differing backgrounds in international affairs. Most local libraries will have the books in their collections. It is well to remember that there often is little time to study once a contract has been signed to assist with a project in a foreign country. Getting the necessary immunization shots, filling out required forms, getting business affairs in order, and similar time-consuming activities leave little time for background reading.

After becoming familiar with the technical language and literature of overseas work, it is necessary to determine the opportunities an individual may have with a particular level of education and experience. There is a wide range of experience and degree of skill desired by the various agencies which recruit development technicians. Sometimes even within the same organization this range is surprisingly great. As mentioned by Trail[3] in his excellent book: "Peace Corps volunteers probably best represent this range. The majority of volunteers recruited to work abroad are young, unskilled college graduates: however, in the same training program for the same project, one may find highly specialized college professors training for the same job."

Trail lists the following general characteristics of the typical technician:

1. His training and experience have been mainly within the U.S.
2. He knows no language other than English.
3. His knowledge of foreign cultures and environments is very slight.
4. His acquaintance with world events, U.S. history and foreign policy, and economic development is no more than the average professional within his group.
5. He has little knowledge, if any, of impact of technological change in the developing areas.
6. He is, in effect, a person with some degree of professional skill selected by the technical assistance agency to carry out some aspect of the program overseas.

Government, voluntary, business, university and religious agencies all have recruiting programs. The book by Trail includes specific names and addresses of the various agencies. The more flexible an individual can be about such things as starting date and length of service, the easier it will be for him to obtain employment.

As noted by Foster[4] in his book, "The peoples of newly developing countries recognize the world as changing rapidly; they want and need economic and technical help of many kinds. American technical experts in such fields as public health, agriculture, education, and community development are as well trained and professionally competent as any in the world."

Anyone who has worked successfully in a foreign country for any extended period of time will agree with Toynbee[5] that the rapid expansion of technical assistance activities to the newly developing countries of the world is both man's greatest challenge and contribution of the twentieth century. The tips provided in this article should help anyone truly interested in becoming part of this great adventure to make decisions based on knowledge rather than unsupported guesses.

SELECTED REFERENCES

[1]Lawrence H. Skromme, "Agricultural Engineering Abroad," Agricultural Engineering, 51, No. 4, (April, 1970), 221.

[2]Erly Diaz Brandao, "Technical Assistance and Agricultural Education," Agriculture and the University, Council on Higher Education in the American Republics, Institute of International Education, 809 United Nations Plaza, New York, N. Y. 10017. p. 185.

[3]Thomas F. Trail, Educationof Development Technicians, Frederick A. Praeger, Publishers, Inc., New York, N.Y. 1968, p. ix.

[4]George M. Foster, Traditional Cultures, Harper and Row, Publishers, Inc., New York, N.Y. 1962. p. 2.

[5]Arnold Toynbee, Interview on Meet the Press (New York: NBC, July 7, 1955).

Teaching the Analysis of Variance Concept[1]

Elmer Gray
Western Kentucky University

Biological organisms are characterized by variation. The use of statistics to analyze biological variation has become well established. The analysis of variance is one of the most widely used procedures for analyzing biological experiments.

The analysis of variance concept and procedure were developed by R. A. Fisher in the 1920's and subsequently have been employed in many different statistical analyses. Any biological measurement, such as yield, height, chemical composition, rate of growth, etc., consists of various effects. For example, the weight of a pig would be affected by the population mean weight of pigs, the particular litter from which the pig came, the diet it had received, and some unexplainable factors. The analysis of variance permits the experimenter to objectively measure the various effects of components of a measurement. The variability in measurements or observations is analyzed and allocated to the various effects. The experimenter wants to know whether the variability due to controlled factors is meaningful in terms of the variability due to uncontrolled factors. Objectivity in analyzing research data is an essential part of the scientific method.

Greater emphasis on research appreciation and training has necessitated teaching introductory statistics courses at the undergraduate level. The purpose of this treatise is to develop an explanatory method of teaching the analysis of variance concept in an introductory statistics course. The method will be illustrated for two of the simpler experimental designs – completely randomized and randomized complete block.

Completely randomized design – The completely randomized design (CRD) is the simplest experimental design. Treatments are randomly assigned to the entire experimental area; therefore, every experimental unit has an equal probability of receiving any treatment (Fig. 1). Advantages of the CRD result from its flexibility and simplicity. The number of observations may vary from treatment to treatment. The loss of information resulting from missing observations is small in comparison to losses in other designs. The number of degrees of freedom associated with experimental error is greater for the CRD than for other designs. The main disadvantage of the CRD results from the fact that all the variation among experimental units, except that due to treatments, is included in the experimental error. The CRD is suitable when the experimental units are homogeneous or when there is no systematic gradient in the variability among heterogeneous experimental units.

