EXPERIMENTAL DESIGN

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EXPERIMENTAL DESIGN

Theory and Application

by

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To LILLIAN

Preface

DURING the academic year 1948-1949 a set of mimeographed notes entitled "Designs for Research and Observational Experiments" was prepared for a series of lectures at the Geneva Experiment Station, Geneva, New York, and for class use in a course on the design of experiments at Cornell University. The original notes were revised and expanded to obtain the material for this text; this material is used for the present course on the design of experiments at Cornell University.

Facts concerning experimental designs are scattered throughout statistical literature, and no single text suffices entirely for a course on the design of experiments. The basic reason for writing this book was to present subject matter and techniques not available in other texts. A comprehensive coverage of the design and the analysis of experiments is given. The analyses of designs with missing observations, the analyses for variations from standard designs, components of variance analyses, and covariance analyses are treated in detail. Extensive use is made of numerical examples. Problems and literature citations to numerical examples are given for the various designs. A unified approach is followed for confounding in factorial experiments and for the analysis of incomplete block designs.

The design and analysis for a particular experimental design is presented in the first part of a chapter. This is followed by variation in the basic design. The material on components of variance is presented in the last part of a chapter. With the material arranged in this fashion it is possible to teach several types of courses from this book. The most elementary course would utilize the material in Chapter I, the first parts of Chapters IV, V, and VI, Chapter VII, and the first parts of some of the remaining chapters. A more comprehensive course would make use of the material in other sections of the book. Also, a short course on variance component analyses would use the material presented in the last sections of Chapters IV, V, VI, VIII, X, XI, and XVI.

The student will need to understand basic statistical analyses and concepts as described in the texts by Professors G. W. Snedecor, *Statistical Methods*, and R. A. Fisher, *Statistical Methods for Research Workers*. The latter reference is somewhat more advanced than is required for an understanding of most parts of the present dissertation. Full comprehension of the mathematical theory underlying the analyses of the various designs requires mastery of the principles of calculus and matrix algebra. However, the material on variance

component analyses is presented in such a fashion that the manipulations may be performed with an understanding of college algebra.

In general, the 5 per cent level of significance is used throughout the text. Whether or not this is a desired level depends upon the stage of the research program, the possible economic gains or losses resulting from the research, and the number of replicates used. In the early stages of a program, the experimenter may be investigating a number of treatments to determine if they should be tested further. Material for more than one, two, or three replicates may be unavailable, and the experimenter may wish to use a 10, 15, 20, or 25 per cent level of significance test in order to have a type II error (error of failing to detect a specified difference when the treatments actually differ by more than this amount) of reasonable size. In the final stages of a program, the experimenter may wish to use a one per cent level of significance test and to use enough replicates or samples to have a type II error which is one per cent or less.

The author wishes to express his thanks and appreciation to J. M. May, E. S. Pearson, H. O. Hartley, and the Biometrika Trustees for permission to reprint the material in table II-1; to D. B. Duncan and the editor of *Biometrics*, G. M. Cox, for permission to reprint the material in table II-3; to T. F. Kurtz, R. F. Link, D. L. Wallace, and J. W. Tukey for permission to reprint the material in table II-4; to D. B. Duncan and to the editor of the *Virginia Journal of Science*, Boyd Harshbarger, for permission to reprint the material in tables II-5 and II-6; to M. Merrington, C. M. Thompson, E. S. Pearson, H. O. Hartley, and to the Biometrika Trustees for permission to use the material in table II-8; to J. F. Crow and to the editor of the *Journal of the American Statistical Association*, Allan Wallis, for permission to reproduce figure II-4; to M. Harris, D. G. Horvitz, A. M. Mood, and to the editor of the *Journal of the American Statistical Association* for permission to reprint tables III-1, III-2, and a part of III-3; to E. Lehmer and to the editor of the *Annals of Mathematical Statistics*, E. L. Lehmann, for permission to reprint the material in table III-4; to C. R. Henderson for permission to use the material in section VIII-5.3; to W. G. Cochran for the material in section XI-8.1; to M. L. Richards for the analysis in section XIV-2.4; to G. M. Cox for the analysis in section XV-3; to D. B. Duncan for some of the problems in Chapters X and XI; and to the numerous other experimenters whose work has been used.

My philosophy of experimentation and education in statistics has been influenced by my association with A. C. Hildreth, LeRoy Powers, W. H. Leonard, R. W. Jugenheimer, A. J. King, G. W. Snedecor, W. G. Cochran, A. M. Mood, P. G. Homeyer, G. F. Sprague, I. J. Johnson, G. M. Cox, O. Kempthorne, other staff members of Iowa State College, staff members of Cornell University, and the numerous persons with whom I have consulted on statistical problems. I am particularly indebted to F. Yates, W. G.

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For their many helpful comments on the first set of mimeographed notes, I wish to thank R. G. D. Steel, W. J. Drapala, J. H. Torrie, and students in my classes. The constructive comments and searching queries of D. S. Robson, H. Fairfield Smith, Prasert Na Nagara, and C. R. Henderson were of great value in preparing the final manuscript; these individuals spent a considerable amount of time in reading the manuscript and their aid is greatly appreciated. Several other individuals, J. E. Dowd, W. H. Leonard, A. G. Clark, E. E. Houseman, H. L. Everett, D. D. Mason, H. L. Lucas, and other associates, offered helpful comments on parts of the mansucript. J. W. Tukey, D. B. Duncan, and F. J. Anscombe were kind enough to read chapter II; their comments were greatly appreciated.

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W. T. FEDERER

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EXPERIMENTAL DESIGN

CHAPTER I

Introduction

All fields of research have at least one feature in common, i.e., the variability of experimental material. The universality of heterogeneity has been known and discussed at considerable length in the literature (e.g., in the writings of J. Arthur Harris [142-146] from 1913 to the early thirties). The variability in experimental material may vary in size, but its omnipresence cannot be denied. When the variability is small relative to the group or class differences and the observations are not costly, an elaborate experimental design is not required. The experimenter simply takes the results from a large number of readings and observes the relationships or mean differences. For example, it may be a simple procedure to take ten thousand observations on each level of a factor. The variation around a mean for a given level may be made as small as desired simply by taking more observations. A curve may be fitted to the several means, and the curve is essentially the theoretical one.

When there is considerable variation from observation to observation on the same material and it is not feasible to take a large number of observations, the experimenter is forced to refine his experimental techniques and (or) to use an experimental design which allows for unbiased estimates of the true treatment differences with a specified degree of precision. It is necessary that a probability statement be attached to treatment differences if inferences beyond the data are to be made. (The term *treatment* is a general one and is used to denote a class, category, or group.) In order to attach a probability statement to estimated treatment differences, Fisher [126] has stated that *randomization* and *replication* are two required conditions for an experimental procedure or design. This also means that randomization and replication are necessary to obtain a valid estimate of the error variation. A valid estimate of the error variance (or *experimental error variance*) makes possible statements concerning the percentage of errors committed by the experimenter when he states that treatment means are different when the differences are actually chance sampling fluctuations and that treatment means do not differ when they actually do differ by more than some specified value.

Regardless of the size of relative variability, there are certain principles of scientific experimentation that should be followed. Perhaps it is not the place of the statistician to stress these principles, since most of them are

nonstatistical [72]. *However, a consulting statistician is continually faced* with statistical problems incurred because the experimenter failed to observe one or more of the basic principles. The progress of research in any field will be greatly accelerated if the questions whose answers are sought, probable results, design of the experiment, selection of the entries to be tested (the treatments), the method of analysis, presentation of results, etc., are closely studied *before* the experiment has been conducted.

I-1 The Principles of Scientific Experimentation

The principles involved in scientific experimentation have been stated in many ways and in many places. A few of the books written on the subject are cited at the end of this book [41, 63, 89, 167, 245, 312]. The books by Wilson [312] and Churchman [41] are recommended for reading since they include a discussion of scientific experimentation in light of present statistical knowledge. Also, articles on experimental methods, on statistical analyses, and on the interpretation of results appear in a number of scientific journals [e.g., 2, 170, 250, 274]. Goulden [135], Love [196], and Leonard and Clark [193] have enunciated these principles for agronomists. The principles of scientific experimentation are listed below and are discussed individually.

I-1.1 FORMULATION OF QUESTIONS TO BE ANSWERED AND HYPOTHESES TO BE TESTED

A scientific experiment should be set up to answer a specific question or questions. Precise formulation of the question (or questions) to be answered enables the experimenter to state his hypotheses in more precise terms and to plan his experimental procedure more effectively. Clear and precise questions and hypotheses at this stage enable the experimenter to proceed rapidly to the next step. It is advisable to have a statement of the questions and hypotheses in written form.

A *hypothesis* is a statement about the parameter or parameters in one or more populations. A *null hypothesis* is a statement of no difference between the parameters involved. The null hypothesis can never be proved by any finite amount of experimentation [126, sec. 8]. Given a particular null hypothesis with no specific *alternative hypothesis*, the experimenter either rejects or does not reject the null hypothesis. The term "does not reject" does not imply acceptance of the null hypothesis for the case where no specific alternative is postulated. Experimental evidence can lead to the rejection of the null hypothesis but not to its acceptance.

Fisher [126, sec. 8] states that the above ideas may be extended to the simultaneous consideration of several hypothetical possibilities "in cases involving statistical 'estimation.'" Specific alternative hypotheses about the parameters may be set up. All hypotheses so set up should be free from vagueness and ambiguity; they must be formulated in precise terms. In this case,

the hypothesis of no difference, say H_0, may be accepted. Given a rule of procedure for accepting or rejecting a null hypothesis, it is possible to calculate the errors committed when any one of the hypotheses is true. Rules of procedure generally fix in advance the probability of error of rejection when the null hypothesis is true. Then, one is able to calculate the errors associated with alternatives.

If a null hypothesis, H_0, and an alternative hypothesis, say H_1, are postulated by the experimenter, two kinds of error may result. The first kind of error (type I error) arises when H_0 is rejected for a given experiment when H_0 is actually true. The second kind of error (type II error) arises when H_0 is accepted when it is actually false and H_1 is true.

Null hypotheses and alternative hypotheses should be formulated precisely for each experiment. Clear thinking about the problems involved in each experiment is necessary in setting up hypotheses, and care should be used in selecting the appropriate tool for testing a hypothesis. Various statistical methods have been developed for testing hypotheses. A statistical test or a test of significance of a hypothesis is a procedure whereby the hypothesis is rejected or is not rejected, with sufficient explanation of what "not rejected" means. "Not rejected" may mean a qualified acceptance until sufficient evidence accumulates. Statistical tests for various hypotheses are given throughout the following chapters.

Several tests of significance may be available for testing a given hypothesis. A good test is one which minimizes the probabilities of all errors over all possible values of the parameters. Some tests are better than others for a given alternative hypothesis but are not as good for other alternatives. In order to compare two tests for two hypotheses, the procedure is as follows. First select a type I error (say .05). Then compute the quantity, 1 − probability of a type II error, which is called *the power of the test,* for each test; the test with a smaller type II error, or a higher power, is the better test.

The above discussion contains the general ideas concerned with testing hypotheses. For a fuller treatment of this topic the reader is referred to the books by Mood [212, Ch. 12], Wald [299, 300], and Mann [207, Ch. 6].

I-1.2 A CRITICAL AND LOGICAL ANALYSIS OF THE PROBLEM OR PROBLEMS RAISED

After formulating the hypotheses, the experimenter should critically and logically evaluate them. A review of pertinent literature is a valuable aid in evaluating hypotheses. The reasonableness and utility of the aims of the experiment should be carefully considered. After a critical evaluation of the hypotheses in step 1, the experimenter may find it advisable to reformulate the questions and hypotheses before proceeding with the experiment.

In order to utilize statistical tools such as *Statistical Decision Functions* [300], the experimenter is required to evaluate the possible outcomes of the

experiment in terms of relative costs. This step involves more time spent in considering the hypotheses in terms of various outcomes. Consideration of possible outcomes and costs is necessary if one is to make use of new and powerful statistical tools. The experimenter needs to provide answers to such questions as: (i) "If the results are of such and such a nature, what do they mean to me in dollars and cents?," (ii) "If the results are of another nature, what value do I ascribe to them?," etc.

I-1.3 SELECTION OF A PROCEDURE FOR RESEARCH

The procedure to be followed depends to a large extent upon the field in which the research is being conducted. The selected experimental procedure in most fields of research will involve some or all of the following considerations:

(i) the selection of treatments or entries to be included in the experiment,
(ii) the selection of characteristics to be measured,
(iii) the selection of the unit of observation, number of replications, and the sampling or experimental design (The latter should be amenable to statistical analyses.),
(iv) the control of the effect of adjacent units on each other or "competition,"
(v) an outline of pertinent summary tables and probable results,
(vi) an outline of analyses to be performed,
(vii) a statement of costs in terms of material, personnel, equipment, etc.

After considering the above items, the experimenter may decide to reformulate the hypotheses, to change the experiment, or to continue to the next step in scientific experimentation. The formulation of hypotheses and the selection of treatments are extremely important and closely associated considerations. A large part of the success of an experiment may depend upon the correct selection of treatments. Also, the selection of an appropriate experimental or sampling design is of considerable importance in the testing of hypotheses and in estimating treatment effects.

I-1.4 SELECTION OF SUITABLE MEASURING INSTRUMENTS AND CONTROL OF THE PERSONAL EQUATION

In any experiment the experimenter must select measuring instruments that are sufficiently accurate and an experimental procedure that is free from personal biases or favoritisms. In some studies, it is the practice to observe three samples and to discard the "most discrepant" one. The procedure goes even further in the so-called "intelligent placement" of treatments, where the favorites are placed under the best conditions, and if a "favorite" is lower than preconceived, the result is discarded. If the experimenter follows the procedure described by Fortmann [130] and in the references cited by him, he may utilize the practice of discarding one of the observations even though it will not be possible to use ordinary statistical techniques. The question of discarding units of observations often arises in experiments in which the units of observa-

tions are not of equal value in their contribution to a treatment mean. For example, a field plot or a laboratory sample for one of the treatments may be destroyed or damaged because of the carelessness of the technician; a group of the experimental animals [198] or plants may become diseased; adverse weather conditions may destroy or damage part of an experiment; etc. The question may arise as to whether or not zeros or abnormal values from the damaged experimental units should be retained in the statistical analyses or deleted prior to statistical analyses. In this regard, experimenters are urged to consider the following rule: *If the observation contributes to the true treatment difference, it should always be retained. If the observation does not contribute to the true treatment difference, it should be discarded, provided that the reason for discarding the observation is one that would be held valid by the experts in that field.*

The appropriate measuring instrument should be selected prior to taking any measurements and should be retained throughout. Changes of measuring instruments should not be made in the middle of an experiment.

The problem of controlling other personal biases may or may not require elaborate precautions. Often some simple procedure suffices. For example, in the analyses of samples in the laboratory a random assignment of numbers to samples often provides unbiased results which are not possible when the technician knows the identity of the samples.

I-1.5 A COMPLETE ANALYSIS OF THE DATA AND THE INTERPRETATION OF RESULTS IN LIGHT OF EXPERIMENTAL CONDITIONS AND HYPOTHESES TESTED

Statistical procedures are valuable aids in *reducing* the data to summary form. The results of an experiment must be interpreted in light of the statistical evidence obtained and the theoretical considerations of the subject under experimentation. The "interpretation" of the results of an experiment does not end with the calculation of a mean, of an F value, or of a confidence interval! In testing hypotheses and estimating effects, it is essential to use the appropriate statistical procedures. The resulting computations should be checked to insure against computational errors.

I-1.6 PREPARATION OF A COMPLETE, CORRECT, AND READABLE REPORT OF THE EXPERIMENT

The pertinent results of an experiment should be completely and carefully reported in written form. This may involve typewritten copies to a selected group of individuals or a printed report in published form. One should present enough of the summary data to allow others to test the various hypotheses. By giving an account of the experimental procedure and statistical methods (or references to statistical methods) used, the reader may decide for himself whether or not he considers the procedure sound.

The question often arises about so-called "negative results," i.e., results which would lead one not to reject the hypothesis of no difference (the null

hypothesis). If the experimental procedure *is correct, there are no "negative results,"* and a report should be made. The experimenter should offer a reason for not finding differences which he suspected were present. Explanations of "negative results" are often of considerable value. A careful report on such experiments may be of considerable value to other experimenters conducting similar research.

It is conceivable that a principle could appear to be established if only "positive results" were published and if the reported results reflected merely discrepant sampling fluctuations and not true treatment differences. For example, medical researchers might be evaluating the effectiveness of two drugs, A and B. Suppose that A and B are equally effective but that the only experimental results published are those which indicate that A is better than B. As a result, drug A may be selected over drug B. In this case, no harm results to the patient, since the drugs are equally effective. If detrimental side effects of drug A over drug B are present, or if the cost of manufacturing drug A is higher, the failure to report all results would have undesirable consequences.

I-1.7 STATISTICS IN RELATION TO THE PRINCIPLES OF SCIENTIFIC EXPERIMENTATION

It should be emphasized that the above principles of scientific experimentation are not entirely statistical, nor does every step involve statistics. For example, steps one and two are entirely nonstatistical and are completely within the scope of the field in which the experiment is performed. In step three (i), (ii), (iv), and (vii) are nonstatistical, (v) may or may not be statistical, and items (iii) and (vi) are entirely statistical in nature. Step four is partly statistical. Step five is highly statistical, but step six is nonstatistical. Hence, the design of the experiment is only one item to be considered in planning and conducting the experiment and in interpreting and reporting the experimental results.

I-2 Classification of Experimental Designs

In planning any experiment, an experimental procedure or design of experiment is selected. The selection may be good or bad. The experimental plan may be of a "systematic nature" or of a "random nature." Analysis of variance techniques are suitable only for experiments in which use has been made of the elements of chance in allotting the treatments to the experimental units or experimental plans of a "random nature" [126, sec. 26].

All experimental designs may be divided into *systematic designs* and *randomized designs*. Since the same partitions of systematic designs and of randomized designs can be made, only a few selected examples of systematic designs are given.

I-2.1 SYSTEMATIC DESIGNS

Prior to the development of modern experimental designs, experimenters tried various arrangements which are not subject to the laws of chance. Syste-

matic schemes of arranging the treatments in the various repetitions have been devised. One such scheme is to arrange all duplicates, triplicates, etc. of the treatment together. Suppose the experimenter wishes to test three treatments, A, B, and C, and that he decides to have four repetitions of each treatment. The arrangement of the three treatments over the experimental area could be one of the following:

A A A A	B B B B	C C C C

A A A A
B B B B
C C C C

A	B	C
A	B	C
A	B	C
A	B	C

A A	B B	C C
A A	B B	C C

From fertilizer, yield, and other trials or experiments, it is evident that it might be better to test treatments A, B, and C together in a compact block and then to repeat these blocks. Before Prof. Fisher established the principle of randomization, a systematic ordering of the treatments in each block or repetition seemed natural. One of the more common types of systematic arrangements in which the treatments are repeated several times is the following:

Replicate I	Replicate II	Replicate III
A B C	A B C	A B C

or

Replicate I	A B C
Replicate II	A B C
Replicate III	A B C

In this case the ordering of the treatments is exactly the same in every replicate (a unit which contains all the treatments). Another systematic arrangement is the following:

Replicate I	Replicate II	Replicate III
A B C	C A B	B C A

In this case, each treatment occupies each order in the replicate.

