Farm, England.

The weights of dry heads are shown immediately underneath the treatment symbols. It is evident that the first row of plots is of poor fertility—treatments appearing in that row have only about half the yields that they give elsewhere. Further, there are indications that every row differs in fertility, the last row being second worst and the third row best. The fertility gradients are especially troublesome in that the four untreated controls all happen to lie in outside rows. The two replications give practically identical totals and remove none of this variation.

There is clearly little hope of obtaining information about the treatment effects unless weights are adjusted for differences in fertility from row to row. The adjustment may be made by covariance.

For simplicity, adjustments for the first row only will be shown: these remove the most serious environmental disturbance. As x variable we choose a variable that takes the value 1 for all plots in the first row and zero elsewhere. The x values are shown under the weights in Table V. The rest of the analysis follows the usual covariance technique, Snedecor (10).

TABLE VI

SUMS OF SQUARES AND PRODUCTS
(y = weights, x = dummy variates)

	d.f.	y^2	yx	x^2
Blocks	1	657	0.0	0.00
Treatments	13	33,323	−200.2	1.75
Error	17	46,486	−380.0	4.25
Total	31	80,466	−580.2	6.00

Note that there are only 14 distinct treatments, since L1 is the same as L2. The reduction in the error S.S. due to covariance is $(380.0)^2/4.25$, or 33,976. The error mean square is reduced from 2,734 to 782 by means of the covariance, i.e., to less than one-third of its original value. The regression coefficient is $-380.0/4.25$, or -89.4 units.

Treatment means are adjusted in the usual way. For L1, which was unlucky in having two plots in the first row, the unadjusted mean is 89. The mean x value is 1, whereas the mean x value for the whole experiment is 8/32, or $\frac{1}{4}$. Hence the adjustment increases the L1 mean by (3/4) (89.4), the adjusted value being 156. For L2, which had no plots in the first row, the x mean is 0, and the adjustment reduces the mean from 180 to 158. It may be observed that the unadjusted mean

of L2 was double that of L1, while the two adjusted means agree closely, as is reasonable since the two treatments are in fact identical.

If it were desired to adjust separately for every row, a multiple covariance with four x variables could be computed. Each x would take the value 1 for all plots in the corresponding row and 0 elsewhere. It will be realized that the covariance technique, if misused, can lead to an underestimation of errors. It is, however, worth keeping in mind as an occasional weapon for difficult cases.

8. *Effects of Non-Additivity.* Suppose that in a randomized blocks experiment, with two treatments and two replicates, the treatment and block effects are multiplicative rather than additive. That is, in either replicate, treatment B exceeds treatment A by a fixed percentage, while for either treatment, replicate 2 exceeds replicate 1 by a fixed percentage. Consider treatment percentages of 20% and 100% and replicate percentages of 10% and 50%. These together provide four combinations. Taking the observation for treatment A in replicate 1 as 1.0, the other observations are shown in Table VII.

TABLE VII

HYPOTHETICAL DATA FOR FOUR CASES WHERE EFFECTS ARE MULTIPLICATIVE

Rep.	T 20% R 10%		T 20% R 50%		T 100% R 10%		T 100% R 50%	
	A	B	A	B	A	B	A	B
1	1.0	1.2	1.0	1.2	1.0	2.0	1.0	2.0
2	1.1	1.32	1.5	1.8	1.1	2.2	1.5	3.0
d	.02		.10		.10		.50	
σ_{na}	.01		.05		.05		.25	

Thus, in the first case, 1.32 for B in replicate 2 is 1.2 times 1.1. Since no experimental error has been added, the error variance in a correct analysis should be zero. If the usual analysis of variance is applied to each little table, the calculated error in each case will have 1 d.f. If d is the sum of two corners minus the other two corners, the error S.S. is $d^2/4$, so that the standard error σ_{na} is $d/2$ (taken as positive). The values of d and of σ_{na} are shown below each table.

Consequently, in the first experiment, say, the usual analysis would lead to the statement that the average increase to B is 0.21 units $\pm$ 0.01, instead of to the correct statement that the increase to B is 20%. The standard error, although due entirely to the failure of the additive rela-

tionship, does perform a useful purpose. It warns us that the actual increase to B over A will vary from replication to replication and measures how much it will vary, so far as the experiment is capable of supplying information on this point. An experimenter who fails to see the correct method of analysis and uses ordinary methods will get less precise information from the experiment for predictive purposes, but if he notes the standard error he will not be misled into thinking that his information is more precise than it really is.

When experimental errors are present, the variance σ_{na}^2 will be added to the usual error variance σ_e^2. The ratio $\sigma_{na}^2/(\sigma_{na}^2+\sigma_e^2)$ may appropriately be taken as a measure of the loss (fractional) of information due to non-additivity. In the four experiments, from left to right, the values of σ_{na} are respectively 0.9, 3.6, 3.2, and 13.3 percent of the mean yields of the experiments. In the first case, where treatment and replicate effects are small, the loss of information due to non-additivity will be trivial unless σ_e is very small. For example, with $\sigma_e = 5$ percent, the fractional loss is 0.81/25.81 or about 3 percent. In the two middle examples, where either the treatment or the replicate effect is substantial, the losses are beginning to be substantial. With $\sigma_e = 5$ percent in the second case, the loss would be about 30 percent. Finally, when both effects are large the loss is great.