The model for the CRD is $X_{ij} = \mu + \Upsilon_i + \epsilon_{ij}$. This is a linear or an additive model in which any observed value, X_{ij}, is the sum of three parts: μ, an overall mean; Υ_i, a treatment effect; and ϵ_{ij}, the error or unexplained portion of the observation. The i's refer to treatments and take on values from 1 to the number of treatments (t). The j's refer to replications within

[1]Contribution from the Department of Agriculture, Western Kentucky University, Bowling Green, Kentucky 42101.

the treatment and take on values from 1 to the number of replications (r) in the treatment. Some hypothetical data and the appropriate symbols for the CRD are given in Table 1.

In the conventional analysis of variance procedure the sums of squares (SS) are calculated as follows:

Total unadjusted SS = $\Sigma(X_{ij})^2 = 3^2 + 4^2 + \ldots + 9^2 = 354$

Correction factor (CF) for the overall mean = $(\Sigma X_{ij})^2/n = (3 + 4 + \ldots + 9)^2/9 = 324$

Total adjusted SS = Total unadjusted SS – CF = 354 – 324 = 30

Among treatment SS = $\Sigma(X_{i.})^2/r - CF = (12^2 + 18^2 + 24^2)/3 - CF$

$348 - 324 = 24$

Within treatment SS = Total adjusted SS – Among treatment SS (Error SS)

$30 - 24 = 6$

Variation among the observed values (Table 1) has been partitioned and allocated to the sources. This is demonstrated graphically in Fig. 2. Importance of the relative magnitudes of the among and within SS will be considered later.

There is a relationship between the model, $X_{ij} = \mu + \Upsilon_i + \epsilon_{ij}$, and the above analysis of variance procedure. This relationship can be explored by calculating the individual components of each X_{ij} value. The components of X_{11} are calculated as follows:

$X_{11} = \mu + \Upsilon_1 + \epsilon_{11}$

$3 = 6 + (4 - 6) + \epsilon_{11}$

$3 = 6 + (-2) + \epsilon_{11}$

$3 = 4 + \epsilon_{11}$

$3 - 4 = \epsilon_{11}$

$-1 = \epsilon_{11}$

The overall mean or μ is 6, the effect of treatment 1 is a -2, and the error associated with X_{11} is -1. These values have been calculated for each X_{ij} and are presented in Fig. 3. The CF (324) from the analysis of variance is the same as the nine μ's squared and summed ($9 \times 6^2 = 324$). The among treatment SS of 24 is equal to the individual treatment effects (Υ_i's) squared and summed ($-2^2 + -2^2 + -2^2 + 0^2 + 0^2 + 0^2 + 2^2 + 2^2 + 2^2 = 24$). The within treatment or error SS of 6 is equal to the nine ϵ_{ij}'s squared and summed ($-1^2 + 0^2 + 1^2 + 1^2 + 1^2 + 0^2 + -1^2 + 0^2 + 1^2 = 6$). This method of partitioning and allocating the SS to the sources of variation illustrates the relationship between the model and the analysis of variance procedure, but is more cumbersome than the conventional method.

The next step is to compare the relative magnitudes of the among treatment and within treatment SS. The among treatment SS is the controlled variability; whereas, the within treatment SS is the uncontrolled variability or error. Are the differences among the treatment means meaningful when compared to the within treatment differences? Since it is not possible to prove anything with statistics, the procedure is to disprove something with some probability of being wrong in the conclusion. Therefore, the null hypothesis, a hypothesis of no difference, is used. The experimenter assumes that there are no differences among the treatment means, but may disprove this assumption at some level of probability.

In the present example, the null hypothesis is that there are no differences among the three treatment means. In order to determine whether this hypothesis should be accepted or rejected, the among treatment and within treatment SS or variability must be compared (Table 2). However, SS are not compared directly because the number of degrees of freedom (df) vary with the sources of variation. Degrees of freedom are the number of unrestricted sums of squares for observations. The number of degrees of freedom equals the total number of observations (n) minus the number of restrictions imposed (Fig. 4). In the CRD, calculation of the CF is one restriction; therefore, the total df is n-1. This total is divided into the among treatments (t–1) and within treatments t(r–1) (Table 2). In the present example, n = 9, total df (n–1) = 8, among treatment df(t–1) = 2, and within treatment df(t) (r–1) = 6.