Another systematic arrangement proposed is the "diagonal square" [126, sec. 34]; for three treatments the design is

A	B	C
C	A	B
B	C	A

and for five treatments the design is

A	B	C	D	E
E	A	B	C	D
D	E	A	B	C
C	D	E	A	B
B	C	D	E	A

In order to eliminate the effect of *A* appearing on one diagonal, a systematic arrangement involving the Knight's Move was proposed, i.e., one down and two over. This arrangement for five treatments in three replicates gives the following design:

Replicate I	A	B	C	D	E
Replicate II	D	E	A	B	C
Replicate III	B	C	D	E	A

For five replicates the arrangement is

A	B	C	D	E
D	E	A	B	C
B	C	D	E	A
E	A	B	C	D
C	D	E	A	B

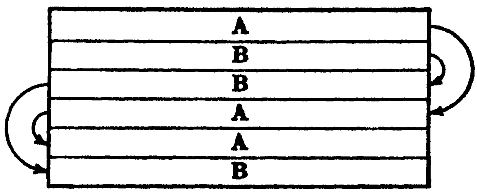
Fisher [126, sec. 34] states that the above design has been known in Denmark since about 1872, but that it is usually ascribed to a Norwegian, Knut Vik, and called the Knut Vik Square.

Another well-known systematic design is Dr. Beavan's "chessboard" system with square-yard plots. Student [284] states that this design was in use at Warminster prior to 1909. Beavan's arrangement follows:

A	F	C	H	E	B	G	D
B	G	D	A	F	C	H	E
C	H	E	B	G	D	A	F
D	A	F	C	H	E	B	G
E	B	G	D	A	F	C	H

The above arrangement is repeated as often as necessary.

Student [284] describes another arrangement, Beavan's half-drill strip method, that has been used extensively in field trials with cereals. Arrangement for the half-drill method follows:



Numerous other systematic arrangements have been devised, with the various experimenters attempting to outguess natural variation. Regardless of the type of systematic design, they all have relatively the same advantages and disadvantages [326a]. The advantages are often given as

(i) Simplicity. Many experimenters feel that planting, note-taking, and harvesting in agronomic trials are facilitated by using systematic arrangements. In judging or scoring experiments, it is sometimes felt that the judge will be better able to "discriminate" between the treatments if he (the judge) knows the order in which the treatments occur in the different repetitions.

(ii) The systematic design provides "adequate" sampling of the experimental area; that is, it allows for "intelligent placement" of the various treatments.

(iii) Varieties may be arranged in order of maturity; fertilizer treatments may be arranged in order of increasing fertility; etc.

(iv) It may be desirable to alternate dissimilar varieties (say, bearded versus beardless barley) so that natural crossing or mechanical mixtures can be detected in subsequent years.

(v) There is no need to randomize, since the heterogeneity of the experimental site is such as to randomize the effects on the treatments. (This does not lessen the effect of one treatment on another or of a single arrangement; these facts should not be ignored.)

The disadvantages of the systematic designs are:

(i) there is no valid estimate of the variance;

(ii) the correlation between adjacent plots may lead to systematic errors in assessing treatment differences.

The latter point is easily illustrated by the following systematic arrangement:

Replicate I	Replicate II	Replicate III
A B C	A B C	A B C

where the yield gradient is assumed to exist from left to right. Even though treatments A, B, and C may be the same thing, the experiment would show A to be better than B, and B better than C. In the event that the treatments are different, their differences may be exaggerated or underestimated, depending upon the arrangement of the treatments.

Field experience has demonstrated that with proper organization the experimental procedure in randomized arrangements is generally as simple as with systematic arrangements. It is often advantageous if the judge does not know the identity of the treatments, since the comparison of material with known identity often leads to biases.

Fisher [126, sec. 27 and 34] discusses some systematic arrangements and their effect on tests of significance and on the estimation of an error variance [see 70]. Suppose that systematic arrangements are tried on uniformity trial data (plot data on the same treatment over the whole of the experimental site or area).[1] Then the treatments in the experimental area would all be the same thing. The total sum of squares (*ss*) would be a constant regardless of what arrangement was chosen. If the experimenter is able to place the treatments so that all are subjected to about the same heterogeneity, then the sum of squares due to the differences between dummy or pseudo varieties would be decreased. The decrease must be counterbalanced by an increase in the error or remainder sum of squares, since the total is a constant:

$$\text{total } ss = ss \text{ among dummy var.} + ss \text{ within var.}$$

If, on the other hand, the experimenter does a poor job of placing the dummy varieties, the estimate of the error sum of squares will be smaller than it really should be and the differences between the dummy varieties will be exaggerated. Some arrangements may consistently underestimate the error variance. The amount of underestimation is unknown, and any attempt to obtain an estimate of the error variance from systematic arrangements is pretty much a matter of guesswork [see 70].

I-2.2 RANDOMIZED DESIGNS

Various classifications for randomized designs may be made. For example, such classifications as completely randomized (randomized over the whole of the experimental area), randomized complete block (randomization confined to allotment of all treatments within each of the several blocks), latin squares and variations, incomplete blocks (only a fraction of the set of treatments in

[1]The reader is referred to the book entitled *Field Plot Technique*, by Leonard and Clark [193], for a discussion of systematic versus random arrangements on uniformity trial data.

each of the several blocks), and others could be used. One could extend this classification in the following manner:

1A. Systematic—treatments laid out systematically by design
1B. Randomized—treatments randomly allotted to the experimental units
    2A. Experimental area not subdivided prior to randomization
        —completely randomized
    2B. Experimental area subdivided prior to randomization
        3A. Complete blocks—all treatments appear together in a block
            4A. No additional restriction—
            4B. Randomized complete block
                —additional restrictions
                5A. Two restrictions
                    —latin square
                    —cross-over
                5B. More than two restrictions
                    —graeco-latin square
                    —hyper-graeco-latin square
                    —others
        3B. Incomplete blocks—only k of the v treatments appear in any one block
            4A. Incomplete blocks not grouped into complete blocks by replicates (v treatments in blocks of k)
                    —partially balanced
                    —balanced
                    —others
            4B. Other designs
                5A. Incomplete blocks grouped into complete blocks
                    6A. n-dimensional (k^n treatments)
                        7A. Partially balanced
                            one restriction
                                —simple lattice
                                —triple lattice
                                —cubic lattice
                                —quartic lattice
                                —split plot
                                —split block
                            two restrictions
                                —incomplete lattice square
                                —semi-balanced lattice square
                                —split split plots
                                  etc.
                            .
                            .
                            .
                            n-restrictions

7B. Balanced (k = prime or power of a prime number)
one restriction
—balanced lattice
two restrictions
—balanced lattice square
.
.
.
n-restrictions
6B. *pqrs* . . . treatments
7A. Partially balanced
—rectangular lattice
—split plot
—split block
7B. Balanced
5B. Other designs
6A. Incomplete latin square
—Youden square
—quasi-latin square
—semi-latin square
6B. Others
—latin squares with split plots
—rotation experiments

The above dichotomous classification indicates the diversity of experimental designs available to the experimenter. There is no one best design for all experiments, but each design was developed to control variation for a particular set of experimental conditions. Thus, the choice of a design depends upon the nature of the material being tested and the variability present.

The chief advantage of randomized designs relative to systematic designs is that they are subject to statistical analyses such as the analysis of variance [127, 273].

I-3 Selection of an Experimental Design

The selection of an experimental design of the randomized nature necessitates knowledge of the variability of the material under test; the selection of the number of replicates to be used is determined by the minimum size of the treatment difference which the experimenter wishes to detect. Before considering the selection of a design, one might investigate ways of controlling the variability of the experimental material or area. Snedecor [273] describes four ways in which variability of treatment means may be reduced:

(i) selection of more homogeneous material,
(ii) stratification of experimental material into homogeneous subgroups,
(iii) increasing the number of observations or replications, and

(iv) measurement of one or more related characteristics in order to use regression techniques.

The selection of homogeneous material may be relatively easy in that the experimental animals, insects, plants, etc. of a particular strain may be relatively homogeneous genetically. The variation among members of the strain or samples of the material may be small relative to the size of treatment differences to be detected. For such material the design listed under 2A of section I-2.2 is suggested.

If it is impractical to obtain a single homogeneous group of material, several small groups of homogeneous material are often available, e.g., several litters of animals in which the litter mates are similar but the litters differ, half leaves or opposite leaves on a plant, pieces of a cake or pie, lengths of a piece of lumber or steel, etc. In such a situation, one of the designs listed under 2B of the previous section is suggested. The complexity of the design is determined to a large extent by the number and nature of treatments and by the sources of variation to be controlled.

The selection of an experimental design is dependent upon the nature of the sources of variation in the experimental area or material. However, if an experimenter is given one guiding principle in selecting a design, it might be to *choose the simplest experimental design possible from a layout and analysis standpoint which allows for adequate control of the variability*. If a design presents procedural difficulties and is costly to analyze *relative to the cost of collecting the data*, a simpler experimental design should be chosen.

One experimental design is said to be more *efficient* than a second if the error variance of a treatment mean is smaller for the first design than for the second design. In computing the efficiency of one design relative to another, the cost as well as the number of degrees of freedom may be considered. A fuller discussion of efficiencies of the various experimental designs is presented in later chapters of the book. Two designs with different numbers of replicates could have the same relative efficiency per dollar spent if the cost of the additional replicates in one design equals the additional cost of analysis and additional experimental procedural costs for the second design, i.e., on a per unit of information basis. The efficiency of one design relative to a second, for fixed cost, is

$$\left(\frac{c_2}{s_1^2}\right)\left(\frac{df_1+1}{df_1+3}\right) \Big/ \left(\frac{c_1}{s_2^2}\right)\left(\frac{df_2+1}{df_2+3}\right), \tag{I-1}$$

where s^2 = error variance per unit of observation, r = number of replicates, c = cost per replicate, df = error degrees of freedom, and the subscripts refer to the first or second experimental design. The correction for the difference in the number of degrees of freedom associated with the two error variances is necessary whenever the variances are estimated [126, sec. 74].

For situations which require a short period of time to obtain the experimental results relative to the time required for the statistical analysis, (i.e., some laboratory experiments), it may prove less costly to use more replicates of a less efficient design with a simple analysis than to use a more efficient design with a more complex analysis. If the unit of observation is very difficult and costly to obtain (e.g., field experiments, animal feeding experiments, rotation experiments, some genetic experiments, etc.) and the cost of analysis is relatively minor, the experimenter would be well advised to control most sources of variation by all the means at his disposal, whether it be a more complex design or (and) the measurement of related variates.

Reduction in the experimental error variance is often accomplished by measuring a related variate or variates and using a covariance analysis. If the cost of measuring the related variate is relatively cheap and if the correlation with the related variate is fairly high, the covariance technique may prove extremely useful. (For example, the initial weight of animals in a feeding experiment is easy to obtain and is related to rate of gain.) A good criterion for the research worker might be: *if the variability of experimental material is not easily controllable, then measure it and use regression techniques to remove the initial variability*. Often both control of variability and measurement of related variates are effective in reducing the experimental error variance.

I-4 Validity and Choice of an Experimental Error

The principles guiding the validity and choice of an error variance have been fully explained by Fisher [126, secs. 9, 10, 11, 65], but will be briefly recapitulated here for completeness. In order to have a valid error term for testing the differences among treatments, an experimental design of the randomized nature and replication of treatments is a necessity. Such a procedure allows for the calculation of unbiased estimates of treatment differences and an appropriate error variance. As stated by Fisher [126, sec. 65] the correct error variance for testing the variation among a set of treatment means is one which contains *all the sources of variation inherent in the variation among treatment means except that portion of the variance due specifically to the treatments themselves*.

These facts are evident from a consideration of the components of variance in the various mean squares in the analysis of variance. To illustrate the effect of randomization on the validity of an error variance, the following example has been prepared.

Example I-1. Suppose that fifty pigs are available for the experiment and that the initial weights [randomly selected samples from 273, table 3.1] for ten random samples of five pigs each are: (30, 29, 39, 17, 12), (19, 42, 27, 25, 22), (16, 41, 37, 31, 25), (17, 30, 24, 28, 35), (47, 33, 17, 33, 29), (17, 23, 31, 39, 30), (41, 26, 19, 32, 27), (20, 28, 39, 43, 30), (38, 20, 30, 46, 36), and (42, 47, 41, 31, 29). The means for the ten samples

are 25.4, 27.0, 30.0, 26.8, 31.8, 28.0, 29.0, 32.0, 34.0, and 38.0, respectively. The analysis of variance is

Source of variation	df	ss	ms
Among sample means	9	661.2	73.5
Individuals within samples	40	3314.8	82.87

Suppose that ten treatments are applied and that three of the treatments have a treatment effect equal to -6, three of the treatments have an effect equal to $+6$, and the remaining 4 treatments have zero effects. The "among sample means" mean square becomes $\frac{1}{9}[661.2 + 5(6)(36)] = 193.5$. The resulting $F = 193.5/82.87 = 2.33$, which is approximately equal to the corresponding tabulated F value at the 3 per cent level. Such a result might lead one to reject the hypothesis of equality of sample means.

On the other hand, let us disturb the random feature of the experiment and use a "balanced" grouping as used in some animal and educational experimentation. The individuals are rearranged in such a way that the means of all ten lots of five pigs are equal to the over-all mean, 30.2. Some such grouping as the following might result: (30, 29, 39, 17, 36), (19, 42, 27, 25, 38), (16, 41, 37, 31, 26), (17, 30, 41, 28, 35), (47, 25, 17, 33, 29), (17, 23, 31, 39, 41), (43, 30, 19, 32, 27), (20, 28, 39, 33, 31), (24, 20, 30, 47, 30), and (42, 12, 22, 46, 29).

For the above ten samples the analysis of variance is

Source of variation	df	ss	ms
Among sample means	9	0	0
Individuals within samples	40	3976	99.4

Now, adding the treatment effects previously obtained results in an F value $= \dfrac{5(216)/9}{99.4} = \dfrac{120.0}{99.4} = 1.21$, which is considerably lower than $F = 2.33$ as found for random samples. If the treatment effects for the three treatments are increased to -8.34 and to $+8.34$ for the other three treatments, then $F = \dfrac{5(6)(8.34)^2}{9(99.4)} = 2.33$. This requires an increase of 2.34, or 39 per cent in the size of the effects in order to detect significance at the 3 per cent level. In addition to the fact that larger effects are required for significance, the procedure is incorrect; fewer type I errors (rejection of null hypothesis when in fact it is true) and more type II errors (acceptance of null hypothesis when it is in fact false) are committed than should have been made when using the proper experimental procedure. Also, the within-sample mean square is larger than it should be.

Another way to observe the disastrous effect of the above erroneous experimental procedure is to observe the additional number of pigs per group that is required to obtain an F value equal to 2.33, i.e., $2.33 = \dfrac{n(216)/9}{99.4}$. Solving, $n = 9.65$ pigs. Roughly twice as many pigs would have been required to detect the same differences among the sample means.

The above computations have been carried out assuming a within-treatment correlation of unity between initial weight and final weight of the animal. If the corre-

lation between initial and final weights were zero, the above "balancing" or "equalizing" of sample or lot means would be meaningless and would not affect the validity of the resulting statistical analyses. The true situation, however, is somewhere between the two extremes, and the experimental procedure of balancing is not recommended when significance tests are later desired. The above balancing violates one of the basic conditions, i.e., randomization, necessary for obtaining a valid error term and hence invalidates the tests of significance. Yates [329] presents a discussion of the above procedure. He shows that the above procedure can be made valid through the use of covariance techniques for certain types of material.

I-5 Symbolism

As with all statistical manuscripts, the present one is subject to the criticism that the author did not use "standard" statistical symbolism. Since the last three words of the preceding sentence may imply something different to each writer in the statistical field, it is perhaps best to list and define the symbols used in the majority of places in the text. In general, Greek letters are used to denote the parameters in the population, and Latin letters or Greek letters with a hat ($\hat{}$) over them are used to denote the fact that these are sample estimates of the parameters.

X_i or Y_i ($i = 1, 2, \cdots, n$ = a specified number) is the symbol for the ith observation. X_1 = first measurement, X_2 = second measurement, etc. X_{ij} or Y_{ij} ($i = 1, 2, \cdots, n$ and $j = 1, 2, \cdots, m$) is the jth measurement in the ith classification.

$\dfrac{\sum_{i=1}^{n} X_i}{n} = \dfrac{X.}{n} = \bar{x}$ = the sample mean of n observations $(X_1, X_2, \cdots, X_n)$ = the sum of the observations, $X_1 + X_2 + \cdots + X_n$, divided by the number n. $\bar{y}$ is obtained similarly. m is sometimes used to denote the sample mean, with m_x denoting the sample mean of the X_i and m_y the sample mean of the Y_i. $\bar{x}$ is an unbiased estimate of the population mean μ, a parameter, if the average value of all X_i in the population is μ.

$\dfrac{1}{nm}\sum_{i=1}^{n}\sum_{j=1}^{m} X_{ij} = \dfrac{X..}{mn} = \bar{x}$ = the sample mean of nm observations.

$\dfrac{1}{m}\sum_{j=1}^{m} X_{ij} = \dfrac{X_{i.}}{m} = \bar{x}_{i.}$ = the sample mean over all $j = 1, 2, \cdots, m$ for the ith classification. Likewise, $\dfrac{1}{n}\sum_{i=1}^{n} X_{ij} = \dfrac{X_{.j}}{n} = \bar{x}_{.j}$.

$(X_i - \bar{x}) = x_i$ = deviation from the sample mean.

$\sum_{i=1}^{n}(X_i - \bar{x})^2 = \sum_{i=1}^{n} x_i^2$ = sum of squares of deviations from the mean.

$\dfrac{\sum_{i=1}^{n} x_i^2}{n-1} = s^2$ = the sample variance (sometimes mean square) of a single obser-

vation and is an unbiased estimate of the population parameter σ^2.

$\dfrac{\sum_{i=1}^{n} x_i^2}{n(n-1)} = s_{\bar{x}}^2 =$ the estimated variance of a mean of n individuals and is an unbiased estimate of the population parameter $\sigma_{\bar{x}}^2$.

$\sqrt{s_{\bar{x}}^2} = s_{\bar{x}} =$ the standard error of a mean or the estimated standard deviation of means of n observations.

$\sqrt{\dfrac{s_1^2}{n_1} + \dfrac{s_2^2}{n_2}} = s_{\bar{x}_1-\bar{x}_2} = s_{\bar{d}} =$ the standard error of a difference between two independently estimated means based on n_1 and n_2 observations, respectively. In the event that $n_1 = n_2$, $s\sqrt{\dfrac{2}{n}} = \sqrt{2}\, s_{\bar{x}} = s_{\bar{d}}$.

$b_{12} =$ regression coefficient of the variate X_{1i} on the variate X_{2i} and is an unbiased estimate of the population parameter β_{12}. Other symbols will be used to designate regression coefficients, but they will be explained when used.

$b_{12.3} =$ partial regression coefficient of X_{1i} on X_{2i} independent of X_{3i}.

$r_{12} =$ total or zero-order correlation coefficient of X_{1i} with X_{2i} and is an estimate (though biased) of the population parameter, ρ.

$cv =$ coefficient of variation or variation coefficient.

$df =$ degrees of freedom.