Little study has been made in the literature of the general effects of non-additivity or of the extent to which this problem is present in the data that are usually handled by analysis of variance.[5] I believe, however, that the results from these examples are suggestive of the consequences in other cases. The principal effect is a loss of information. Unless experimental errors are low or there is a very serious departure from additivity, this loss should be negligible when treatment and replication effects do not exceed 20 percent, since within that range the additive relationship is likely to be a good approximation to most types that may arise.

Since the deviations from additivity are, as it were, amalgamated with the true error variance, the pooled error variance as calculated from the analysis of variance will take account of these deviations and should be relatively unbiased. This pooled variance may not, however, be applicable to comparisons between individual pairs of treatments. The examples above are too small to illustrate this point. But, clearly, with three treatments A, B, and C, the comparison (A–B) might be much less affected by non-additivity than the comparison (A–C).

[5] A relevant discussion of this problem for regressions in general, with some interesting results, has been given recently by Jones (7).

Thus non-additivity tends to produce heterogeneity of the error variance.[6]

If treatment or block effects, or both, are large, it will be worth examining whether treatment differences appear to be independent of the block means, or vice versa. There are, of course, limitations to what can be discovered from a single experiment. If relations seem non-additive, the next step is to seek a scale on which effects are additive. Again reference should be made to the paper following on transformations.

9. *Summary and Concluding Remarks.* The analysis of variance depends on the assumptions that the treatment and environmental effects are additive and that the experimental errors are independent in the probability sense, have equal variance and are normally distributed. Failure of any assumption will impair to some extent the standard properties on which the widespread utility of the technique depends. Since an experimenter could rarely, if ever, convince himself that all the assumptions were exactly satisfied in his data, the technique must be regarded as approximative rather than exact. From general knowledge of the nature of the data and from a careful scrutiny of the data before analysis, it is believed that cases where the standard analysis will give misleading results or produce a serious loss of information can be detected in advance.

In general, the factors that are liable to cause the most severe disturbances are extreme skewness, the presence of gross errors, anomalous behavior of certain treatments or parts of the experiment, marked departures from the additive relationship, and changes in the error variance, either related to the mean or to certain treatments or parts of the experiment. The principal methods for an improved analysis are the omission of certain observations, treatments, or replicates, subdivision of the error variance, and transformation to another scale before analysis. In some cases, as illustrated by the numerical examples, the more exact methods require considerable experience in the manipulation of the analysis of variance. Having diagnosed the trouble, the experimenter may frequently find it advisable to obtain the help of the mathematical statistician.

[6] It is an over-simplification to pretend, as in the discussion above, that the deviations from addivity act entirely like an additional component of random error. Discussion of the effects introduced by the systematic nature of the deviations would, however, unduly lengthen this paper.

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THE USE OF TRANSFORMATIONS

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1. *Theoretical Discussion.* The purpose of this note is to summarize the transformations which have been used on raw statistical data, with particular reference to analysis of variance. For any such analysis the usual purpose of the transformation is to change the scale of the measurements in order to make the analysis more valid. Thus the conditions required for assessing accuracy in the ordinary unweighted analysis of variance include the important one of a constant residual or error variance, and if the variance tends to change with the mean level of the measurements, the variance will only be stabilized by a suitable change of scale.

If the form of the change of variance with mean level is known, this determines the type of transformation to use. Suppose we write

$$\sigma_x^2 = f(m), \tag{1}$$

where σ_x^2 is the variance on the original scale of measurements x with the mean of x equal to m. Then for any function $g(x)$ we have approximately[1]

$$\sigma_g^2 = (dg/dm)^2 f(m), \tag{2}$$

so that if σ_g^2 is to be constant, C^2 say, we must have

$$g(m) = \int \frac{C\,dm}{\sqrt{f(m)}}. \tag{3}$$

For example, if the standard deviation σ_x tends to be proportional to the mean level m, we have $f(m)$ proportional to m^2, and $g(m)$ proportional to $\log m$; i.e., we should use the logarithmic scale. Appropriate scales of this kind for types of data often encountered in statistical analysis are discussed in sections 2, 3, and 4.

However, a constant variance is not the only condition we seek, and precautions are still necessary when using analysis of variance with the transformed variate. In the ideal case (cf. Reference 6 at the end of this paper),

(a) The variance of the transformed variate should be unaffected by changes in the mean level (this is taken to be the primary purpose of the transformations of sections 2, 3, and 4).

(b) The transformed variate should be normally distributed.

[1] For a more precise formulation, see Reference 15.