The SS associated with a source of variation is divided by the df associated with that source of variation to give an average value or mean square (MS). Acceptance or rejection of the null hypothesis is based upon the relative magnitudes of appropriate mean squares. In the CRD, the among treatment MS is divided by the within treatment or error MS. Error MS is a measure of the uncontrolled variability or the variability among experimental units treated alike. In the example being considered, the observed values of 3, 4, and 5 indicate the within variability for treatment number 1. The failure of experimental units, treated alike, to respond the same reflects inherent variability in the experimental units or inconsistencies in the experimental techniques. The among treatment MS contains the error effect plus the treatment effect if the null hypothesis is not true. If the null hypothesis is true, differences among means are due to the random ϵ_{ij}'s and it is expected that the treatment mean square would be equal to the error mean square.

The F test is a ratio of two mean squares. For the CRD,

$$F = \frac{\text{Among treatment MS}}{\text{Within treatment MS}} = \frac{\text{error effect + treatment effect}}{\text{error effect}}$$

When there is no treatment effect, the F value would be expected to fluctuate around 1. As the treatment effect increases, the F value would be expected to increase. Tables are available which give the probabilities of chance occurrences of F values of different magnitudes. In the present example, calculated F was 12 (Table 2). According to the F table, a value of 12 with 2 and 6 df is expected to occur less than 1/100 due to chance; therefore, the differences among the treatments are said to be highly significant. Since there appear to be meaningful or real differences among the treatments, the null hypothesis of no difference would be rejected, and an alternative hypothesis of difference among treatments would be accepted.

The highly significant F indicates that there are differences among the three treatment means being compared, but does not indicate which means are different. The lowest mean is expected to be different from the highest mean (3 vs. 8 in Table 1), but the highly significant F does not indicate whether these means are different from the intermediate mean of 6. Each treatment mean can be compared with every other treatment mean through the appropriate use of individual degree of freedom comparisons or through the use of mean separation techniques.

Randomized complete block design – The randomized complete block design (RCB) differs from the CRD in that the treatments are grouped into blocks (Fig. 5). A block contains one set of the treatments. Grouping is used to get the conditions within a block as uniform as possible; therefore, any observed differences will be largely due to treatment effects. In order to keep the within block variability to a minimum, the block must run perpendicular to the gradient (Fig. 5). Differences among blocks are removed from the experimental error. In the CRD replications provided a basis for estimating experimental error. In the RCB, blocks (also implying replication) serve not only this purpose, but also make possible reduction of the uncontrolled variability. The block df and SS are taken from the error df and SS, respectively. For the RCB to be more efficient than the CRD, the error SS must be reduced proportionally more than the error df. In other

words, blocks should remove a significant amount of the variation. Variation among blocks adds to the credibility of an experiment; whereas, variation within blocks takes from the credibility of the experiment.

The RCB, design or some modification of it, is the most common design used in biological research. The analysis of variance procedure is relatively simple for the RCB. Missing data may be estimated arithmetically so that the experiment can be analyzed. As the blocks become large, the within block variation increases thereby increasing the experimental error. This is the main disadvantage in the RCB.

The linear or additive model for the RCB is $X_{ij} = \mu + \Upsilon_i + \beta_j + \epsilon_{ij}$. Any observed value, X_{ij}, is the sum of four components; whereas, each observed value in the CRD is the sum of three components. In the RCB model, μ, Υ_i, and ϵ_{ij} have the same meaning as they had in the CRD. The fourth component, β_j, is the important difference between the two models. Since the treatments are grouped into blocks in the RCB, each observation contains a block effect. Hypothetical data and the appropriate symbols for the RCB are given in Table 3.

Calculations of the SS in the analysis of variance procedure is as follows:

Total unadjusted SS = $\Sigma(X_{ij})^2 = 1^2 + 3^2 + \ldots + 4^2 = 285$

Correction factor (CF) for the overall mean = $(\Sigma X_{ij})^2/n = (1 + 3 + \ldots + 4)^2/9 = 225$

Total adjusted SS = Total unadjusted SS − CF = 285 − 225 = 60

Treatment SS = $\Sigma(X_{i.})^2/r - CF = (6^2 + 24^2 + 15^2)/3 - CF = 279 - 225 = 54$

Block SS = $\Sigma(X_{.j})^2/t - CF = (15^2 + 17^2 + 13^2)/3 - CF = 227.67 - 225.00 = 2.67$

Treatment X Block SS = Total SS − (Treatment SS + Block SS) (Error SS)
= 60 − (54.00 + 2.67) = 60.00 − 56.67 = 3.33

The variation has been partitioned and allocated to the source as illustrated in Fig. 6.