$ss =$ sum of squares.

$ms =$ mean square.

E_a, E_b, E_c, E_e, etc. = error mean squares.

$Eff =$ efficiency expressed as percentage.

$F = \dfrac{ms_1}{ms_2} =$ Snedecor's F is used as a test of significance.[1]

$t =$ "Student's" t is used as a test of significance.[1]

$\chi^2 =$ chi-square is used as a test of significance.[1]

$z = \frac{1}{2} \log_e F =$ Fisher's z is used as a test of significance.[1]

$t_{\alpha,ndf} =$ tabulated value of t at the α per cent level for n degrees of freedom associated with the standard error.

$F_\alpha(n_1, n_2df) =$ tabulated value of F at the α per cent point with n_1 degrees of freedom associated with the numerator and n_2 degrees of freedom associated with the denominator.

$\chi_\alpha^2(ndf) =$ tabulated value of χ^2 at the α per cent level for n degrees of freedom.

$<$ is less than.

$>$ is greater than.

$\geq$ is greater than or equal to.

$E[X] =$ average value over all possible samples of a given size or the expected value of the quantity X in the population.

[1]The statistics F, t, χ^2, and z are random variables with known distributions. Tables for these distribution functions have been prepared and are used in making tests of significance (see Chapter II).

CHAPTER II

Some Useful Statistical Tools and Concepts

Statistical procedures and concepts are useful in the analysis of data and in the interpretation of the results from an experiment. A number of useful statistical tools—tests of significance for comparisons among a set of ranked means, transformations for experimental data, tests of significance for homogeneity of variances, tests for additivity of data, nonparametric tests in the analysis of variance, tables of probability levels for the range and F, and a figure of probability levels of t and χ^2—are presented in the following sections. Additional methods are described and illustrated in statistics textbooks[75, 87, 102, 127, 137, 179, 212, 273].

II-1 Tests of Significance for a Group of Ranked Means

In experiments involving more than two treatments, the resulting $F =$ treatment mean square/error mean square may be larger than the corresponding tabulated F value at a previously chosen level of significance, say $F_{.05}$. The experimenter, lacking other evidence, then rejects the null hypothesis and is confronted with the problem of deciding which means are different. Several tests for making comparisons within a set of means have appeared in statistical literature. The test procedure and the merits of several of these tests are discussed to some extent in the following sections. All of the tests discussed assume normality. Some of the procedures test different hypotheses; the alternative hypotheses and the objectives of the tests may differ. The "goodness" of the tests as measured by the relative power of the tests may vary depending upon the alternative hypotheses being considered. Tukey [296] and Duncan [96, 97] have discussed the various tests and their properties.

Before discussing the tests, it is emphatically stressed that *prior to employing any of the following tests the treatment comparisons must be partitioned into all logical and meaningful comparisons.* Then, the means within each subgroup are compared. The problem of making individual comparisons in the anaylsis of variance is easily resolved if the $n - 1$ degrees of freedom in the analysis of variance can be partitioned into $n - 1$ independent single degree of freedom contrasts in a meaningful manner; i.e., there must be a biological, physical, or

sociological reason for the comparisons.[1] In some problems, there is no meaningful reason for partitioning the $n - 1$ degrees of freedom associated with comparisons among n means. In such situations, we may, and quite often do, wish to know which means may be considered as different from the others. Formulated in terms of possible outcomes, this represents the total number of decisions one might make about a set of n means. For three treatments the possible decisions about the population means are

(1) $\mu_1 = \mu_2 = \mu_3$.
(2) $\mu_1 <$ (μ_2 is not appreciably different from μ_3).
(3) $\mu_2 <$ (μ_1 " " " " " μ_3).
(4) $\mu_3 <$ (μ_1 " " " " " μ_2).
(5) (μ_1 is not appreciably different from μ_2) $< \mu_3$.
(6) (μ_1 " " " " " μ_3) $< \mu_2$.
(7) (μ_2 " " " " " μ_3) $< \mu_1$.
(8) $\mu_1 < \mu_2 < \mu_3$.
(9) $\mu_1 < \mu_3 < \mu_2$.
(10) $\mu_2 < \mu_1 < \mu_3$.
(11) $\mu_2 < \mu_3 < \mu_1$.
(12) $\mu_3 < \mu_1 < \mu_2$.
(13) $\mu_3 < \mu_2 < \mu_1$.
(14) $\mu_1 < \mu_2$ but μ_3 cannot be ranked relative to μ_1 or μ_2.
(15) $\mu_2 < \mu_1$ but μ_3 " " " " " μ_1 or μ_2.
(16) $\mu_1 < \mu_3$ but μ_2 " " " " " μ_1 or μ_3.
(17) $\mu_3 < \mu_1$ but μ_2 " " " " " μ_1 or μ_3.
(18) $\mu_2 < \mu_3$ but μ_1 " " " " " μ_2 or μ_3.
(19) $\mu_3 < \mu_2$ but μ_1 " " " " " μ_2 or μ_3.

Also, decisions of the form $\mu_1 + \mu_2 < 2\mu_3$ are possible. The number of other comparisons possible is determined by the nature of the experimental material.

Fisher's z test or Snedecor's F test may or may not reject decision (1). If (1) is rejected, it may be because one or more of the other possible decisions is the correct one, and the problem is to find which one or ones to accept.

Although these tests involve a very large number of decisions, it is convenient, from the point of view of understanding their properties, to regard them as simultaneous applications of a relatively small number of two-decision tests [96, 97]. For example, a multiple range test involving four means,

[1]This does not necessarily imply that comparisons between members of different subgroups are ruled out. The experimenter, not the statistician, should determine whether or not such comparisons are meaningful.

say A, B, C, D, may be regarded as the joint application of two-decision tests of each of the following hypotheses:

(i) all four means are equal,
(ii) the three means, A, B, and C, are equal,
(iii) " " " , A, B, " D, " " ,
(iv) " " " , A, C, " D, " " ,
(v) " " " , B, C, " D, " " ,
(vi) the two means, A and B, are equal,

(xi) the two means, C and D, are equal.

Any one of the multiple decision tests can be said to have a type I error (as defined in Chapter I) with respect to each of the homogeneity hypotheses. In testing the four means, there is a type I error corresponding to each of the eleven different homogeneity tests of which the over-all test is comprised. To distinguish between these type I errors, it is convenient to refer to

(i) the error of wrongly rejecting the hypothesis that all means are homogeneous as a *four-treatment type I error*,
(ii) the error of wrongly rejecting the hypothesis that any three means are homogeneous as a *three-treatment type I error*, and
(iii) the error of wrongly rejecting the hypothesis that any two means are homogeneous as a *two-treatment type I error.*

In general, then, the *n-treatment type I error* refers to the error of wrongly rejecting the hypothesis that n treatments are homogeneous [96, 97].

In the following sections the aggregate of the two-treatment, three-treatment, $\cdots$, and v-treatment type I errors is referred to as the "type I error." The quotes are used to distinguish this error from the one involving only two decisions.

The discussion on the kinds of errors committed is necessarily limited in the present text. For a fuller understanding of the various errors the reader is referred to the works of Duncan [96, 97] and Tukey [296].

II-1.1 MULTIPLE RANGE TESTS

II-1.1.1 The *lsd* test or the multiple *t* test. A commonly used procedure for comparing the differences among a set of means and for comparing each one of a set of means with a standard treatment is the least significant difference, or the *lsd* test. This procedure is an outgrowth of discussions by Fisher [126, sec. 24]. The *lsd* is equal to the product of the standard error of the mean, the $\sqrt{2}$, and the value of t at the 5 per cent level for the number of degrees of freedom, f, associated with the standard error of the mean. Symbolically the *lsd* is $s_{\bar{x}}\sqrt{2}\, t_{05,fdf} = s_{\bar{d}} t_{05,fdf}$. All differences of means are compared

with the *lsd*, and if the difference exceeds the *lsd*, the means are said to come from populations with different means. The 1 per cent value of t is sometimes used instead of t_{05}. The value of the quantity $s_{\bar{x}}\sqrt{2}\, t_{01,fdf}$ is called the *msd*, or the most significant difference. Occasionally the *lsd* is called the minimum significant difference. A further variation of the multiple t test at the α per cent level is the use of an F or z test first, and if this shows significance, then a multiple t test is performed on the differences among the treatment means. This test is discussed by Fisher [126, sec. 24] and in section II-1.2.1.

In connection with these tests, Fisher [126, sec. 24] has stated that the $v - 1$ degrees of freedom among a set of v treatments may be partitioned into $v - 1$ orthogonal comparisons, each with a single degree of freedom, and that it is possible to obtain an estimate of the error variance for each single degree of freedom comparison in a number of the experimental designs. The difficulty here is that the individual error variances usually are associated with a small number of degrees of freedom; because of this, the pooled error variance with a larger number of degrees of freedom is used whenever possible. Also, more than the $v - 1$ orthogonal comparisons are usually desired.

The *lsd* test is appropriate if the comparison is selected prior to conducting the experiment. However, if the comparison is selected after the treatment means are observed, a certain number of the differences will be large, owing to sampling variation. The most extreme case is the comparison of the largest mean with the smallest mean in a set of v treatment means. In this case, Pearson and Hartley [243, 244] have shown that the v treatment type I error is not 5 per cent for $v > 2$ means but is some larger value. The size of a v-treatment type I error is equal to $1 - {}_fP_n(Q)$, where ${}_fP_n(Q)$ is obtained from the following formula:

$$ {}_fP_n(Q) = P_n(Q) + \frac{1}{f}a_n(Q) + \frac{1}{f^2}b_n(Q), \tag{II-1} $$

where $P_n(Q)$, $a_n(Q)$, and $b_n(Q)$ are obtained from table 1 in Pearson and Hartley's paper [244], $n = v$ = number of treatments in the group, f = degrees of freedom associated with the error variance, and $Q = \sqrt{2}\, t_{\alpha,fdf}$.

For more than two treatments in a group the size of the v-treatment type I error associated with the comparison of the largest mean with the smallest mean is larger than 5 per cent. For $f = 40$ and for $t_{05}\sqrt{2} = 2.86$ the v-treatment type I error associated with the comparison of the largest with the smallest mean is approximately equal to 27 per cent for five treatments, 59 per cent for ten treatments, and 86 per cent for twenty treatments. Thus, the *lsd* procedure for comparing the largest and smallest means should not be used with more than two treatments in the experiment.

II-1.1.2 Student-Newman-Keuls test. In 1927, Student [283] suggested that use be made of the range as a procedure for rejecting divergent samples in a series of routine analyses. Newman [231] made use of Student's

TABLE II-1. Upper percentage points of the studentized range $q_\alpha = \dfrac{\bar{x}_{maximum} - \bar{x}_{minimum}}{s_{\bar{x}}}$ *

$\alpha = .05$

df \ n	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
10	3.15	3.88	4.33	4.66	4.91	5.12	5.30	5.46	5.60	5.72	5.83	5.93	6.03	6.12	6.20	6.27	6.34	6.41	6.47
11	3.11	3.82	4.26	4.58	4.82	5.03	5.20	5.35	5.49	5.61	5.71	5.81	5.90	5.98	6.06	6.14	6.20	6.27	6.33
12	3.08	3.77	4.20	4.51	4.75	4.95	5.12	5.27	5.40	5.51	5.61	5.71	5.80	5.88	5.95	6.02	6.09	6.15	6.21
13	3.06	3.73	4.15	4.46	4.69	4.88	5.05	5.19	5.32	5.43	5.53	5.63	5.71	5.79	5.86	5.93	6.00	6.06	6.11
14	3.03	3.70	4.11	4.41	4.64	4.83	4.99	5.13	5.25	5.36	5.46	5.56	5.64	5.72	5.79	5.86	5.92	5.98	6.03
15	3.01	3.67	4.08	4.37	4.59	4.78	4.94	5.08	5.20	5.31	5.40	5.49	5.57	5.65	5.72	5.79	5.85	5.91	5.96
16	3.00	3.65	4.05	4.34	4.56	4.74	4.90	5.03	5.15	5.26	5.35	5.44	5.52	5.59	5.66	5.73	5.79	5.84	5.90
17	2.98	3.62	4.02	4.31	4.52	4.70	4.86	4.99	5.11	5.21	5.31	5.39	5.47	5.55	5.61	5.68	5.74	5.79	5.84
18	2.97	3.61	4.00	4.28	4.49	4.67	4.83	4.96	5.07	5.17	5.27	5.35	5.43	5.50	5.57	5.63	5.69	5.74	5.79
19	2.96	3.59	3.98	4.26	4.47	4.64	4.79	4.92	5.04	5.14	5.23	5.32	5.39	5.46	5.53	5.59	5.65	5.70	5.75
20	2.95	3.58	3.96	4.24	4.45	4.62	4.77	4.90	5.01	5.11	5.20	5.28	5.36	5.43	5.50	5.56	5.61	5.66	5.71
24	2.92	3.53	3.90	4.17	4.37	4.54	4.68	4.81	4.92	5.01	5.10	5.18	5.25	5.32	5.38	5.44	5.50	5.55	5.59
30	2.89	3.48	3.84	4.11	4.30	4.46	4.60	4.72	4.83	4.92	5.00	5.08	5.15	5.21	5.27	5.33	5.38	5.43	5.48
40	2.86	3.44	3.79	4.04	4.23	4.39	4.52	4.63	4.74	4.82	4.90	4.98	5.05	5.11	5.17	5.22	5.27	5.32	5.36
60	2.83	3.40	3.74	3.98	4.16	4.31	4.44	4.55	4.65	4.73	4.81	4.88	4.94	5.00	5.06	5.11	5.15	5.20	5.24
120	2.80	3.36	3.69	3.92	4.10	4.24	4.36	4.47	4.56	4.64	4.71	4.78	4.84	4.90	4.95	5.00	5.04	5.09	5.13
∞	2.77	3.32	3.63	3.86	4.03	4.17	4.29	4.39	4.47	4.55	4.62	4.68	4.74	4.80	4.84	4.89	4.93	4.97	5.01

$\alpha = .01$

df \ n	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
10	4.48	5.26	5.77	6.14	6.43	6.67	6.88	7.06	7.22	7.36	7.49	7.60	7.71	7.81	7.91	7.99	8.08	8.15	8.23
11	4.39	5.14	5.62	5.98	6.25	6.47	6.67	6.84	6.99	7.13	7.25	7.36	7.46	7.56	7.65	7.73	7.81	7.88	7.95
12	4.32	5.04	5.50	5.84	6.10	6.32	6.51	6.67	6.81	6.94	7.06	7.17	7.26	7.36	7.44	7.52	7.60	7.67	7.73
13	4.26	4.96	5.40	5.73	5.98	6.19	6.37	6.53	6.67	6.79	6.90	7.01	7.10	7.19	7.27	7.35	7.42	7.49	7.55
14	4.21	4.89	5.32	5.64	5.88	6.08	6.26	6.41	6.54	6.66	6.77	6.87	6.96	7.05	7.13	7.20	7.27	7.34	7.40
15	4.17	4.83	5.25	5.56	5.80	5.99	6.16	6.31	6.44	6.55	6.66	6.76	6.85	6.93	7.00	7.07	7.14	7.20	7.26
16	4.13	4.78	5.19	5.49	5.72	5.91	6.08	6.22	6.35	6.46	6.56	6.66	6.74	6.82	6.90	6.97	7.03	7.09	7.15
17	4.10	4.73	5.14	5.43	5.66	5.85	6.01	6.15	6.27	6.38	6.48	6.57	6.66	6.73	6.81	6.87	6.94	7.00	7.05
18	4.07	4.70	5.09	5.38	5.60	5.79	5.95	6.08	6.20	6.31	6.41	6.50	6.58	6.65	6.73	6.79	6.85	6.91	6.97
19	4.05	4.66	5.05	5.34	5.55	5.73	5.89	6.02	6.14	6.25	6.34	6.43	6.51	6.58	6.65	6.72	6.78	6.84	6.89
20	4.02	4.63	5.02	5.30	5.51	5.69	5.84	5.97	6.09	6.19	6.28	6.37	6.45	6.52	6.59	6.66	6.71	6.77	6.82
24	3.96	4.54	4.91	5.17	5.37	5.54	5.69	5.81	5.92	6.02	6.11	6.19	6.26	6.33	6.39	6.45	6.51	6.57	6.61
30	3.89	4.45	4.80	5.05	5.24	5.40	5.53	5.65	5.76	5.85	5.93	6.01	6.08	6.14	6.20	6.26	6.31	6.36	6.41
40	3.82	4.36	4.70	4.93	5.11	5.26	5.39	5.50	5.60	5.69	5.77	5.84	5.90	5.96	6.02	6.07	6.12	6.17	6.21
60	3.76	4.28	4.60	4.82	4.99	5.13	5.25	5.36	5.45	5.53	5.60	5.67	5.73	5.78	5.83	5.88	5.93	5.98	6.01
120	3.70	4.20	4.50	4.71	4.87	5.00	5.12	5.21	5.30	5.38	5.44	5.50	5.56	5.61	5.66	5.71	5.75	5.79	5.83
∞	3.64	4.12	4.40	4.60	4.76	4.88	4.99	5.08	5.16	5.23	5.29	5.35	5.40	5.45	5.49	5.53	5.57	5.61	5.64

*This table was reproduced with the permission of the Biometrika Trustees from the paper by May, J. M., "Extended and corrected tables of the upper percentage points of the 'studentized' range," *Biometrika* 39: 192-193, 1952. An improved version of this table may be found in table 29 of *Biometrika tables for statisticians*, Vol. 1, Cambridge University Press, 1954, by E. S. Pearson and H. O. Hartley.

[231, 283] ideas for subdividing a group of ranked treatment means into subgroups asserted to be not heterogeneous and presented tables for a number of percentage points of q for

$$q_{\alpha.n} = \frac{\bar{x}_{\max} - \bar{x}_{\min.}}{s_{\bar{x}}} = \frac{\text{range}}{\text{standard deviation}},$$

where α = the percentage level of q for n treatments, $\bar{x}_{\max.}$ = the largest mean in the group, $\bar{x}_{\min.}$ = the smallest mean in the group, and $s_{\bar{x}} = \frac{s}{\sqrt{r}}$ is an independent estimate (such as is obtained from a randomized design) of the standard error for each mean. The means must be obtained from the same number of observations, say r, and all standard errors must be equal.

Newman's table was computed by quadrature from E. S. Pearson's approximate probability law of the studentized range. Later, Pearson and Hartley [244] computed a table similar to the one obtained by Newman. The tables were computed by an approximate method. May [208] recomputed their table [244] in order to obtain results correct to more significant figures. This table is reproduced in table II-1. The last lines (∞ *df*) in the two parts of table II-1 correspond to the values given in table 5.5 of Snedecor's book [273] and to the values used by Student [283]. The first column ($n = 2$) may be obtained by multiplying the t_α values for the corresponding degrees of freedom by $\sqrt{2}$. The table may be extended for other values of α and degrees of freedom in the manner described by May [208]. It is desirable to have not less than ten degrees of freedom, and preferably more, associated with the independent estimate of the standard error. Therefore, the part of May's table relating to degrees of freedom less than 10 was omitted.

In applying the Student-Newman-Keuls multiple-range test (so called because each contributed to its formation [180, 231, 283]), the following steps are taken:

Step (I). Subdivide the treatment means into biological, physical, or sociological groups. Natural groupings as prescribed by the choice of the particular set of treatments have meaning; it is doubtful if a ranked set of means from two or more natural groupings has any practical significance.

Step (II). Choose a significance level, α, which usually will be the 5 or 1 per cent level.

Step (III). Compute the standard error of a treatment mean, $s_{\bar{x}}$, and the values $W_n = q_{\alpha.n}s_{\bar{x}}$, $W_{n-1} = q_{\alpha.n-1}s_{\bar{x}}, \cdots$, $W_3 = q_{\alpha.3}s_{\bar{x}}$, and $W_2 = q_{\alpha.2}s_{\bar{x}} = t_{\alpha.fdf}\sqrt{2}\, s_{\bar{x}} = lsd$. Rank the treatment means from highest to lowest, $\bar{x}_n, \bar{x}_{n-1}, \cdots, \bar{x}_2, \bar{x}_1$.

Step (IV). Compare the range of n treatments, $\bar{x}_n - \bar{x}_1$, with the calculated W_n. If $\bar{x}_n - \bar{x}_1$ is less than W_n, the process stops, and the n means are asserted to belong to a non-heterogeneous group. If $\bar{x}_n - \bar{x}_1 > W_n$ subdivide the means into two groups of $n - 1$ means each, $\bar{x}_n$ to $\bar{x}_2$ and $\bar{x}_{n-1}$ to $\bar{x}_1$, and state that $\bar{x}_n$ is different from $\bar{x}_1$. Then, compare the ranges $\bar{x}_n - \bar{x}_2$ and $\bar{x}_{n-1} - \bar{x}_1$

with W_{n-1}. If either range is less than W_{n-1} the means in the group are said to belong to a single group. If either range exceeds W_{n-1} the $n - 1$ means are divided into two groups of $n - 2$ means each and compared with W_{n-2}. The process continues until a subset of means is obtained which does not exceed the calculated value W_i. The process stops whenever the actual range of a subset is less than the calculated range. No subset of means is compared if the subset is included in a larger subset which is less than the calculated range W_i.

The above procedure has been stated in more concise terms by Duncan [97] as follows: "The difference between any two means in a set of n means is significant provided the range of each and every subset which contains the given means is significant according to an α-level range test."

Care should be employed in making comparisons within a subset of means after some observations have been rejected [231]. It is possible to obtain a significant F value on a subset of means whose range is less than the computed W_i. The Student-Newman-Keuls multiple range test compares the range of means in the subset with the calculated range at the α per cent level. The F test compares the variance for the subset means with the error variance. Also the error rate per experiment is different for the two procedures [96, 97, 296]. One or more of the single degree of freedom contrasts from the subset of means could give a relatively large F value, whereas the observed range might be less than the calculated range. An example of this situation is encountered when the means in the subset fall into two groups. However, in this case there probably are two natural groupings, and this should be taken into account before beginning the test. If the grouping in the subset of means is not known, then one of the multiple comparisons tests described in the next section may be used to determine which contrast among the means accounts for the significance of the variance ratio test.

Example II-1. Snedecor [273, table 10.3] presents the data from an experiment on grams of fat absorbed by twenty-four doughnuts for eight different fats. The eight means are presented in table II-2. The independently estimated standard error of a mean is equal to $s_{\bar{x}} = \sqrt{141.6/6} = 4.86$, with 40 degrees of freedom.

Step (I). No information is given concerning the relationship among the various fats. Some of the fats may be vegetable fats, others may be animal fats, and the method of processing may have varied. However, with no further information the fats are considered to be a single natural group.

Step (II). The $\alpha = 5$ per cent level of significance is chosen.

Step (III). From table II-1, the values of $q_{05,n}s_{\bar{x}}$ = calculated range for a homogeneous population = W_n for $n = 8, 7, 6, 5, 4, 3$, and 2 and for $f = 40$ degrees of freedom are

$$\begin{aligned} W_{n=8} &= 4.52\ (4.86) = 22.0, \\ W_{n=7} &= 4.39\ (4.86) = 21.3, \\ W_{n=6} &= 4.23\ (4.86) = 20.6, \\ W_{n=5} &= 4.04\ (4.86) \doteq 19.6, \end{aligned}$$

$$W_{n-4} = 3.79\ (4.86) = 18.4,$$
$$W_{n-5} = 3.44\ (4.86) = 16.7, \text{ and}$$
$$W_{n-6} = 2.86\ (4.86) = 13.9 = lsd.$$

TABLE II-2. Differences of fat means (grams of fat absorbed in 24 doughnuts)

Fat no.	Fat no. →	4	3	2	6	1	5	8
	Mean →	185	182	178	176	172	165	162
Fat no.	**Mean**							
7	161	24	21	17	15	11	4	1
8	162	23	20	16	14	10	3	x
5	165	20	17	13	11	7	x	x
1	172	13	10	6	4	x	x	x
6	176	9	6	2	x	x	x	x
2	178	7	4	x	x	x	x	x
3	182	3	x	x	x	x	x	x

The ranked means and all possible differences among the eight means are presented in table II-2. Only a part of the table may actually be computed in practice.

Step (IV). The range of the largest and smallest means exceeds the calculated range; thus:

$$\bar{x}_4 - \bar{x}_7 = 185 - 161 = 24 > W_{n-8} = 22.0.$$

Therefore, we compare the ranges for the two subsets of seven means with the calculated range; thus:

$$\bar{x}_4 - \bar{x}_8 = 185 - 162 = 23 > W_{n-7} = 21.3.$$
$$\bar{x}_3 - \bar{x}_7 = 182 - 161 = 21 < W_{n-7} = 21.3.$$

Only the first subset of seven means exceeds the calculated range. Therefore, we subdivide the seven means into two subsets of six means each and compare their range with the calculated range. The subset of six means including $\bar{x}_3$ and $\bar{x}_8$ is included in the larger group $\bar{x}_3$ to $\bar{x}_7$, which has already been declared nonsignificant. The range for the subset of means for fat numbers 4, 3, 2, 6, 1, 5 is compared with the calculated range; thus:

$$\bar{x}_4 - \bar{x}_5 = 185 - 165 = 20 < W_{n-6} = 20.6.$$

The process terminates, since the range is less than the calculated range.

From the above test procedure the experimenter would conclude that significantly more fat is absorbed for fat number 4 than for fat numbers 7, 8, and probably 5. Likewise, fat number 3 is absorbed to a higher degree than are fat numbers 7 and probably 8.

II-1.1.3 Duncan's multiple range test. Duncan [96, 97] proposed a multiple range test which combines the simplicity of the Student-Newman-Keuls test and the power advantages of the multiple comparisons test described in section II-1.2.2. He feels that the proposed test is better than previously proposed tests for comparing differences between means. The test

allows the experimenter to commit fewer "type II errors" and more "type I errors" than the test discussed in the preceding section.

The test procedure is essentially the same as for the Student-Newman-Keuls test except that the values in table II-3 are used instead of those in table II-1. The various steps in the test are illustrated in the following example.

Example II-2. The data from example II-1 and table II-2 are used to illustrate the calculations for Duncan's multiple range test.

Step (I). There appear to be no natural groupings.

Step (II). For the present example, choose the significance level $\alpha = 5$ per cent.

Step (III). The standard error of the mean is $s_{\bar{x}} = \sqrt{141.6/6} = 4.86$, with 40 degrees of freedom. The calculated ranges (Duncan denotes these as least significant ranges) are computed as follows:

$$\begin{aligned}
D_{n-8} &= 3.30\ (4.86) = 16.0,\\
D_{n-7} &= 3.27\ (4.86) = 15.9,\\
D_{n-6} &= 3.22\ (4.86) = 15.6,\\
D_{n-5} &= 3.17\ (4.86) = 15.4,\\
D_{n-4} &= 3.10\ (4.86) = 15.1,\\
D_{n-3} &= 3.01\ (4.86) = 14.6, \text{ and}\\
D_{n-2} &= 2.86\ (4.86) = 13.9.
\end{aligned}$$

The values 3.30, 3.27, $\cdots$, 2.86 are obtained from Duncan's tables [96] or by interpolation from table II-3.

Step (IV). The ranked means and the differences between the means are given in table II-2. The range of the largest and smallest means is compared with the calculated range; thus:

$$\bar{x}_4 - \bar{x}_7 = 185 - 161 = 24 > D_{n-8} = 16.0.$$

Then,

$$\begin{aligned}
\bar{x}_4 - \bar{x}_8 &= 185 - 162 = 23 > D_{n-7} = 15.9 \qquad \text{and}\\
\bar{x}_3 - \bar{x}_7 &= 182 - 161 = 21 > D_{n-7} = 15.9.
\end{aligned}$$

The next step is to compare subsets of six means with the calculated ranges; thus:

$$\begin{cases} \bar{x}_4 - \bar{x}_5 = 185 - 165 = 20 > D_{n-6} = 15.6.\\ \bar{x}_3 - \bar{x}_8 = 182 - 162 = 20 > D_{n-6} = 15.6. \end{cases}$$

$$\begin{cases} \bar{x}_3 - \bar{x}_8 = \text{see above.}\\ \bar{x}_2 - \bar{x}_7 = 178 - 161 = 17 > D_{n-6} = 15.6. \end{cases}$$

The next step is to compare subsets of five means with the calculated ranges; thus:

$$\begin{aligned}
\bar{x}_4 - \bar{x}_1 &= 185 - 172 = 13 < D_{n-5} = 15.4.\\
\bar{x}_3 - \bar{x}_5 &= 182 - 165 = 17 > D_{n-5} = 15.4.
\end{aligned}$$

$$\begin{cases} \bar{x}_3 - \bar{x}_5 = \text{see above.}\\ \bar{x}_2 - \bar{x}_8 = 178 - 162 = 16 > D_{n-5} = 15.4. \end{cases}$$

$$\begin{cases} \bar{x}_2 - \bar{x}_8 = \text{see above.}\\ \bar{x}_6 - \quad = 176 - 161 = 15 < D_{n-5} = 15.4. \end{cases}$$

TABLE II-3. Significant ranges for a 5% level new[a] multiple range test[*]

n_2 \ p	2	3	4	5	6	8	10	14	20	50	100
10	3.15	3.29	3.37	3.43	3.46	3.47	3.47	3.47	3.48	3.48	3.48
12	3.08	3.23	3.33	3.36	3.40	3.44	3.46	3.46	3.48	3.48	3.48
14	3.03	3.18	3.27	3.33	3.37	3.41	3.44	3.46	3.47	3.47	3.47
16	3.00	3.15	3.23	3.30	3.34	3.39	3.43	3.45	3.47	3.47	3.47
18	2.97	3.12	3.21	3.27	3.32	3.37	3.41	3.45	3.47	3.47	3.47
20	2.95	3.10	3.18	3.25	3.30	3.36	3.40	3.44	3.47	3.47	3.47
24	2.92	3.07	3.15	3.22	3.28	3.34	3.38	3.44	3.47	3.47	3.47
30	2.89	3.04	3.12	3.20	3.25	3.32	3.37	3.43	3.47	3.47	3.47
60	2.83	2.98	3.08	3.14	3.20	3.28	3.33	3.40	3.47	3.48	3.48
100	2.80	2.95	3.05	3.12	3.18	3.26	3.32	3.40	3.47	3.53	3.53
∞	2.77	2.92	3.02	3.09	3.15	3.23	3.29	3.38	3.47	3.61	3.67

Significant ranges for a 1% level new[a] multiple range test

n_2 \ p	2	3	4	5	6	8	10	14	20	50	100
10	4.48	4.73	4.88	4.96	5.06	5.20	5.28	5.42	5.55	5.55	5.55
12	4.32	4.55	4.68	4.76	4.84	4.96	5.07	5.17	5.26	5.26	5.26
14	4.21	4.42	4.55	4.63	4.70	4.83	4.91	5.00	5.07	5.07	5.07
16	4.13	4.34	4.45	4.54	4.60	4.72	4.79	4.88	4.94	4.94	4.94
18	4.07	4.27	4.38	4.46	4.53	4.64	4.71	4.79	4.85	4.85	4.85
20	4.02	4.22	4.33	4.40	4.47	4.58	4.65	4.73	4.79	4.79	4.79
24	3.96	4.14	4.24	4.33	4.39	4.49	4.57	4.64	4.72	4.74	4.74
30	3.89	4.06	4.16	4.25	4.32	4.41	4.48	4.58	4.65	4.72	4.72
60	3.76	3.92	4.03	4.12	4.17	4.27	4.34	4.44	4.53	4.66	4.66
100	3.71	3.86	3.98	4.06	4.11	4.21	4.29	4.38	4.48	4.64	4.65
∞	3.64	3.80	3.90	3.98	4.04	4.14	4.20	4.31	4.41	4.60	4.68

[a]Using special protection levels based on degrees of freedom.

[*]This table was reproduced with the permission of the editor of *Biometrics* from the paper by D. B. Duncan, *Biometrics* **11**:1-42, 1955.

Since the means for fat numbers 3, 2, 6, and 1 are included in the larger subset 43261, which was declared nonsignificant, the range of means in the subset 3261 is not compared with D_{n-4}; only the range of means in subset 2615 from the subset 32615 is compared with D_{n-4}; thus:

$$\bar{x}_2 - \bar{x}_5 = 178 - 165 = 13 < D_{n-4} = 15.1.$$

In the next subset of means (26158) greater than the calculated range the two subsets of four means each are 2615 and 6158. Both subsets have already been compared with the calculated range in larger subsets. Therefore, the process terminates.

The experimenter would conclude that the amount of fat absorbed with fat numbers 4 and 3 is significantly higher than for fat numbers 5, 8, and 7, and the amount of fat absorbed with fat number 2 is significantly higher than for fat numbers 7 and 8. The remainder of the fats are said not to differ with respect to the amount of fat absorbed. The results may be pictured graphically as in figure II-1.

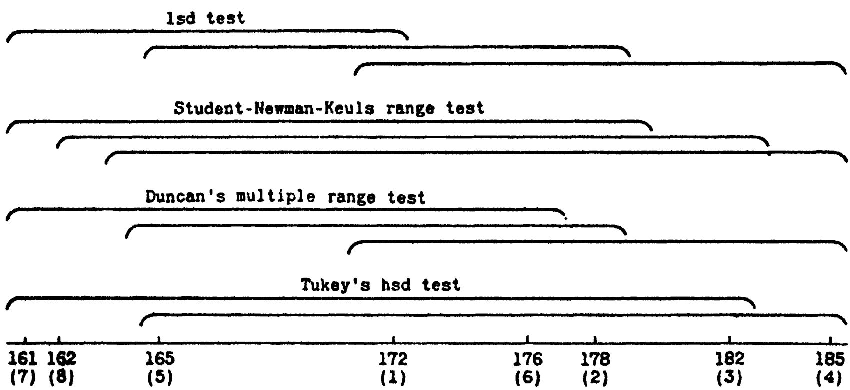


Figure II-1. Graphical array of grams of fat absorbed for the eight fats in example II-1. The above bracketing is possible at the conclusion of various multiple range tests. The means within a bracket are asserted to be not heterogeneous, and means not bracketed together are asserted to be different.

II-1.1.4 Tukey's test based on allowances. A multiple range test similar in application to the *lsd* test has been proposed by Tukey [295, 296]. He discusses several variations of the test, but only one of the procedures—the *hsd* (honestly significant difference) test, or the *w*-procedure—has been selected to illustrate his method. Using the *hsd* test and testing at the α per cent level, the number of experiments in which misstatements are made is α per cent out of all experiments. The v-treatment type I error is set at α per cent, and the two-treatment, the three-treatment, $\cdots$, $(v - 1)$-treatment type I errors are less than α per cent. In Tukey's other tests the percentage of error may be set per line in the analysis of variance, per set of experiments, etc. [296].

The value for the *hsd* is equal to the value of W_{n-}, obtained in the Student-Newman-Keuls multiple range test. If two means (or groups of means) differ

by more than the *hsd*, they are said to differ. Also, the *hsd* may be used to set a confidence interval on the difference between any two means. Tukey [296] says that it may be more important to state that the true difference between two means lies in a specified interval than to state that the means (or groups of means) differ significantly. A confidence statement always contains a significance statement, but the converse is not true.

Example II-3. The procedure of applying Tukey's *hsd* test is illustrated with the data from example II-1. Steps I and II of the *hsd* test are the same as steps I and II of the Student-Newman-Keuls test. The third step is to compute the *hsd* value, which is equal to

$$W_{n-\nu} = q_{\alpha.\nu}s_{\bar{x}} = 4.52\ (4.86) = 22.0 = hsd.$$

The difference between any two treatment means (table II-2) is compared with the *hsd*. If the difference exceeds this value, the two means are asserted to come from populations with different means. Two differences, $\bar{x}_4 - \bar{x}_7 = 185 - 161 = 24$ and $\bar{x}_4 - \bar{x}_8 = 185 - 162 = 23$, exceed the *hsd*. Therefore, it is stated that fat number 4 is absorbed to a greater degree than are fat numbers 7 and 8. Also, the true difference of means $\bar{x}_4$ and $\bar{x}_7$ is asserted to lie in the interval $(\bar{x}_4 - \bar{x}_7) \pm hsd = 24 \pm 22.0$, and the true difference between fats 6 and 7 is asserted to lie within the interval $15 \pm 22.0 = -7.0$ to 37.0. This is the 95 per cent confidence interval. The test is completed with the significance statements about the differences and with the calculation of the confidence intervals.

II-1.1.5 Short cut to allowances. The analytical procedure for the *hsd* test may be considerably simplified by using the sum of the ranges rather than the standard error of a treatment mean. The procedure developed by Link and Wallace [195] involves the calculation of ranges rather than of sums of squares. Different procedures are utilized for one-way (the completely randomized design) and for two-way (the randomized complete block design) classifications. The procedure for the one-way grouping is illustrated in the following example. The test procedure for two-way classifications is illustrated by Link and Wallace [195].

Example II-4. The data of example II-1 are used to illustrate the Link and Wallace short cut procedure. From the original data [273, table 10.3] the ranges in amount of fat absorbed for the six observations on each of the eight fats are

Fat No.	1	2	3	4	5	6	7	8	Sum
Range	39	36	28	20	33	30	41	21	248
Mean	172	178	182	185	165	176	161	162	...