The relationship between the RCB model, $X_{ij} = \mu + \Upsilon_i + \beta_j + \epsilon_{ij}$, and the analysis of variance procedure can be seen by calculating the individual components of each X_{ij} value. The components of X_{11} are calculated as follows:

$X_{11} = \mu + \Upsilon_1 + \beta_1 + \epsilon_{11}$
$1.00 = 5.00 + (2.00 - 5.00) + (5.00 - 5.00) + \epsilon_{11}$
$1.00 = 5.00 + (-3.00) + 0 + \epsilon_{11}$
$1.00 = 2.00 + \epsilon_{11}$
$1.00 - 2.00 = \epsilon_{11}$
$-1.00 = \epsilon_{11}$

The overall mean or μ is 5.00, the effect of treatment 1 is −3.00, the effect of block 1 is 0, and the error associated with X_{11} is −1.00. Calculated values for the components of each of the X_{ij} values are presented graphically in Fig. 7. The CF (225) from the analysis of variance computation is equal to the nine μ's squared and summed ($9 \times 5^2 = 225$). The treatment SS of 54 is equal to the individual treatment effects squared and summed ($-3.00^2 + -3.00^2 + -3.00^2 + 3.00^2 + 3.00^2 + 3.00^2 + 0.00^2 + 0.00^2 + 0.00^2 = 54$). The block SS of 2.67 is equal to the individual block effects squared and summed ($0^2 + 0^2 + 0^2 + 0.67^2 + 0.67^2 + 0.67^2 + -0.67^2 + -0.67^2 + -0.67^2 = 2.67$). The error SS of 3.33 is equal to the nine ϵ_{ij}'s squared and summed ($-1.00^2 + 0.33^2 + 0.67^2 + 1.00^2 + -0.67^2 + -0.33^2 + 0.00^2 + 0.33^2 + -0.33^2 = 3.33$).

Are the differences among blocks and treatments significant in comparison with the uncontrolled variability or error? The null hypotheses would be that there are no differences among treatments or among blocks. In order to test the null hypotheses, the treatment SS, the block SS, and the error SS are divided by the appropriate df to get the MS (Table 4, Fig. 8). When the null hypotheses are not true, the treatment MS contains the error effect plus the treatment effect, and the block MS contains the error effect plus the block effect. The F test for treatments is the treatment MS divided by the error MS, whereas, the F test for blocks is the block MS divided by the error MS. In the present example, calculated F was 32.53 for treatments and 1.60 for blocks. With 2 and 4 df, an F of 4.32 is required for significance and a value of 6.94 is required before the differences are considered to be highly significant. Since the calculated F for treatments exceeds 6.94, differences of these magnitudes would be expected to occur less than 1/100 due to chance, and the null hypothesis would be rejected. The calculated F of 1.60 for replications indicates that there likely are no real differences among blocks, and the null hypothesis would be accepted.

The highly significant F for treatments indicates that there are differences among the three treatment means. It follows that the lowest mean is expected to be different from the highest mean (2 vs 8 in Table 3), but these means may or may not be different from the intermediate mean of 5. This question can be answered through the appropriate use of individual degree of freedom comparisons or through the use of mean separation techniques.

Assumptions of the Analysis of Variance – Use of the analysis of variance is appropriate only when the basic assumptions are met. The linear or additive characteristic of the model is important. In the model, $X_{ij} = \mu + \Upsilon_i + \beta_j + \epsilon_{ij}$, μ is usually positive and it is assumed that each of the other components adds either a positive or a negative effect. If any of the effects contribute a multiplicative effect, the analysis of variance is not appropriate. Additivity of treatment effects implies that the change in response from treatment to treatment is a fixed (additive) amount and not a product or ratio. The same is true for additivity of replication or block effects. Logarithmic and other types of transformations can be used to change multiplicative into additive effects.

The second assumption concerns the individual ϵ_{ij}'s of the model. The ϵ_{ij}'s are assumed to be random, independent, and normally distributed about a zero mean. Randomness and independence are related and will be considered together. Since experimental areas or environments vary, treatments applied on adjacent experimental units are compared with greater precision than those applied to more widely separated experimental units. When measuring biological responses, those measurements made at about the same time are likely to vary less than those made over a longer period of time. Subjective evaluations made by the same person will likely vary less than evaluations made by two or more persons. If the errors are to be independent, the location of treatments in the experimental area, the sequence of measurements, and the division of treatments to be evaluated by different people must be determined through a random process. These are only three of many known and unknown (to the experimenter) reasons why randomization is necessary to insure independence of errors.

The ϵ_{ij}'s are also assumed to have a normal distribution with a mean of zero. If frequency is plotted against magnitude of the ϵ_{ij}'s, the resulting curve should be symmetrical and bell shaped. The most frequent class would be zero. Since the number and magnitude of the positive ϵ_{ij}'s should equal the number and magnitude of the negative ϵ_{ij}'s, the sum and mean of the ϵ_{ij}'s are expected to equal zero. When the ϵ_{ij}'s are not normally distributed, logarithmic, square root, or other transformations may be used to make the ϵ_{ij}'s more normally distributed.