Instead of computing the *hsd* as in example II-3, use is made of the sum of the ranges and of the tabulated values in table II-4, which is a condensed version of a larger table [195]. The value of α is 5 per cent, the number in the group is 6, and the number of ranges is 8. The corresponding figure from table II-4 is 0.55. The value of 248(0.55)/6

TABLE II-4. Factors for allowances for one-way classifications*

(r) no./group = no./range	Number of Groups = Number of Ranges (v)																		
	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
								5% risk											
2	3.45	2.37	1.78	1.40	1.16	1.00	.87	.78	.70	.66	.63	.58	.50	.47	.44	.42	.40	.38	.36
3	1.91	1.44	1.13	.94	.80	.70	.62	.56	.51	.47	.43	.40	.38	.36	.33	.32	.30	.29	.27
4	1.63	1.25	1.01	.84	.72	.63	.57	.51	.47	.43	.40	.37	.35	.33	.31	.29	.28	.27	.25
5	1.53	1.19	.96	.81	.70	.61	.55	.50	.45	.42	.39	.36	.34	.32	.30	.29	.27	.26	.25
6	1.50	1.18	.95	.80	.69	.61	.55	.49	.45	.42	.39	.36	.34	.32	.30	.29	.27	.26	.25
7	1.49	1.17	.95	.80	.69	.61	.55	.50	.45	.42	.39	.36	.34	.32	.30	.29	.28	.26	.25
8	1.49	1.17	.96	.81	.70	.62	.55	.50	.46	.42	.39	.37	.35	.33	.31	.29	.28	.27	.25
9	1.50	1.18	.97	.82	.71	.62	.56	.51	.47	.43	.40	.37	.35	.33	.31	.30	.28	.27	.26
10	1.52	1.20	.98	.83	.72	.63	.57	.52	.47	.44	.41	.38	.35	.34	.32	.30	.29	.27	.26
								1% risk											
2	7.92	4.42	2.96	2.06	1.69	1.39	1.20	1.03	.91	.82	.75	.68	.63	.59	.55	.51	.48	.46	.43
3	3.14	2.14	1.57	1.25	1.04	.89	.78	.69	.62	.57	.52	.48	.45	.42	.39	.37	.35	.34	.32
4	2.47	1.74	1.33	1.08	.91	.78	.69	.62	.56	.51	.47	.44	.41	.38	.36	.34	.32	.31	.29
5	2.24	1.60	1.24	1.02	.86	.75	.66	.59	.54	.49	.46	.42	.40	.37	.35	.33	.31	.30	.29
6	2.14	1.55	1.21	.99	.85	.74	.65	.59	.53	.49	.45	.42	.39	.37	.35	.33	.31	.30	.28
7	2.10	1.53	1.21	.99	.84	.74	.65	.59	.53	.49	.45	.42	.40	.37	.35	.33	.32	.30	.29
8	2.08	1.52	1.21	.99	.85	.74	.66	.59	.54	.50	.46	.43	.40	.37	.35	.33	.32	.30	.29
9	2.09	1.53	1.22	1.00	.85	.75	.66	.60	.54	.50	.46	.43	.40	.38	.36	.34	.32	.31	.29
10	2.10	1.55	1.23	1.01	.86	.75	.67	.61	.55	.51	.47	.44	.41	.38	.36	.34	.33	.31	.30

*This table was reproduced with the permission of T. F. Kurtz, R. F. Link, D. L. Wallace, and J. W. Tukey, Princeton University, Princeton, New Jersey.

= 22.7 closely approximates the *hsd* value = 22.0 obtained in the previous example. From here on the test procedure is exactly the same as for the *hsd* test.

II-1.1.6 Comments on multiple range tests. The use of the *lsd* test leads to v-treatment type I errors larger than $\alpha = 5$ per cent when the largest mean is compared with the smallest mean in a set of v treatments for $v > 2$. The size of the type I error is obtained from formula II-1. In order to remedy this defect in the *lsd* test, other tests have been proposed. Some of these are described in the preceding sections. The choice among the three tests given in sections II-1.1.2 to II-1.1.4 will depend upon the size of the "type I error" desired. Duncan's test has a larger "type I error" but a smaller "type II error" than either of the other two. Likewise, the Student-Newman-Keuls test has a larger "type I error" and a smaller "type II error" than Tukey's test based on allowances. The choice between the tests will depend upon the relative importance of the errors. Discussion of the relative merits of the various procedures is limited to that given in unpublished reports [96, 97, 296]. The effect of non-normality and heterogeneity of error variances upon the test has not been fully discussed [296]; despite this the tests described in sections II-1.1.2 to II-1.1.5 are considerably better than the *lsd* test and should be used in preference to it. The *lsd* test does not take into account the number of treatments in the experiment, whereas the other tests do.

If the *lsd* test is used, the following procedure tends to decrease the size of the "type I error" and to retain the simplicity attached to the *lsd* and *hsd* tests. Among the n means, there are $n(n - 1)/2$ possible differences. The *lsd* test on all differences gives approximately the right two-treatment type I error. The trouble arises when only the largest differences are compared with the *lsd*. When $t_{05,fdf}$ is used to compute a significant difference, approximately 5 per cent of the differences would be expected to exceed this value by chance alone. Therefore, if the $(0.05)(n)(n - 1)/2$ *smallest differences exceeding* the *lsd* are asserted to be nonsignificant and if all larger differences exceeding the *lsd* are said to be significant, the size of the "type I error" *may* be approximately correct as far as the significance statements are concerned. The size of the 95 per cent confidence interval would not be 2 *lsd* but some larger value. The properties of the above procedure have not been discussed, and it is suggested that one of the tests in sections II-1.1.2-5 be used in preference to this procedure.

II-1.2 MULTIPLE *F* TESTS

II-1.2.1 Fisher's least significant difference test. Fisher [126, sec. 24] suggests the combined use of an F test and of the *lsd* test. If the observed F exceeds the tabulated F_α, the *lsd* is used to compare differences among the n means. If the observed $F < F_\alpha$ the *lsd* is not used to make comparisons among the means. Duncan [97] has examined some of the properties of this test and has found that the procedure results in an α per cent "type I error"

for two and three treatments in the experiment. With more than three means in the group the "type I error" is larger than α, and in some cases it is as large as that obtained for the *lsd* test (section II-1.1.1). Hence, the procedure cannot be regarded as satisfactory.

The F test may be used with any multiple range test. The properties of such a procedure have not been discussed. Further study is required to determine whether such procedures are worth while.

II-1.2.2 Duncan's multiple comparisons test. In addition to comparisons between any two means, comparisons between groups of means are often desired. Duncan [93-7] has developed a test procedure for such comparisons. His multiple comparisons test resolves the difficulty of obtaining a nonsignificant range and a significant F value. The test procedure involves three stages. The first stage is essentially a multiple range test but with different ranges than any of those given in the previous sections, although the calculated ranges are not too divergent from those obtained for the Student-Newman-Keuls test. The calculated ranges for the multiple comparisons test are obtained from tables II-5 or II-6.

The second stage of Duncan's multiple comparisons test involves the calculation of a set of least significant sums of squares and of the sums of squares among certain combinations of the means. The sums of squares among means are compared with the least significant sums of squares which are obtained from the least significant ranges. This comparison is essentially a comparison of observed F values with tabular F values, although sums of squares are used instead of mean squares and the tabular values in tables II-5 and II-6 are different from those used for ordinary F tests [273].

The third stage of Duncan's test is required only in certain instances. The observed range for the means in the group must be less than the calculated range for two means, and the observed sum of squares among means must be larger than the calculated sum of squares for the number in the group. This stage of the test is useful in detecting the significance of a comparison of groups of means. The various stages in Duncan's multiple comparisons test are illustrated with a numerical example.

Example II-5. The means in table II-2 and the estimated standard error of a mean, $s_{\bar{x}} = 4.86$ with 40 degrees of freedom, are used to illustrate the procedure for applying Duncan's multiple comparisons test. The first step is to construct table II-7, using $s_{\bar{x}}$ and values of $R'_{p.40}$ from either table II-5 or table II-6, depending upon the significance level chosen. To be consistent with the preceding examples the 5 per cent level of significance is chosen. In table II-7 the first row contains the numbers 2, 3, $\cdots$, $v = 8$, the number of treatments in the group. The second row contains the values of R'_p for 40 degrees of freedom obtained from table II-5. The third row is computed by multiplying $s_{\bar{x}}$ by the tabulated value of $R'_{p.40}$ for $p = 2, 3, \cdots, 8$; for example, $R_3 = R'_{3.40}s_{\bar{x}} = 3.143(4.86) = 15.27$. The fourth row is computed from the formula $ss_p = \frac{1}{2}R_p^2$; thus, $ss_3 = \frac{1}{2}R_3^2 = 116.59$. The symbol ss_p indicates a sum of squares

TABLE II-5. Significant ranges, $R'_{p,df}$, for a 5% level multiple comparisons test*

df \ p	2	3	4	5	6	7	8	9	10	11	13	15	17	19	21	51	101
10	3.151	3.444	3.691	3.908	4.101	4.28	4.44	4.58	4.72	4.85	5.08	5.30	5.48	5.66	5.82	7.37	8.63
11	3.113	3.405	3.653	3.871	4.065	4.24	4.40	4.55	4.69	4.82	5.06	5.28	5.48	5.65	5.82	7.43	8.77
12	3.082	3.373	3.621	3.840	4.036	4.21	4.38	4.53	4.67	4.80	5.05	5.27	5.47	5.65	5.82	7.49	8.89
13	3.055	3.346	3.595	3.814	4.011	4.19	4.36	4.51	4.65	4.79	5.03	5.26	5.46	5.65	5.82	7.54	9.00
14	3.033	3.324	3.572	3.792	3.989	4.17	4.34	4.49	4.64	4.77	5.02	5.25	5.45	5.64	5.82	7.58	9.10
15	3.014	3.304	3.553	3.773	3.971	4.15	4.32	4.48	4.62	4.76	5.01	5.24	5.45	5.64	5.82	7.62	9.20
16	2.998	3.288	3.536	3.757	3.955	4.14	4.30	4.46	4.61	4.75	5.00	5.23	5.44	5.64	5.82	7.66	9.28
17	2.984	3.273	3.522	3.742	3.941	4.12	4.29	4.45	4.60	4.74	4.99	5.23	5.44	5.64	5.82	7.70	9.36
18	2.971	3.260	3.508	3.729	3.929	4.11	4.28	4.44	4.59	4.73	4.99	5.22	5.44	5.64	5.82	7.73	9.43
19	2.960	3.248	3.497	3.718	3.918	4.10	4.27	4.43	4.58	4.72	4.98	5.22	5.44	5.64	5.82	7.76	9.50
20	2.950	3.238	3.487	3.707	3.908	4.09	4.26	4.42	4.57	4.71	4.97	5.21	5.43	5.64	5.83	7.78	9.56
22	2.933	3.220	3.469	3.690	3.890	4.08	4.25	4.41	4.56	4.70	4.96	5.21	5.43	5.63	5.83	7.83	9.68
24	2.919	3.206	3.454	3.675	3.876	4.06	4.23	4.39	4.55	4.69	4.96	5.20	5.42	5.63	5.83	7.87	9.78
26	2.908	3.193	3.441	3.663	3.864	4.05	4.22	4.38	4.54	4.68	4.95	5.19	5.42	5.63	5.83	7.91	9.87
28	2.896	3.183	3.431	3.652	3.854	4.04	4.21	4.38	4.53	4.67	4.94	5.19	5.42	5.63	5.83	7.94	9.95
30	2.888	3.174	3.442	3.643	3.845	4.03	4.20	4.37	4.52	4.67	4.94	5.19	5.42	5.63	5.83	7.97	10.02
40	2.858	3.143	3.390	3.611	3.814	4.00	4.18	4.34	4.50	4.64	4.92	5.17	5.41	5.63	5.84	8.08	10.30
60	2.828	3.112	3.358	3.580	3.783	3.97	4.15	4.31	4.47	4.62	4.90	5.16	5.40	5.63	5.84	8.21	10.64
100	2.804	3.086	3.333	3.555	3.758	3.95	4.12	4.29	4.45	4.60	4.89	5.15	5.40	5.63	5.85	8.33	10.97
∞	2.772	3.051	3.297	3.518	3.722	3.91	4.09	4.26	4.42	4.57	4.86	5.13	5.39	5.63	5.86	8.54	11.66

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TABLE II-6. Significant ranges, $R'_{p,df}$, for a 1% level multiple comparisons test*

df \ p	2	3	4	5	6	7	8	9	10	11	13	15	17	19	21	51	101
10	4.482	4.876	5.216	5.517	5.788	6.04	6.27	6.48	6.68	6.87	7.23	7.55	7.84	8.11	8.37	11.00	13.45
11	4.393	4.780	5.114	5.411	5.680	5.93	6.16	6.37	6.57	6.76	7.11	7.43	7.73	8.00	8.26	10.93	13.46
12	4.320	4.702	5.032	5.326	5.592	5.84	6.06	6.28	6.48	6.67	7.02	7.34	7.64	7.91	8.17	10.88	13.46
13	4.260	4.637	4.963	5.254	5.519	5.76	5.99	6.20	6.40	6.59	6.94	7.26	7.56	7.84	8.09	10.83	13.47
14	4.210	4.582	4.905	5.194	5.457	5.70	5.92	6.14	6.33	6.52	6.87	7.19	7.49	7.77	8.03	10.79	13.48
15	4.168	4.536	4.856	5.143	5.404	5.64	5.87	6.08	6.28	6.46	6.82	7.14	7.43	7.71	7.97	10.75	13.48
16	4.131	4.496	4.814	5.098	5.358	5.60	5.82	6.03	6.23	6.42	6.76	7.08	7.38	7.66	7.92	10.72	13.49
17	4.098	4.461	4.777	5.060	5.317	5.56	5.78	5.99	6.18	6.37	6.72	7.04	7.34	7.62	7.88	10.70	13.49
18	4.070	4.431	4.744	5.025	5.282	5.52	5.74	5.95	6.14	6.33	6.68	7.00	7.30	7.58	7.84	10.67	13.50
19	4.046	4.404	4.716	4.995	5.250	5.49	5.71	5.92	6.11	6.30	6.64	6.96	7.26	7.54	7.80	10.65	13.50
20	4.023	4.379	4.689	4.968	5.222	5.46	5.68	5.88	6.08	6.27	6.61	6.93	7.23	7.51	7.77	10.63	13.51
22	3.987	4.338	4.646	4.922	5.174	5.41	5.63	5.83	6.03	6.21	6.56	6.88	7.18	7.46	7.72	10.59	13.52
24	3.956	4.304	4.609	4.883	5.134	5.37	5.58	5.79	5.98	6.17	6.51	6.83	7.13	7.41	7.67	10.56	13.53
26	3.930	4.276	4.579	4.851	5.100	5.33	5.55	5.75	5.94	6.13	6.47	6.79	7.09	7.37	7.63	10.54	13.54
28	3.907	4.252	4.553	4.824	5.072	5.30	5.52	5.72	5.91	6.10	6.44	6.76	7.06	7.34	7.60	10.52	13.55
30	3.889	4.231	4.531	4.800	5.048	5.28	5.49	5.69	5.89	6.07	6.41	6.73	7.03	7.31	7.57	10.50	13.55
40	3.824	4.160	4.454	4.719	4.962	5.19	5.40	5.60	5.79	5.97	6.31	6.63	6.92	7.20	7.47	10.43	13.58
60	3.762	4.091	4.379	4.640	4.879	5.10	5.31	5.51	5.70	5.88	6.21	6.53	6.82	7.10	7.36	10.36	13.62
100	3.712	4.036	4.320	4.577	4.813	5.03	5.24	5.44	5.62	5.80	6.13	6.44	6.74	7.01	7.28	10.30	13.66
∞	3.643	3.958	4.235	4.486	4.716	4.93	5.13	5.32	5.51	5.68	6.01	6.32	6.61	6.88	7.14	10.20	13.75

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significant at the $\alpha = 5$ per cent level. It is not necessary to compute the values of ss_p until stage 2 has been reached. By this time, it may be unnecessary to compute all ss_p, since the bracketed groups may contain fewer treatments than the larger values of p.

TABLE II-7. Least significant ranges and least significant sums of squares

p	2	3	4	5	6	7	8
$R'_{p,\ 40}$	2.858	3.143	3.390	3.611	3.814	4.00	4.18
R_p	13.89	15.27	16.48	17.55	18.54	19.44	20.31
ss_p	—	116.59	135.80	154.00	171.87	—	—

STAGE 1. A RAPID FIRST APPROXIMATION

In the first stage, observe that the difference between the largest and smallest means, i.e., $\bar{x}_4 - \bar{x}_7 = 185 - 161 = 24$, exceeds the value of $R_8 = 20.31$. Since it does, the means of fats 4 and 7 are considered to be significantly different. Repeating the process, compare fat 4 with the other fats:

$\bar{x}_4 - \bar{x}_8 = 185 - 162 = 23 > R_7 = 19.44$, hence fats 4 and 8 differ.
$\bar{x}_4 - \bar{x}_5 = 185 - 165 = 20 > R_6 = 18.54$, hence fats 4 and 5 differ.
$\bar{x}_4 - \bar{x}_1 = 185 - 172 = 13 < R_5 = 17.55$, and the process terminates.

At this point, decisions concerning any significance of differences among the means of fats 4, 3, 2, 6, and 1 are deferred until the next stage, and a bracket is placed around this group in figure II-2. Stage 1 is continued by comparing the second highest mean, $\bar{x}_3$, with the lowest, next lowest, etc., as follows:

$\bar{x}_3 - \bar{x}_7 = 182 - 161 = 21 > R_7 = 19.44$, hence fats 3 and 7 differ.
$\bar{x}_3 - \bar{x}_8 = 182 - 162 = 20 > R_6 = 18.54$, hence fats 3 and 8 differ.
$\bar{x}_3 - \bar{x}_5 = 182 - 165 = 17 < R_5 = 17.55$, and the process terminates.

$\bar{x}_2 - \bar{x}_7 = 178 - 161 = 17 < R_6 = 18.54$, and no further comparisons are made until the next stage. *Care should be taken in any particular sequence not to test a difference for which a decision has already been deferred in the previous sequence.* The bracketing in figure II-2 is now completed, resulting in the groups 43261, 32615, and 261587. If a bracket at the end of stage 1 contains only two means, the difference between the two is not significant.

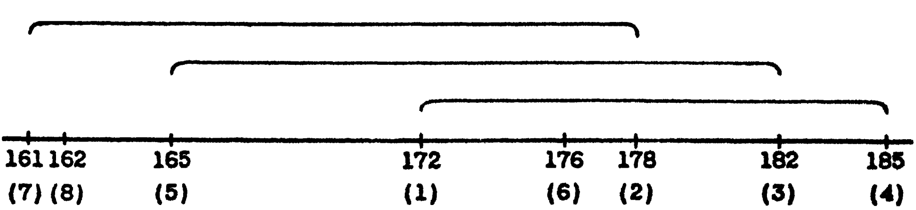

Figure II-2. Graphical array of eight fat means of example II-1 with the identification number of the fat in parentheses. The above bracketing is possible at the end of stage 1 of Duncan's test.

STAGE 2. TEST FOR RANGES AND SUMS OF SQUARES OF GROUPS IN BRACKETS

In this step, repeated applications of the following rule are made: *For a group of means bracketed together, say $\bar{x}_1, \bar{x}_2, \cdots, \bar{x}_n$ ranked in order, the difference $\bar{x}_n - \bar{x}_1$ is significant if $\bar{x}_n - \bar{x}_1 > R_2$ and if also the sum of squares of each combination of means, chosen from $\bar{x}_1, \bar{x}_2, \cdots, \bar{x}_n$ and including $\bar{x}_1$ and $\bar{x}_n$, exceeds the least significant sum of squares ss_p, for p means, p being the number of means in the given combination.*

The first step is to compare the sum of squares among the n means in the group with ss_n. The second step is to compare the range $\bar{x}_n - \bar{x}_1$ with R_2 and with R_3, etc., until $R_{m+1} > \bar{x}_n - \bar{x}_1 > R_m$ for $R_m > \bar{x}_n - \bar{x}_1$. From this fact, it would be established [94, 95] that the sum of squares among m means or less would exceed the corresponding ss_p. If there are several sets of sums of squares among m varieties to compute, select the group giving the smallest sum of squares, and if this exceeds ss_m, the remainder will also.

Na Nagara [230] developed a procedure for determining which combination of means including the extremes yields the minimum sum of squares. Given the set of ranked means $\bar{x}_1, \bar{x}_2, \cdots, \bar{x}_n$ where $\bar{x}_n$ is the largest mean, the mean of the means $\bar{x} = \sum \bar{x}_i/n$, and the deviations $\bar{x}_1 - \bar{x} = y_1$, $\bar{x}_2 - \bar{x} = y_2$, $\cdots$, $\bar{x}_n - \bar{x} = y_n$, the combination of $n - 1$ means yielding the minimum sum of squares will be the one excluding either $\bar{x}_2$ or $\bar{x}_{n-1}$. If $|y_2| > y_{n-1}$, $\bar{x}_2$ is excluded, and if $|y_2| < y_{n-1}$, $\bar{x}_{n-1}$ is excluded. The minimum sum of squares is equal to $\sum_{i=1}^{n}(\bar{x}_i - \bar{x})^2 - y_i^2 - y_i^2/(n - 1)$, where i is the value excluded either 2 or $n - 1$.

The minimum sum of squares among combinations of $n - 2$ means from a set of n means, including the two extremes, is obtained by the following procedure:

(i) If $2y_2 > (n - 2 + 1)(-y_{n-1} - y_3)$, exclude $\bar{x}_2$ and $\bar{x}_3$.
(ii) If $-2y_{n-1} > (n - 2 + 1)(y_2 + y_{n-2})$, exclude $\bar{x}_{n-2}$ and $\bar{x}_{n-1}$.
(iii) If the inequalities in (i) and (ii) are reversed, exclude $\bar{x}_2$ and $\bar{x}_{n-1}$.
(iv) The minimum ss is equal to $\sum(\bar{x}_i - \bar{x})^2 - y_i^2 - y_j^2 - (y_i + y_j)^2/(n - 2)$, where i and j refer to the pair of values excluded in steps (i) to (iii).

Na Nagara [230] presents a procedure for obtaining the minimum sum of squares among combinations of $n - 3$ means from a set of n means when the extremes are retained and indicates the procedure for additional steps. The results are illustrated below.

Applying stage 2 to the group of five means containing the means for fats 4, 3, 2, 6, and 1, we find

(1)
$$\begin{aligned} ss_{43261} &= 185^2 + 182^2 + 178^2 + 176^2 + 172^2 \\ &\quad - \frac{(185 + 182 + 178 + 176 + 172)^2}{5} \\ &= 103.2 < ss_5 = 154.00. \end{aligned}$$

Hence, there are no significant differences in the group 43261.

Considering now the group 32615,

(1) $$ss_{32615} = 167.2 > ss_5,$$
(2) $$\bar{x}_3 - \bar{x}_5 = 182 - 165 = 17 > R_4 = 16.48,$$

and, therefore, $ss_{3215} > ss_4$, $ss_{3265} > ss_4$, $ss_{3615} > ss_4 = 135.80$; $ss_{325} > ss_3$, $ss_{365} > ss_3$,

and $ss_{315} > ss_3 = 116.59$. From (1) and (2), all relevant inequalities are satisfied; hence, fats 3 and 5 are considered to be different. In the above application, note that combinations of three and four means involving only the means for fats 3 and 5 are relevant and that the combination ss_{35} is not required.

Continue this process with the remaining groups of four from the above group of five, i.e., groups 3261 and 2615. The former group has already been included in the larger group 43261 and need not be tested here. The latter group will be tested in the larger group 261587.

We are now ready to consider the last group of means, i.e., the six means for fat numbers 7, 8, 5, 1, 6, and 2.

(1) $$ss_{785162} = 161^2 + 162^2 + 165^2 + 172^2 + 176^2 + 178^2 - (1014)^2/6$$
$$= 268 > ss_6 = 171.87.$$

(2) $$\bar{x}_2 - \bar{x}_7 = 178 - 161 = 17 > R_4.$$

The minimum sum of squares among combinations of five means is obtained from the procedure described by Na Nagara [230]. The mean of the six means is $(178 + 176 + 172 + 165 + 162 + 161)/6 = 169.0$; $y_2 = 162 - 169 = -7$; $y_{n-1} = y_5 = 176 - 169 = 7$. Hence, the sum of squares excluding the mean 176 is equivalent to that obtained excluding the mean 162 and is equal to $ss_{78512} = ss_{75162} = 268 - 7^2 - 7^2/(6-1) = 209.2 > ss_5 = 154.00$. The sums of squares among combinations of four means and among three means need not be computed, since the range $\bar{x}_2 - \bar{x}_7 > R_4$. Hence, the means for fats 7 and 2 are asserted to be different.

From the above six means, consider now groups of five means which do not include both 7 and 2, or 78516 and 85162. The ranges of means in the two groups are $\bar{x}_6 - \bar{x}_7 = 176 - 161 = 15 < R_5 = 17.55$, and $\bar{x}_2 - \bar{x}_8 = 178 - 162 = 16 < R_5$. We now proceed to the stage 2 analysis for each of the two sets of five means, since the ranges are less than the computed ranges for R_5. For the first group,

(1) $$ss_{78516} = 170.8 > ss_5 = 154.00 \text{ and}$$

(2) $$\bar{x}_6 - \bar{x}_7 = 15 > R_2 = 13.89.$$

Since the range is only greater than R_2 and not R_3 or R_4, we need to compute the sums of squares for various ss_p's. The minimum sum of squares among four means is the sum of squares for the group 7516, which is equal to $ss_{78516} - y_i^2 - y_i^2/4 = 170.8 - (162 - 167.2)^2 - (-5.2)^2/4 = 137.0$. The minimum sum of squares among combinations of three means is obtained when either pair of means 5 and 8 or 1 and 8 is excluded and is equal to $170.8 - (-5.2)^2 - (-2.2)^2 - (-7.4)^2/3 = 120.67 = 170.8 - (-5.2)^2 - (4.8)^2 - (-5.2 + 4.8)^2/3$.

All inequalities are satisfied, and it is concluded that means 6 and 7 are different. The group of five means is broken up into two groups of four; i.e., 7851 and 8516. The latter group will be compared in the larger group 85162. The stage 1 test for group 7851 is

$$\bar{x}_1 - \bar{x}_7 = 172 - 161 = 11 < R_4.$$

The stage 2 test is

(1) $$ss_{7851} = 74 < ss_4 = 135.80.$$

Hence, no differences are found among these four means.

Comparisons within the group 85162 are now made. From the stage 2 analysis, we find

(1) $$ss_{85162} = 191.2 > ss_5 = 154.00.$$

(2) $\bar{x}_2 - \bar{x}_8 = 16 > R_3$, and the inequalities for three means ss_{852}, ss_{812}, $ss_{862} > ss_3$ are satisfied. It is now required to check the sum of squares involving four means. The minimum sum of squares is

$$ss_{8162} = 152 > ss_4 = 135.80,$$

and all inequalities are satisfied. A difference between means 2 and 8 is indicated. The groups of four means to be considered now are 8516 and 5162. The groups are not changed by a stage 1 analysis:

$$\bar{x}_6 - \bar{x}_8 = 176 - 162 = 14 < R_4$$

and $$\bar{x}_2 - \bar{x}_5 = 178 - 165 = 13 < R_4.$$

The stage 2 analysis on the group 8516 results in the following:

(1) $$ss_{8516} = 122.75 < ss_4 = 135.80,$$

and the test terminates. No significant differences are included in the group 8516.

We now consider the group 5162.

(1) $$ss_{5162} = 98.75 < ss_4 = 135.80,$$

and the test terminates.

All groups have been separated into subgroups asserted to be not heterogeneous in the above analyses, and no further analysis is required for these data. Brackets include groups of means which do not contain significant differences (see figure II-3). From this it is concluded that

7 is different from 2, 3, 4, and 6;
8 is different from 2, 3, and 4;
5 is different from 3 and 4;

in the remainder of the comparisons the treatments are not different.

If the sum of squares for a group of n means is greater than $ss_n = \frac{1}{2}R_n^2$ but the range of the means is less than R_2, it is necessary to conduct the third stage of Duncan's multiple comparisons test. The stage 3 analysis consists of partitioning the $n - 1$ degrees of freedom into single degree of freedom contrasts. The comparison or comparisons contributing to the significant sum of squares are segregated in the stage 3 analysis. Duncan [94] suggests that inspection of the means usually indicates the linear combination(s) of the means causing the significance. In the above example no stage 3 analysis was required. If a stage 3 analysis had been required, a statement about

the comparison of means contributing to the significance of the sums of squares would be added to the bracketing in figure II-3.

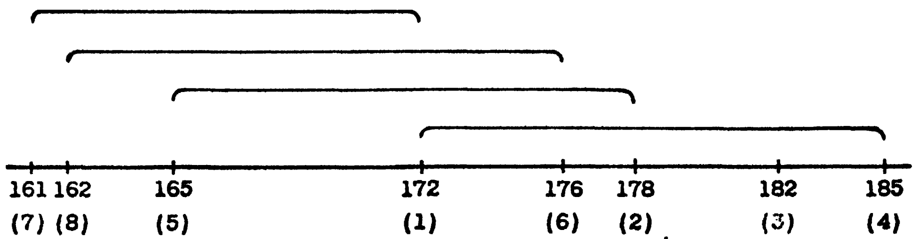

Figure II-3. Graphical array of eight fat means of example II-1 with the identification number of the fat in parentheses. The bracketing is possible at the end of Duncan's test. The means within a bracket are said not to differ, but means not bracketed together are asserted to be different.

In some cases the range of the n means in the group may be greater than R_2 and the sum of squares greater than ss_n, but one or more of the sums of squares involving fewer means may be less than the ss_p. For such cases a stage 3 analysis is necessary.

II-1.2.3 Scheffe's test. Scheffé [262] described a procedure for testing comparisons within a set of v means. The comparison may be decided upon prior to conducting the experiment or after the results have been obtained and studied. The test procedure is similar in application to Tukey's test based on allowances. Instead of obtaining the multiplier for the standard error of a contrast from a table of ranges (table II-1) the multiplier is obtained from the ordinary F table (table II-8) and is equal to $\sqrt{(v-1)F_{\alpha}(v-1, fdf)} = S$, where v = number of means in the experiment, α = level of significance, and f = the number of degrees of freedom associated with the error variance. The standard error for a comparison depends upon the number of means in the comparison and upon the coefficients of the means. For comparisons of two means the standard error of the comparison is equal to $\sqrt{2}\, s_{\bar{x}}$, where $s_{\bar{x}}$ is the standard error of a mean. A comparison of the form $\bar{x}_1 + \bar{x}_2 - \bar{x}_3 - \bar{x}_4$ has a standard error equal to $2s_{\bar{x}}$; etc. The following example will serve to illustrate the various aspects of applying Scheffé's test. The error committed is of the same nature as that committed using Tukey's test based on allowances.

Example II-6. The data of example II-1 are used to illustrate the application of Scheffe's multiple F test. The standard error of the difference between two means is equal to $\sqrt{2}\, s_{\bar{x}} = 1.414(4.86) = 6.87$. The value of F_{05} for $v - 1 = 7$ degrees of freedom in the numerator and $f = 40$ degrees of freedom in the denominator is equal to 2.25. The multiplier S is computed as follows: $\sqrt{(8-1)(2.25)} = 3.969 = S$. The quantity $Ss_{\bar{x}_i - \bar{x}_j} = 3.969(6.87) = 27.3$ is computed; if the difference between any two means exceeds this value, the two means are said to differ significantly. In the present example, all differences between two means (table II-2) are less than 27.3.

Instead of considering comparisons of only two means, comparisons involving four means may be considered. For example, the experimenter might wish to compare the

means for fat numbers 7 and 8 with other pairs of means. The standard error for the difference $\bar{x}_4 + \bar{x}_3 - \bar{x}_8 - \bar{x}_7 = 44$ is equal to $\sqrt{141.6\{\frac{1}{6} + \frac{1}{6} + \frac{1}{6} + \frac{1}{6}\}} = 9.72$, and $Ss_{\bar{x}_4+\bar{x}_3-\bar{x}_8-\bar{x}_7} = 3.969(9.72) = 38.6$. The above difference exceeds the computed value and is declared significant.

Another contrast of possible interest is the mean of the lowest three means and the mean of the highest five means; i.e., $(\bar{x}_7 + \bar{x}_8 + \bar{x}_5)/3$ vs $(\bar{x}_1 + \bar{x}_6 + \bar{x}_2 + \bar{x}_3 + \bar{x}_4)/5$, or $(185 + 182 + 178 + 176 + 172)/5 - (161 + 162 + 165)/3 = 178.6 - 162.7 = 15.9$. The standard error for this contrast is $\sqrt{\frac{141.6}{6}\left\{\frac{1}{3} + \frac{1}{5}\right\}} = 3.55$, and the computed significant difference is equal to $3.969(3.55) = 14.1$. The observed difference, 15.9, exceeds this value, and the means of the two groups are said to differ significantly. Other multiple comparisons may be made if desired. The procedure is first to determine the standard error and then to multiply the standard error by the value of $S = 3.969$. The observed difference is compared with the calculated value in order to determine its significance. Scheffé's test is completed with a statement about the significance of the various comparisons.

II-1.2.4 Comments on multiple *F* tests. An F test comparing the mean square of the means in a subset with the error mean square may be calculated after any multiple range test. The combined use of range tests and variance ratio tests results in a multiple F test. As stated previously the properties of most of these tests are unknown.

Three multiple F tests are presented in this section. The first one involving an F test and subsequently an *lsd* test has undesirable properties when the number of means in the group becomes sizable [97]. Of the other two multiple F tests, Duncan's multiple comparisons test has a larger "type I error" but a much smaller "type II error" than Scheffé's procedure.

Any known or implied grouping of the treatments should be taken into account prior to using a multiple range or multiple F test procedure. After doing this, comparisons between two means will tend to dominate the kind of comparisons made. Since this is so, one of the multiple range tests or Duncan's multiple comparisons test should be used in preference to Scheffé's test. The size of the calculated intervals of two means is 27.3 for Scheffé's test and 22.0 for Tukey's test based on allowances for the example. In order to be declared significant under Scheffé's procedure, a difference must be $3.969\sqrt{2}/4.52 - 1 =$ 24 per cent larger than for Tukey's test based on allowances when $n = 8$, $f = 40$, and $\alpha = .05$. If comparisons of only two means are being made Scheffé's procedure would be inefficient relative to Tukey's test based on allowances. For comparisons involving several means, Scheffé's procedure is more efficient than Tukey's test based on allowances [296].

Duncan [96, 97] sets the type I error rate at α_p for rejecting the null hypothesis that $\mu_1 = \mu_2 = \cdots = \mu_p$ when it is in fact true. The error rate α_p must be computed for each value of p. For $p = 2$, α_p is equal to α. In order to compute the least significant range for n means, Duncan uses the relation $\sqrt{2(n-1)F_{\alpha_n}(n-1, fdf)}\, s_{\bar{x}} = R'_n s_{\bar{x}}$; Scheffé uses the relation

$\sqrt{2}\sqrt{(n-1)F_\alpha(n-1, fdf)}\, s_{\bar{x}}$ for comparisons between two means. Duncan sets his error at $\alpha_n = \alpha_p$, and Scheffé sets his error at α. This is the only difference in the two formulae for the comparison of two means. Duncan's significant range changes when p, the number of treatments in the subset, changes. Thus, for $n - 1 = p$ treatments, Duncan's least significant range is equal to $\sqrt{2(n-2)F_{\alpha_{n-1}}(n-2, fdf)}\, s_{\bar{x}}$. With Scheffé's procedure, groups of means which are said to be not heterogeneous may have a large mean square relative to the error mean square.This disagreement is not found with Duncan's procedure.

As the F analogue of the Student-Newman-Keuls multiple range test, one could compute the various values, $S_{n-p} = \sqrt{(n-1)F_\alpha(n-1, fdf)}$, $S_{n-1} = \sqrt{(n-2)F_\alpha(n-2, fdf)}$, $\cdots$, $S_3 = \sqrt{2F_\alpha(2, fdf)}$, and $S_2 = \sqrt{F_\alpha(1, fdf)} = t_{\alpha, fdf}$. The particular S_i used would depend upon the number of means in the comparison. Tukey [296] states that there is little to recommend such a test.

II-1.3 TUKEY'S GAP, STRAGGLER, AND VARIANCE TEST

Tukey [292] developed a test for detecting non-heterogeneous subsets of means within a set of ranked means. The test consists of first finding gaps in adjacent mean differences which are larger than the $lsd = t_{\alpha, fdf} s_{\bar{x}} \sqrt{2}$ and subdividing the set of ranked means into groups. The next step is to separate straggler means (extreme deviates) from the mean of the group. The last step is to compare the variation among means in the subgroups with the error variation using the F test.

Although Tukey [296] states that this test is now obsolete, it is presented to illustrate the procedure and because it is useful in selecting non-heterogeneous subgroups. The test represents a combination of multiple range and multiple F tests after locating gaps in the ranked means [97].

A multiple range test compares the extremes of a group, whereas the gap, straggler, and variance test compares an extreme mean with the mean of the group. Hence, no confusion should result if the two tests do not agree with each other or with the F test. Different hypotheses are being tested by the various tests.

For Tukey's test, as with the others, an independent estimate of s^2, the error variance for the set of treatments, is required. The number of replicates per treatment should be equal, and all treatments should be subject to the same error variance s^2. If the errors vary from treatment to treatment, none of the tests holds, and a transformation of the data (see section II-2) will be necessary to stabilize the treatment variances.

The various steps in performing the Tukey test are illustrated with the data of example II-1.

Example II-7. In applying Tukey's gap, straggler, and variance test, any natural grouping should be performed first. Assuming then that a single natural (biological,

physical, or sociological) grouping is available, we proceed as follows for the data of example II-1.

Step (I). Choose a level of significance $= \alpha$. For this example, $\alpha = 5$ per cent.

Step (II). Calculate the difference which would have been significant if there were but two treatments. This difference is commonly known as the "least significant difference" (*lsd*). The *lsd* is equal to $s_{\bar{x}} t_{05,40df}\sqrt{2} = 2.021\sqrt{2(141.6)/6} = 13.88$, where t_{05} is the two-sided 5 per cent value for t with 40 degrees of freedom.

Step (III). Arrange the means in order of magnitude and consider any adjacent difference larger than the *lsd* as a group boundary.

The differences of adjacent means from highest to lowest (values given on the diagonal of table II-2) are 3, 4, 2, 4, 7, 3, and 1. None of these differences is larger than the $lsd = 13.88$, and we conclude that there is but one group.

Step (IV). In each group of three or more means, find the group mean $\bar{x}_g$, the most divergent or straggling mean $\bar{x}_d$, and convert the ratio $\dfrac{|\bar{x}_d - \bar{x}_g|}{s_{\bar{x}}}$ into approximate normal deviates by finding

$$\frac{\dfrac{|\bar{x}_d - \bar{x}_g|}{s_{\bar{x}}} - \dfrac{6}{5}\log_{10} n}{3\left\{\dfrac{1}{4} + \dfrac{1}{df\ in\ s^2}\right\}}. \text{ (for } n > \text{ three means in a group)}$$

or

$$\frac{\dfrac{|\bar{x}_d - \bar{x}_g|}{s_{\bar{x}}} - \dfrac{1}{2}}{3\left\{\dfrac{1}{4} + \dfrac{1}{df\ in\ s^2}\right\}}, \text{ (for three means in a group).}$$

Separate off any straggling mean for which this is significant at the chosen two-sided $t_{\alpha,\,\infty df}$.