When the ϵ_{ij}'s for the observations within a treatment are squared and summed, the resulting value is the error variance for that treatment. The error mean square or variance in the analysis of variance is an average error which comprises the contribution from each of the treatments being compared. It is assumed that the error variances contributed by the treatments being compared are homogeneous. Nonhomogeneous error variances may be encountered in comparisons of fungicides, herbicides, etc., when the treatments differ in effectiveness. For example, if a number of herbicides are being compared for their effectiveness in killing weeds, the within variability for the more effective treatments is likely to be less than the within variability for the less effective treatments. In such experiments the treatment error variances are not likely to be homogeneous. In the analysis of variance the treatment means are compared to see whether they are likely to be the same or different, but the treatment variances are assumed to be the same.

For most biological data the assumptions underlying the analysis of variance are not completely fulfilled. Using the analysis of variance when the assumptions are invalid usually increases the frequency of rejection of the null hypothesis when it is true. Therefore, conclusions should be regarded as approximate rather than exact.

Summary

The analysis of variance is a widely used procedure for analyzing biological variation. The researcher wants to know whether variation due to controlled factors is meaningful in terms of the uncontrolled variability. Each biological measurement in an experiment reflects these controlled and uncontrolled effects. In the completely randomized design, the variability is divided into among treatments and within treatments or error. In the randomized complete block design, the variability is allocated to blocks, treatments, and error. After the variability (sums of squares) has been divided by the appropriate degrees of freedom, the test of significance (F) involves a ratio of the treatment or block mean squares divided by the error mean square. F-tables are used to provide a known probability basis for accepting or rejecting the null hypothesis.

Appropriate use of the analysis of variance procedure depends upon a number of assumptions. These include: additive effects; randomness, independence, and normality of errors; and homogeneity of treatment variances.

T_2	T_3	T_3
T_1	T_1	T_2
T_2	T_1	T_3

Fig. 1. A randomized arrangement of three treatments (T_1, T_2, T_3), each replicated three times in a completely randomized design. Each block is an experimental unit.

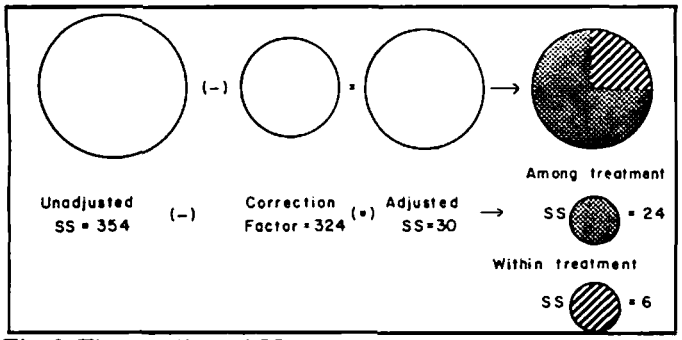

Fig. 2. The unadjusted SS minus the CF equals the adjusted SS which is divided into the among and within treatment SS.

REFERENCES

Cochran, W. G. and G. M. Cox. 1957. Experimental Designs. 2nd ed. John Wiley and Sons, Inc., New York.

Federer, W. T. 1955. Experimental Design. The Maxmillan Company, New York.

Hicks, C. R. 1964. Fundamental Concepts in the Design of Experiments. Holt, Rinehart and Winston, New York.

Kempthorne, O. 1952. The Design and Analysis of Experiments. John Wiley and Sons, Inc., New York.

LeClerg, E. L., W. H. Leonard, and G. C. Clark. 1962. Field Plot Technique. 2nd ed. Burgess Publishing Company, Minneapolis.

Li, C. C. 1964. Introduction to Experimental Statistics McGraw-Hill Book Company, New York.

Snedecor, G. W. and W. G. Cochran. 1967. Statistical Methods. 6th ed. Iowa State University Press, Ames.

Sokal, R. R. and F. J. Rohlf. 1969. Biometry. W. H. Freeman and Company, San Francisco.

Steel, R. G. D. and J. H. Torrie. 1960. Principles and procedures of Statistics. McGraw-Hill Book Company, New York.

ACKNOWLEDGEMENT

The author expresses sincere thanks to the following for their advice and suggestions:

Dr. Jerry M. Arnold — Mr. Gary Elder
Dr. William R. Meredith — Dr. Marvin L. Risius
Mr. James S. Rice — Mr. John E. Shirley

However, this acknowledgement is not meant to imply that these persons approve of all statements and interpretations as given.