The mean of the group of eight means is 172.6. The most divergent mean is 185; therefore,

$$\frac{\dfrac{185 - 172.6}{4.86} - \dfrac{6}{5}\log_{10} 8}{3\left\{\dfrac{1}{4} + \dfrac{1}{40}\right\}} = 1.78,$$

which is less than the normal deviate, 1.96, at the 5 per cent level. Hence, by Tukey's test, none of the means is separated from the group mean, 172.6, as stragglers. Upon applying the F test, a significant value of F is found, $F = 503.9/141.6 = 3.56 > F_{05}$ (7 and 40*df*) $= 2.25$. With such examples as the above, means spread out rather than clumped, Tukey's test does not separate straggler means from the group means even though the F test indicates that differences are present.

The additional steps in Tukey's gap, straggler, and variance test are used when an extreme deviate in the group of n means is obtained. The mean of the $n - 1$ means omitting the extreme mean is obtained, and the procedure in step (IV) is applied to the group of $n - 1$ means. The process continues until no more extreme deviates are obtained.

Step (V). To the subgroups of three or more extreme deviates, apply the procedure in step (IV).

Step (VI). Compare the variation among treatment means in the subgroups with the error variance.

The additional steps in Tukey's gap, straggler, and variance test are illustrated in the original paper [292].

II-1.4 SELECTION OF LARGEST *n* MEANS FROM A SET OF *v* MEANS

In certain experiments, (e.g., variety yield trials) it is desirable to include the largest mean (or the n largest means) and all means not different from the largest mean in the group considered to be "superior" with respect to the other means in the experiment. If the group interval, say $(\bar{x}_{\max.} - \lambda s_{\bar{x}})$ to $\bar{x}_{\max.}$, is large, then the size of a type I error, the error committed by misclassifying the treatments when they are equal, is small, but the size of a type II error, the error committed by misclassifying the treatments when they are different, may be large. If the type II error is not specified and the type I error is set equal to α, say 5 per cent, one of the multiple range tests may be applied.

For the particular case of partitioning a set of v means ranked in order, say $\bar{x}_1, \bar{x}_2, \cdots, \bar{x}_v = \bar{x}_{\max.}$, into an inferior and a superior group, Paulson [237-9] has provided the mathematical formulae but has not evaluated the various integrals in tabular form. The number of decisions made possible by partitioning v means into an inferior and a superior group is $2^v - 1$. For $v = 3$, seven decisions are possible; these are the first seven listed in section II-1. Paulson suggests that the situation $\mu_1 = \mu_2 = \cdots = \mu_{v-1} = \mu$ and $\mu_v = \mu + \Delta$ be considered in computing type II errors. A type I error of size α is computed as

$$\alpha = 1 - P\left(\frac{\text{range of } v \text{ means}}{\text{standard error of a mean}} = \frac{w_v}{s_{\bar{x}}} < \lambda\right), \tag{II-2}$$

where $P\left(\frac{w_v}{s_{\bar{x}}} < \lambda\right)$ represents the probability of obtaining a range less than $\lambda s_{\bar{x}}$. The type II error, say β, of the kind suggested by Paulson [237] is computed as

$$\beta = 1 - P\left(\frac{\bar{x}_i}{s_{\bar{x}}} < \frac{\bar{x}_v}{s_{\bar{x}}} - \lambda \text{ for each } i = 1, 2, \cdots, v - 1\right). \tag{II-3}$$

Tables for $P\left(\frac{w_v}{s_{\bar{x}}} < \lambda\right)$ have been prepared by May [208; table II-1] for $v = 2, 3, \cdots, 20$ and for various degrees of freedom in $s_{\bar{x}}$.

Paulson's [237] formulae, (II-2) and (II-3), may be extended to the case where it is desired to select as few treatments as possible so as to obtain a reasonable certainty of including the n largest ones; thus:

$$\alpha = 1 - P\left(\frac{\text{range below } v - n + 1 \text{ th mean}}{s_{\bar{x}}} < \lambda\right) \tag{II-4}$$

and

$$\beta = 1 - P\left(\frac{\bar{x}_i}{s_{\bar{x}}} < \frac{\bar{x}_{v-n}}{s_{\bar{x}}} - \lambda \text{ for each } i = 1, 2, \cdots, v - n\right). \tag{II-5}$$

As before, values for formula (II-4) may be obtained from May's tables or tabulated from formula (3) in Pearson and Hartley's paper [244] for various numbers of individuals in the group and degrees of freedom associated with the standard error.

II-2 Transformation of Data

II-2.1 GENERAL COMMENTS

The selection of a scale of measurement will depend upon

(i) the nature of the data and

(ii) the type of statistical procedures to be used.

The above two conditions are not incompatible, since the scale of measurement may be purely arbitrary for certain data. For example, the nonmetric (e.g., small, medium, large) system would not create any real misgivings among experimenters. Transformations such as the logarithmic or square root may cause some objections. Such objections are, in some cases, merely personal prejudices, and if presently known statistical techniques are usable for log X or $\sqrt{X}$ but not for X measured in the ordinary units, say pounds, there should be little reason for not measuring the variable X on another scale.

A transformation of X to some function, $f(X)$, should be made considering the two conditions set out above. If the principles of (i) are not violated and if the purpose for which the transformation is made is realized, the new function should not cause any confusion. Bartlett [15] lists the following requirements for an ideal transformation:

> (i) The variance of the transformed variate should be unaffected by changes in the mean.
>
> (ii) The transformed variate should be normally distributed.
>
> (iii) The transformed scale should be one for which an arithmetic average from the sample is an efficient estimate of the true mean.
>
> (iv) The transformed scale should be one for which real effects are linear and additive.

The above conditions are related to some extent, since conditions (i), (ii), and (iv) usually imply (iii). However, a transformation selected to satisfy (i) may not satisfy the remaining conditions, and transformations satisfying condition (ii) may not fulfill requirement (i). The nature of the data and the type of statistical analyses used govern the importance of the above requirements.

The following sections deal with a selected set of transformations which are considered useful in experimental work. Other transformations are discussed in the references cited at the end of the book. The theoretical considerations concerned in making transformations are more fully discussed by Curtiss [81], Deming [87], Bartlett [15], Bartlett and Kendall [16], and Eisenhart *et al.* [102].

The assumptions underlying the analysis of variance and the usual tests of significance are discussed by Eisenhart [100] in *Biometrics*; in the same issue, Cochran [54] discusses the consequences when the assumptions underlying the analysis of variance are not fulfilled. These two papers, plus the one by Bartlett [15], are recommended reading for those interested in the assumptions underlying the analysis of variance and in methods of processing experimental data when the assumptions are not satisfied.

For the general case, let the variate X_i be transformed to some new variate, say $f(X_i)$, in such a way that $f(X_i)$ is normally and independently distributed with mean μ and variance σ^2. For a sample of size n, this may be represented symbolically as

$$dF(X_1, X_2, \cdots, X_n) = \left[\frac{1}{\sigma\sqrt{2\pi}}\right]^n e^{-\frac{1}{2\sigma^2}\sum_{i=1}^{n}[f(x_i)-\mu]^2} \prod_{i=1}^{n} df(X_i). \quad \text{(II-6)}$$

The function $f(X_i)$ necessary to normalize the original variate X_i may be known for some variates X_i and unknown for others. If the distribution of the X_i is known, it is possible to find the correct $f(X_i)$ for normalizing the data [212].

It should be remembered that a function $f(X_i)$ may normalize the data for each of the v treatments, but that the treatment variances may not all be estimates of the same population variance as is required in the analysis of variance. It may be impossible to find a transformation which results in homogeneous error variances. Methods for combining data with heterogeneous error variances are described in various places [43, 56, 59, 111, 209, 332].

In situations for which the population variance is a function of the population mean, say $\sigma^2 = g(\mu)$, the approximate transformation[1] is found by evaluating the indefinite integral,

$$\int \frac{dX}{\sqrt{g(X)}} = f(X), \quad \text{(II-7)}$$

and using the transformation $f(X)$ on the X variates [15]. Occasionally, it is known that the sample means and variances are related by some unspecified function. If enough sample means and the corresponding variances are available, it may be possible to evaluate the relationship and to find the transformation from equation (II-7). The resulting transformation tends to make the means and variances independent.

II-2.2 SQUARE ROOT TRANSFORMATION

The square root transformation is useful for data following the Poisson distribution. Counts of events having a small probability of occurrence often

[1]The appropriateness of the indicated transformation depends upon the relative sizes of the standard deviation of X and the second derivative of $g(\mu)$.

follow this distribution. In the Poisson the variance is equal to the mean, $\sigma^2 = \mu$. Hence, by equation (II-7), we find that

$$\int \frac{dX}{\sqrt{X}} = 2\sqrt{X}, \tag{II-8}$$

which indicates that the square root transformation is the appropriate one. The constant may be ignored. If the variances are proportional to the means, the square root transformation stabilizes the variances.

Bartlett [15] has shown that $\sqrt{X + 1/2}$ for numbers between zero and 10 is more nearly correct than $\sqrt{X}$. Even for numbers up to 15, Curtiss [81] indicates that $\sqrt{X + 1/2}$ is somewhat better than $\sqrt{X}$. As a further refinement, Kendall [179] has proposed $\sqrt{X + 1/2 + k}$ and has given the method for evaluating k. The additional refinement over $\sqrt{X + 1/2}$ usually is of little importance in experimental work, but the choice should be made by the experimenter [also, see 4, 131].

II-2.3 THE ARCSINE OR ANGULAR TRANSFORMATION

For data having binomial variation, it will be found from formula (II-7) that the arcsine transformation,

$$\int \frac{dX}{\sqrt{X(1 - X)}} = 2 \sin^{-1} \sqrt{X}, \tag{II-9}$$

for X measured in fractions, is effective in stabilizing the variances. Snedecor [273] provides a convenient table (table 16.8) for making the angular transformation [also, see 129]. Bartlett [15] suggests that $\frac{1}{2n}$ or $\frac{1}{4n}$ be substituted for X equal to zero and $1 - \frac{1}{2n}$ or $1 - \frac{1}{4n}$ be substituted for X equal to one [also, see 4, 102, 131].

II-2.4 THE LOGARITHMIC TRANSFORMATION

For certain types of data the standard deviation is proportional to the mean; i.e., $\sigma^2 = k^2\mu^2$. From equation (II-7), we find

$$\int \frac{dX}{\sqrt{k^2X^2}} = \frac{1}{k} \log X. \tag{II-10}$$

Thus, the logarithmic transformation is appropriate. This transformation converts multiplicative effects into additive effects.

The quantity $\log (X - \alpha)$ and not $\log X$ may be normally distributed. Since α usually is unknown, it must be evaluated from the data. The procedure for estimating α would be to set $f(X_i)$ equal to $\log (X_i - \alpha)$ in equation (II-6) and obtain the formulae for estimating μ, σ^2, and α by the method of maximum likelihood [62] or by the method of moments [75, p. 258].

Transformations of the nature of $\log (X_i - \alpha)$, $\log (\alpha - X_i)$, $\log \left(\frac{\alpha + X_i}{\alpha - X_i}\right)$, $\log \left(\frac{X_i}{\alpha - X_i}\right)$, and $\log \left(\frac{X_i + \alpha}{X_i - \alpha}\right)$ have not been used as extensively as they probably should be, and it is felt that their popularity will increase as the experimenter becomes more familiar with them. The values for α in the other transformations may be obtained in the same manner as outlined for $\log (X_i - \alpha)$. The latter transformation is appropriate for data having a lower bound on the X variate; e.g., number of nodes on a plant. $\operatorname{Log} (\alpha - X_i)$ would be suitable for data having an upper bound equal to α on the variation of the X_i. The transformation $\log \left(\frac{\alpha + X_i}{\alpha - X_i}\right)^k$ is the general form which includes Fisher's [127] z transformation for the correlation coefficient; i.e., $z = \frac{1}{2} \log_e \left(\frac{1 + r}{1 - r}\right)$. In this special case the parameter σ_z^2 may be found when the size of sample is known. This is also true for certain other transformations [15, 81].

A frequently occurring problem in experimental work is the analysis of variance for a set of sample variances. The logarithmic transformation of the sample variances is the appropriate transformation in this case. Bartlett and Kendall [16] state that the individual variances should be associated with 10 or more degrees of freedom in order that the transformed values satisfy the requirements in the analysis of variance. They present an illustrative example, discuss the theoretical implications of the transformation, and indicate what happens when the individual variances have small numbers (less than 10) of degrees of freedom.

II-2.5 OTHER TRANSFORMATIONS

Bartlett [15] and others [19, 22-24, 126, 129] have discussed some additional transformations. Some of these are the probit transformation developed by Bliss [23, 24] and the logit transformation, $\log p/(1 - p)$, [22, 129] for percentage data; the normal score transformation (rankit) for ranked data [15, 129]; the methods [15, 293] for satisfying the additivity requirement; and the transformation for data following the negative binomial [19].

II-3 Test for Homogeneity of Variances

Tests of significance of variances from normally distributed variates are available for testing the equality of

(i) two independent variances (Snedecor's F, Fisher's z, Mahalanobis' x [202], and Fisher and Yates' variance ratio [129],
(ii) k independent variances, [14, 16, 43],
(iii) two variances with unknown correlation [213, 248],
(iv) k variances and associated covariances [309],
and

(v) the variances and covariances within each of several sets and the covariances between sets [298].

In addition to the above a test is available for testing the proportionality of k independent covariance matrices [110].

A rather simple procedure for detecting obvious heterogeneity among a set of k independently estimated variances is to compare the ranges and means of the samples. To illustrate the procedure, consider the data of example I-1. The ranges and means for the ten samples are

	1	2	3	4	5	6	7	8	9	10
Mean	25.4	26.8	27.0	28.0	29.0	30.0	31.8	32.0	34.0	38.0
Range	27	18	23	22	22	25	30	23	26	18

There appear to be no wide discrepancies among the ranges or any relationship with the means. For larger numbers, say larger than fifty, of ranges the observed frequency could be compared with the tabulated frequencies [244] by a chi-square test.

Box [34] has shown that Bartlett's test for homogeneity of variances [14] is almost as sensitive for testing non-normality as for testing heterogeneity of variances. Thus, it is essential to have the original data normally distributed, or nearly so, before computing sample variances.

II-4 Test for Additivity of Data

Tukey [293] presents a method for isolating a single degree of freedom associated with nonadditivity in a two-way classification. The degree of freedom is isolated from the residual sum of squares and compared with the remainder mean square in order to test the hypothesis of additivity of data. Two well-known stituations wherein the sum of squares due to the one degree of freedom for nonadditivity is increased are

(i) where one or more observations are unusually discrepant

and

(ii) where the row and column effects are not additive.

The sum of squares associated with the one degree of freedom for nonadditivity is

$$\frac{\left[\sum_{i=1}^{r} \sum_{j=1}^{c} X_{ij}(\bar{x}_{i.} - \bar{x})(\bar{x}_{.j} - \bar{x})\right]^2}{\sum_{i} (\bar{x}_{i.} - \bar{x})^2 \sum_{j} (\bar{x}_{.j} - \bar{x})^2}, \tag{II-11}$$

where X_{ij} represents the individual observation in an $r \times c$ two-way classification, $\bar{x}_{i.}$ represents the row mean, $\bar{x}_{.j}$ represents the column mean, and $\bar{x}$ represents the mean of the rcX_{ij}'s.

The quantity obtained from equation (II-11) is the linear row by linear

column interaction sum of squares with one degree of freedom [273, Ch. 15]. Although this type of interaction appears reasonable for a number of situations, there is no reason why the linear row by quadratic column interaction, the quadratic row by linear column interaction, or the quadratic by quadratic interaction could not account for nonadditivity. Also, the nonadditivity could arise from more than one source.

For three-way classifications, one could use the linear × linear × linear interaction with one degree of freedom to test for additivity. Also, interactions involving linear and quadratic effects may be the cause of the nonadditivity. An alternative approach to test for additivity in a three-way classification would be to ignore one of the classifications and use Tukey's procedure for a two-way classification. Other approaches are possible.

If nonadditivity is present and is due to one or more unusually discrepant observations whose discrepancy is due to assignable causes, the discrepant observations are omitted and the analysis is conducted on the remaining observations. When there is no assignable reason for the discrepancy, Tukey suggests that a transformation of the data be made and that an analysis of variance be performed for the transformed values. An aid in selecting the appropriate transformation is described in his paper [293].

Although other tests for additivity have not been developed for many situations, some are available. In addition to those discussed by Fisher [127, sec. 26.2] and Kempthorne [175, sec. 8.3], D. S. Robson[1] has considered two special tests. One of these is presented in example VIII-1. The second test involves the use of a standard or check treatment in an experiment. The analysis of variance for the completely randomized design is

		Average value of mean square	
Source of variation	df	Additive model	Multiplicative model
Among treatments	$v-1$	———	———
Within treatments (excluding check)	$(v-1)(r-1)$	σ^2	$\sigma^2 \sum_{i=1}^{v-1} b_i^2/(v-1)$
Within check treat.	$r-1$	σ^2	σ^2

If the within treatment variance is significantly different from the within check variance, σ^2, one would reject the hypothesis of additivity. Of the possible alternative models the multiplicative model is the one considered under the randomization test procedure [175, Ch. 7-8] in the above analysis of variance, the model is

$$Y_{ij} = b_i x_{oj} + \mu_i, \tag{II-12}$$

where Y_{ij} is the observed value of the jth observation from the ith treatment in the experiment, b_i is the proportionality constant associated with the ith

[1]Unpublished results, Cornell University.

treatment, x_{0j} is the value associated with the particular experimental unit when no treatment is applied, and μ_i is the mean of the ith treatment. If the b_i are all equal to one, equation (II-12) becomes the additive model [175, sec. 8.4]. Under the additive model the average values of the within treatment and of the within check variances are both equal to σ^2; under the multiplicative model the mean squares have the average values given in the above analysis of variance table.

II-5 Nonparametric Tests in the Analysis of Variance

A great deal of importance has been placed on statistics developed for variates which are normally distributed. This has been justifiable since most distributions, or their transforms, are not often so abnormal but that the distribution of means is near enough normal for the usual tests of significance to apply to a sufficient degree of accuracy. When the appropriate transformation is unknown, statistical methods independent of the parent distribution are required. Such methods are called *nonparametric* or *distribution-free* methods and are usually based on order statistics; e.g., the median. Mood [212] presents a number of these techniques in Chapter 16 of his book. One of the techniques is suited for the analysis of a two-way classification with one or more observations per cell and with a test of the main effects against the interaction. In some cases, considerable time may be saved by using Mood's method instead of the ordinary analysis of variance procedure.

Wilcoxon [306] has presented a nonparametric test for a two- or more-way classification involving the ranks of the treatments in one of the classifications. The computational procedure is simple and the assumption of normality is not required. Several numerical illustrations and the necessary tables have been supplied in Wilcoxon's instructive paper.

In comparing paired observations, one may use a plus and minus sign test of the differences and compare the sample results with expectations from the binomial $(1/2 + 1/2)^n$ or with χ^2 [90]. Fisher [126, sec. 21] presents a test for comparing the observed results with theoretical results without using the assumption of normality. Nonparametric methods for other situations are discussed in various places in the literature [e.g., 37, 83, 151].