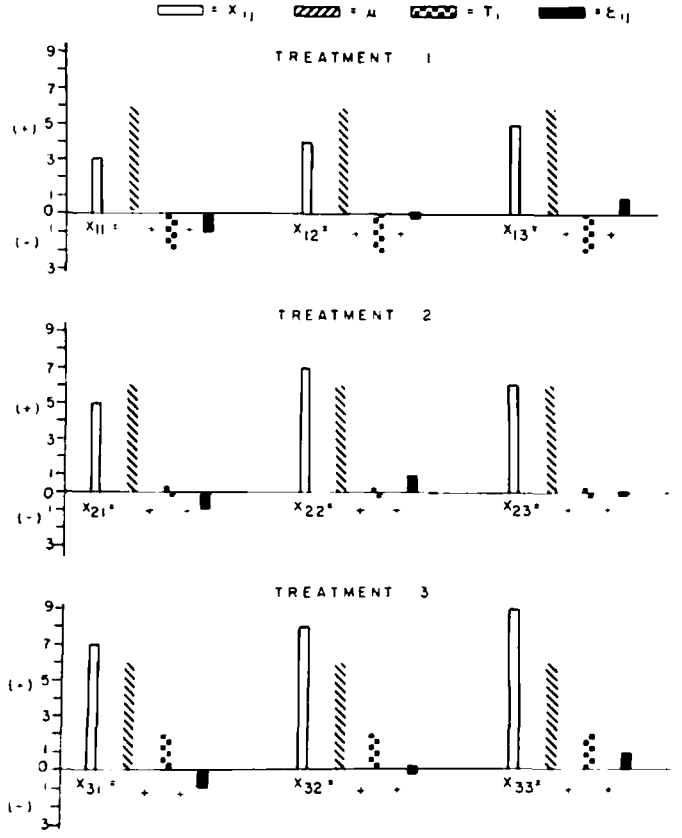

Fig. 3. $X_{ij} = \mu + T_i + \epsilon_{ij}$ for completely randomized design.

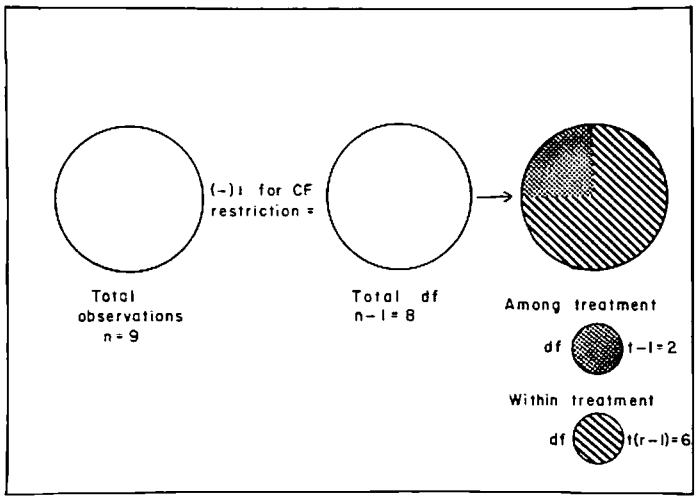

Fig. 4. The total number of observations minus one for the CF restriction equals the total df which is divided into the among and within treatment df.

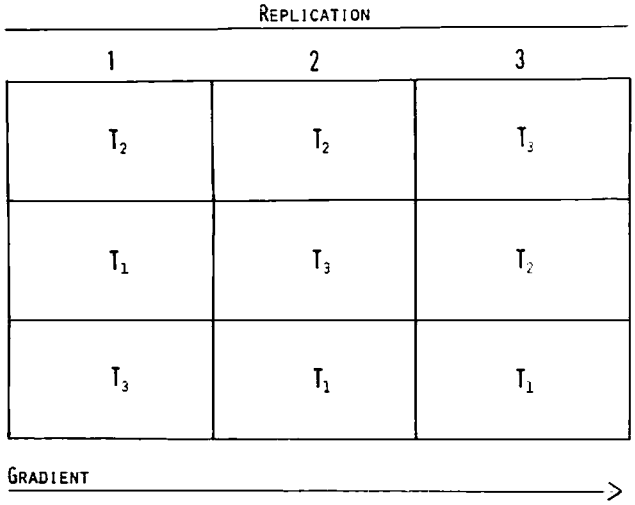

Fig. 5. A randomized arrangement of three treatments (T_1, T_2, and T_3) in each block of a randomized complete block design. Each block is an experimental unit.

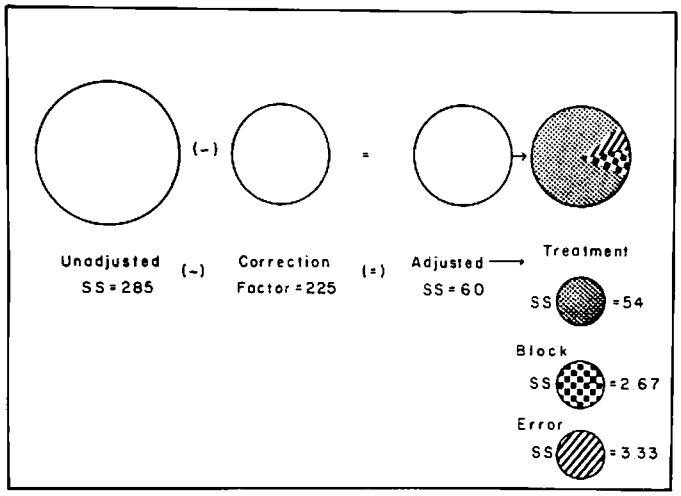

Fig. 6. The unadjusted SS minus the CF equals the adjusted SS which is divided into the treatment, block, and error SS.

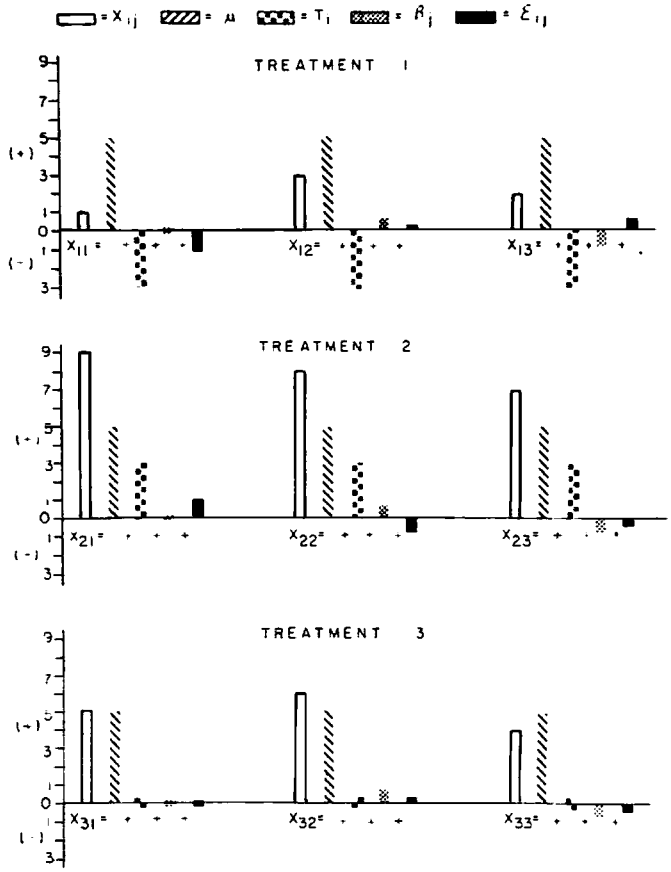

Fig. 7. $X_{ij} = \mu + \Upsilon_i + \beta_j + \epsilon_{ij}$ for randomized complete block design.

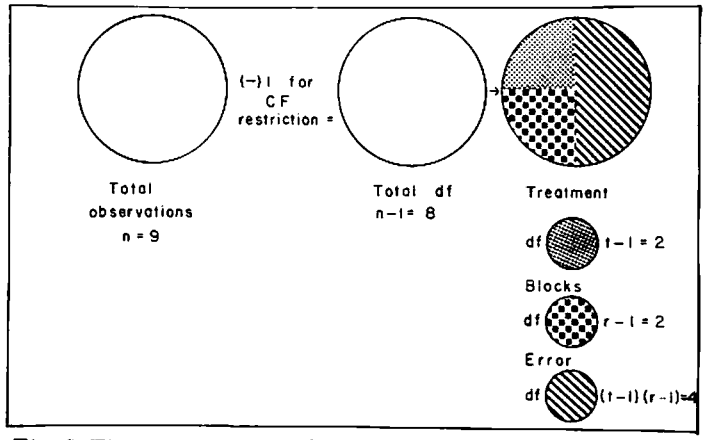

Fig. 8. The total number of observations minus one for the CF restriction equals the total df which is divided into treatment, block, and error df.

Table 1. Hypothetical data and symbols for three treatments and three replications in a completely randomized design[1].

Treatment 1		Treatment 2		Treatment 3	
Observed value	Symbol	Observed value	Symbol	Observed value	Symbol
3	X_{11}	5	X_{21}	7	X_{31}
4	X_{12}	7	X_{22}	8	X_{32}
5	X_{13}	6	X_{23}	9	X_{33}
Sum 12	$X_{1.}$	18	$X_{2.}$	24	$X_{3.}$
Mean 4	$\bar{X}_{1.}$	6	$\bar{X}_{2.}$	8	$\bar{X}_{3.}$

[1]The grand total, X . . , is 54 and the overall mean is 6.

Table 2. Analysis of variance for completely randomized design.

Source of variation	Degrees of freedom	Sum of squares	Mean squares	F
Total	8	30		
Among treatments	2	24	12	12**
Within treatments (Error)	6	6	1	

**Indicates significance at the .01 level of probability.

Table 3. Hypothetical data and symbols for three treatments and three replications in a randomized complete block design[1].

	Treatment 1		Treatment 2		Treatment 3		Replication			
Replication	Observed value	Symbol	Observed value	Symbol	Observed value	Symbol	Sum	Symbol	Mean	Symbol
1	1	X_{11}	9	X_{21}	5	X_{31}	15	$X_{.1}$	5.00	$\bar{X}_{.1}$
2	3	X_{12}	8	X_{22}	6	X_{32}	17	$X_{.2}$	5.67	$\bar{X}_{.2}$
3	2	X_{13}	7	X_{23}	4	X_{33}	13	$X_{.3}$	4.33	$X_{.3}$
Treatment sum	6	$X_{1.}$	24	$X_{2.}$	15	$X_{3.}$				
Treatment mean	2	$\bar{X}_{1.}$	8	$\bar{X}_{2.}$	5	$\bar{X}_{3.}$				

[1]The grand total, X . . , is 45 and the overall mean is 6.

Table 4. Analysis of variance for randomized complete block design.

Source of variance	Degrees of freedom	Sum of squares	Mean squares	F
Total	8	60.00		
Treatments	2	54.00	27.00	32.53**
Blocks	2	2.67	1.33	1.60[ns]
Error	4	3.33	0.83	

**Indicates significance at the .01 level of probability.
[ns]Nonsignificant at the .05 level of probability.

KLIEJUNAS

John Kliejunas will be graduating from the University of Wisconsin in June 1971 with a Ph.D. in Plant Pathology. He is interested in a teaching and/or research position in forest pathology or plant pathology. A resume is on file with the editor.

Ensminger-Interstate Distinguished Teacher Award

1970

The establishment of the Annual Ensminger-Interstate Distinguished Teacher Award of $1,000.00 and a plaque along with the selection criteria were announced in the NACTA Journal, March 1969, pages 16-17. The following selection criteria includes the slight modification of October 2, 1969.

I. The recipient to be selected shall:
   A. Be a NACTA member in good standing for at least one year prior to receiving the Award.
   B. Be participating in a program in which the effectiveness of his teaching is being or has been evaluated by his students. This can be either
      1. The NACTA teacher evaluation program. See the report of the Teacher Evaluation and Recognition Committee (hereafter the Committee), NACTA Journal, June, 1968; or
      2. A formal system such as may be functioning in his Institution; results to be included in his dossier.
   C. Be now primarily engaged in classroom teaching and for at least five years previously have been so professionally involved.
   D. Have his administrative officer (department head or dean) prepare a dossier of evidences that would establish him as an outstanding teacher and place three copies in the hands of the Committee chairman (Dr. E. Grant Moody, Agriculture Division, Arizona State University, Tempe, 85281) three months before the NACTA annual meeting (or April 18, 1971). This dossier should include but is not limited to:
      1. An evaluation of the applicant's qualifications as a teacher which will indicate the extent to which the teacher makes himself available for student counselling, advisory and other pedagogical activities; attracts, involves and intellectually stimulates students; keeps himself current in both his subject matter and teaching techniques; and is sincerely dedicated to the search for truth and expanding knowledge and recognizes as legitimate and encourages similar dedication among his colleagues. A completed "Teacher Evaluation by Administrative Officer" will be submitted in addition to other evidence including statements by colleagues.
      2. A letter evaluating his teaching effectiveness from a student honor society (or equivalent) of his institution (Delta Tau Alpha, Alpha Zeta, etc.)
      3. A completed "Teacher Evaluation by Alumni" form with any appropriate additional comments from each of five who are not now enrolled in his school but who have previously been enrolled in his classes.
      4. A written statement by the teacher identifying his philosophy of teaching and how he makes it effective.
      5. Other pertinent material.
II. Final selection will be made by an ad hoc committee to be appointed by the NACTA President, the Committee chairman to serve as ex officio.
III. This award will be presented to any individual only once.
IV. Modification of these criteria in future years may be made by the Committee with approval of the NACTA Executive Committee and in consultation with the Donor.