II-6 Probability Levels for t, F, and χ^2

The relationship among the statistics known as chi-square, χ^2, Fisher's z, Snedecor's F, and Student's t are well known to statisticians but not to all users of statistics. These relationships are set forth here in order to avoid confusion in later sections of the book.

Fisher's z is equal to $\log_e \sqrt{F}$, where F is Snedecor's F. If tables of percentage points are available for z, it is a relatively simple operation to obtain F tables and *vice versa*. Likewise, tables of the percentage points of the correla-

tion coefficient may be computed from tables of percentage points for F (or z), since

$$F(n - 1, k - n\ df) = \frac{R^2}{(1 - R^2)/(k - n)},$$

where F has $n - 1$ degrees of freedom in the numerator and $(k - n)$ in the denominator, R^2 is the correlation coefficient between the dependent variate and the $n - 1$ independent variates, and k represents the sample size. The zero order or total correlation coefficient r is obtained when $n = 2$.

Merrington and Thompson [210] have tabulated several percentage points (.50, .25, .10, .05, .025, .01, and .005) for the F distribution with various numbers of degrees of freedom associated with the mean squares in the numerator and in the denominator. Fisher and Yates [129] have tabulated the .20, .10, .05, .01, and .001 percentage points for F. The percentage points for the F distribution for .10, .05, .025, and .01 are presented in table II-8. For more extensive tables of F the reader is referred to Snedecor [273, table 10.7], to Fisher and Yates [129, table V], and to Merrington and Thompson [210].

Student's t with n_2 degrees of freedom is equal to the square root of $F(1, n_2df)$, or $t^2(n_2df) = F(1, n_2df)$. Thus, if F tables are available, t tables may be computed by obtaining the square root of the F values for one degree of freedom in the numerator and n_2 degrees of freedom in the denominator. The square root of the F values in the second column from the left-hand-side of table II-8 yields a t table. The number of degrees of freedom associated with t are those in the first column on the left-hand side. The two-sided t percentage points correspond to the percentage points for the F distribution. A graph of t values is presented in figure II-4.

In addition to graphing t values and percentage points, Crow [76] prepared a graph of χ^2 values and percentage points for various degrees of freedom (figure II-4). The equality of $t^2(\infty\ df)$ and $\chi^2(1\ df)$ allows t and χ^2 to be presented on the same graph. Also, the values of χ^2 for the various percentage points may be obtained from the F table by multiplying the F values for $n_2 = \infty$ by the degrees of freedom associated with the numerator of F. Hence, a χ^2 table could be prepared from the last row of F's in table II-8. Values of χ^2 with more than 30 degrees of freedom and for percentage points which are not tabulated or which cannot be obtained from the F tables may be approximated from a t table for infinite degrees of freedom (the normal deviate table) since the quantity $\sqrt{2\chi^2(f\ df)} - \sqrt{2f - 1}$ is approximately normally distributed with mean zero and variance unity [127]. If $\sqrt{2\chi^2(f\ df)} - \sqrt{2f - 1}$ is greater than 1.64, the observed χ^2 value with f degrees of freedom is adjudged significant at the 5 per cent level.

For percentage points of F, t, and χ^2 not in tables, formulae are available [5, 6] for approximating these values. Bancroft [5, 6] lists the references to tables that have been prepared and illustrates the procedure for calculating additional tables.

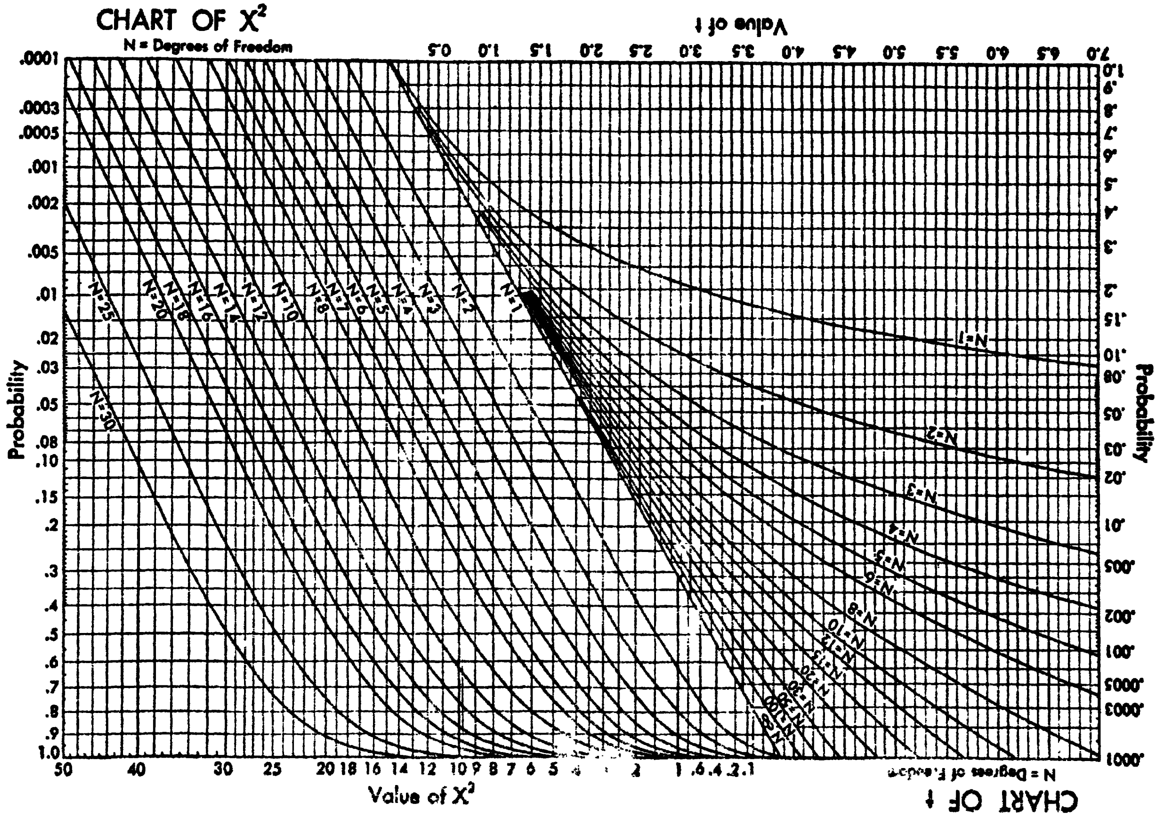

Figure II-4. A chart of t and χ^2 reproduced with the permission of the editor of the *Journal of the American Statistical Association* from the paper by J. F. Crow entitled "A chart of the χ^2 and t distribution," *J. Am. Stat. Assoc.* 40:376, 1945.

TABLE II-8. Percentage points for the F distribution*

10 per cent points

df_2 \ df_1	1	2	3	4	5	6	7	8	9	10	12	15	20	24	30	40	60	120	∞
5	4.06	3.78	3.62	3.52	3.45	3.40	3.37	3.34	3.32	3.30	3.27	3.24	3.21	3.19	3.17	3.16	3.14	3.12	3.10
6	3.78	3.46	3.29	3.18	3.11	3.05	3.01	2.98	2.96	2.94	2.90	2.87	2.84	2.82	2.80	2.78	2.76	2.74	2.72
7	3.59	3.26	3.07	2.96	2.88	2.83	2.78	2.75	2.72	2.70	2.67	2.63	2.59	2.58	2.56	2.54	2.51	2.49	2.47
8	3.46	3.11	2.92	2.81	2.73	2.67	2.62	2.59	2.56	2.54	2.50	2.46	2.42	2.40	2.38	2.36	2.34	2.32	2.29
9	3.36	3.01	2.81	2.69	2.61	2.55	2.51	2.47	2.44	2.42	2.38	2.34	2.30	2.28	2.25	2.23	2.21	2.18	2.16
10	3.28	2.92	2.73	2.61	2.52	2.46	2.41	2.38	2.35	2.32	2.28	2.24	2.20	2.18	2.16	2.13	2.11	2.08	2.06
11	3.23	2.86	2.66	2.54	2.45	2.39	2.34	2.30	2.27	2.25	2.21	2.17	2.12	2.10	2.08	2.05	2.03	2.00	1.97
12	3.18	2.81	2.61	2.48	2.39	2.33	2.28	2.24	2.21	2.19	2.15	2.10	2.06	2.04	2.01	1.99	1.96	1.93	1.90
13	3.14	2.76	2.56	2.43	2.35	2.28	2.23	2.20	2.16	2.14	2.10	2.05	2.01	1.98	1.96	1.93	1.90	1.88	1.85
14	3.10	2.73	2.52	2.39	2.31	2.24	2.19	2.15	2.12	2.10	2.05	2.01	1.96	1.94	1.91	1.89	1.86	1.83	1.80
15	3.07	2.70	2.49	2.36	2.27	2.21	2.16	2.12	2.09	2.06	2.02	1.97	1.92	1.90	1.87	1.85	1.82	1.79	1.76
16	3.05	2.67	2.46	2.33	2.24	2.18	2.13	2.09	2.06	2.03	1.99	1.94	1.89	1.87	1.84	1.81	1.78	1.75	1.72
17	3.03	2.64	2.44	2.31	2.22	2.15	2.10	2.06	2.03	2.00	1.96	1.91	1.86	1.84	1.81	1.78	1.75	1.72	1.69
18	3.01	2.62	2.42	2.29	2.20	2.13	2.08	2.04	2.00	1.98	1.93	1.89	1.84	1.81	1.78	1.75	1.72	1.69	1.66
19	2.99	2.61	2.40	2.27	2.18	2.11	2.06	2.02	1.98	1.96	1.91	1.86	1.81	1.79	1.76	1.73	1.70	1.67	1.63
20	2.97	2.59	2.38	2.25	2.16	2.09	2.04	2.00	1.96	1.94	1.89	1.84	1.79	1.77	1.74	1.71	1.68	1.64	1.61
21	2.96	2.57	2.37	2.23	2.14	2.08	2.02	1.98	1.95	1.92	1.88	1.83	1.78	1.75	1.72	1.69	1.66	1.62	1.59
22	2.95	2.56	2.35	2.22	2.13	2.06	2.01	1.97	1.93	1.90	1.86	1.81	1.76	1.73	1.70	1.67	1.64	1.60	1.57
23	2.94	2.55	2.34	2.21	2.11	2.05	1.99	1.95	1.92	1.89	1.84	1.80	1.74	1.72	1.69	1.66	1.62	1.59	1.55
24	2.93	2.54	2.33	2.19	2.10	2.04	1.98	1.94	1.91	1.88	1.83	1.78	1.73	1.70	1.67	1.64	1.61	1.57	1.53
25	2.92	2.53	2.32	2.18	2.09	2.02	1.97	1.93	1.89	1.87	1.82	1.77	1.72	1.69	1.66	1.63	1.59	1.56	1.52
26	2.91	2.52	2.31	2.17	2.08	2.01	1.96	1.92	1.88	1.86	1.81	1.76	1.71	1.68	1.65	1.61	1.58	1.54	1.50
27	2.90	2.51	2.30	2.17	2.07	2.00	1.95	1.91	1.87	1.85	1.80	1.75	1.70	1.67	1.64	1.60	1.57	1.53	1.49
28	2.89	2.50	2.29	2.16	2.06	2.00	1.94	1.90	1.87	1.84	1.79	1.74	1.69	1.66	1.63	1.59	1.56	1.52	1.48
29	2.89	2.50	2.28	2.15	2.06	1.99	1.93	1.89	1.86	1.83	1.78	1.73	1.68	1.65	1.62	1.58	1.55	1.51	1.47
30	2.88	2.49	2.28	2.14	2.05	1.98	1.93	1.88	1.85	1.82	1.77	1.72	1.67	1.64	1.61	1.57	1.54	1.50	1.46
40	2.84	2.44	2.23	2.09	2.00	1.93	1.87	1.83	1.79	1.76	1.71	1.66	1.61	1.57	1.54	1.51	1.47	1.42	1.38
60	2.79	2.39	2.18	2.04	1.95	1.87	1.82	1.77	1.74	1.71	1.66	1.60	1.54	1.51	1.48	1.44	1.40	1.35	1.29
120	2.75	2.35	2.13	1.99	1.90	1.82	1.77	1.72	1.68	1.65	1.60	1.54	1.48	1.45	1.41	1.37	1.32	1.26	1.19
∞	2.71	2.30	2.08	1.94	1.85	1.77	1.72	1.67	1.63	1.60	1.55	1.49	1.42	1.38	1.34	1.30	1.24	1.17	1.00

per cent points

df_2 \ df_1	1	2	3	4	5	6	7	8	9	10	12	15	20	24	30	40	60	120	∞
5	6.61	5.79	5.41	5.19	5.05	4.95	4.88	4.82	4.77	4.74	4.68	4.62	4.56	4.53	4.50	4.46	4.43	4.40	4.36
6	5.99	5.14	4.76	4.53	4.39	4.28	4.21	4.15	4.10	4.06	4.00	3.94	3.87	3.84	3.81	3.77	3.74	3.70	3.67
7	5.59	4.74	4.35	4.12	3.97	3.87	3.79	3.73	3.68	3.64	3.57	3.51	3.44	3.41	3.38	3.34	3.30	3.27	3.23
8	5.32	4.46	4.07	3.84	3.69	3.58	3.50	3.44	3.39	3.35	3.28	3.22	3.15	3.12	3.08	3.04	3.01	2.97	2.93
9	5.12	4.26	3.86	3.63	3.48	3.37	3.29	3.23	3.18	3.14	3.07	3.01	2.94	2.90	2.86	2.83	2.79	2.75	2.71
10	4.96	4.10	3.71	3.48	3.33	3.22	3.14	3.07	3.02	2.98	2.91	2.84	2.77	2.74	2.70	2.66	2.62	2.58	2.54
11	4.84	3.98	3.59	3.36	3.20	3.09	3.01	2.95	2.90	2.85	2.79	2.72	2.65	2.61	2.57	2.53	2.49	2.45	2.40
12	4.75	3.89	3.49	3.26	3.11	3.00	2.91	2.85	2.80	2.75	2.69	2.62	2.54	2.51	2.47	2.43	2.38	2.34	2.30
13	4.67	3.81	3.41	3.18	3.03	2.92	2.83	2.77	2.71	2.67	2.60	2.53	2.46	2.42	2.38	2.34	2.30	2.25	2.21
14	4.60	3.74	3.34	3.11	2.96	2.85	2.76	2.70	2.65	2.60	2.53	2.46	2.39	2.35	2.31	2.27	2.22	2.18	2.13
15	4.54	3.68	3.29	3.06	2.90	2.79	2.71	2.64	2.59	2.54	2.48	2.40	2.33	2.29	2.25	2.20	2.16	2.11	2.07
16	4.49	3.63	3.24	3.01	2.85	2.74	2.66	2.59	2.54	2.49	2.42	2.35	2.28	2.24	2.19	2.15	2.11	2.06	2.01
17	4.45	3.59	3.20	2.96	2.81	2.70	2.61	2.55	2.49	2.45	2.38	2.31	2.23	2.19	2.15	2.10	2.06	2.01	1.96
18	4.41	3.55	3.16	2.93	2.77	2.66	2.58	2.51	2.46	2.41	2.34	2.27	2.19	2.15	2.11	2.06	2.02	1.97	1.92
19	4.38	3.52	3.13	2.90	2.74	2.63	2.54	2.48	2.42	2.38	2.31	2.23	2.16	2.11	2.07	2.03	1.98	1.93	1.88
20	4.35	3.49	3.10	2.87	2.71	2.60	2.51	2.45	2.39	2.35	2.28	2.20	2.12	2.08	2.04	1.99	1.95	1.90	1.84
21	4.32	3.47	3.07	2.84	2.68	2.57	2.49	2.42	2.37	2.32	2.25	2.18	2.10	2.05	2.01	1.96	1.92	1.87	1.81
22	4.30	3.44	3.05	2.82	2.66	2.55	2.46	2.40	2.34	2.30	2.23	2.15	2.07	2.03	1.98	1.94	1.89	1.84	1.78
23	4.28	3.42	3.03	2.80	2.64	2.53	2.44	2.37	2.32	2.27	2.20	2.13	2.05	2.00	1.96	1.91	1.86	1.81	1.76
24	4.26	3.40	3.01	2.78	2.62	2.51	2.42	2.36	2.30	2.25	2.18	2.11	2.03	1.98	1.94	1.89	1.84	1.79	1.73
25	4.24	3.39	2.99	2.76	2.60	2.49	2.40	2.34	2.28	2.24	2.16	2.09	2.01	1.96	1.92	1.87	1.82	1.77	1.71
26	4.23	3.37	2.98	2.74	2.59	2.47	2.39	2.32	2.27	2.22	2.15	2.07	1.99	1.95	1.90	1.85	1.80	1.75	1.69
27	4.21	3.35	2.96	2.73	2.57	2.46	2.37	2.31	2.25	2.20	2.13	2.06	1.97	1.93	1.88	1.84	1.79	1.73	1.67
28	4.20	3.34	2.95	2.71	2.56	2.45	2.36	2.29	2.24	2.19	2.12	2.04	1.96	1.91	1.87	1.82	1.77	1.71	1.65
29	4.18	3.33	2.93	2.70	2.55	2.43	2.35	2.28	2.22	2.18	2.10	2.03	1.94	1.90	1.85	1.81	1.75	1.70	1.64
30	4.17	3.32	2.92	2.69	2.53	2.42	2.33	2.27	2.21	2.16	2.09	2.01	1.93	1.89	1.84	1.79	1.74	1.68	1.62
40	4.08	3.23	2.84	2.61	2.45	2.34	2.25	2.18	2.12	2.08	2.00	1.92	1.84	1.79	1.74	1.69	1.64	1.58	1.51
60	4.00	3.15	2.76	2.53	2.37	2.25	2.17	2.10	2.04	1.99	1.92	1.84	1.75	1.70	1.65	1.59	1.53	1.47	1.39
120	3.92	3.07	2.68	2.45	2.29	2.18	2.09	2.02	1.96	1.91	1.83	1.75	1.66	1.61	1.55	1.50	1.43	1.35	1.25
∞	3.84	3.00	2.60	2.37	2.21	2.10	2.01	1.94	1.88	1.83	1.75	1.67	1.57	1.52	1.46	1.39	1.32	1.22	1.00

TABLE II-8 (continued). Percentage points for the F distribution*

2.5. per cent points

df_2 \ df_1	1	2	3	4	5	6	7	8	9	10	12	15	20	24	30	40	60	120	∞
5	10.01	8.43	7.76	7.39	7.15	6.98	6.85	6.76	6.68	6.62	6.52	6.43	6.33	6.28	6.23	6.18	6.12	6.07	6.02
6	8.81	7.26	6.60	6.23	5.99	5.82	5.70	5.60	5.52	5.46	5.37	5.27	5.17	5.12	5.07	5.01	4.96	4.90	4.85
7	8.07	6.54	5.89	5.52	5.29	5.12	4.99	4.90	4.82	4.76	4.67	4.57	4.47	4.42	4.36	4.31	4.25	4.20	4.14
8	7.57	6.06	5.42	5.05	4.82	4.65	4.53	4.43	4.36	4.30	4.20	4.10	4.00	3.95	3.89	3.84	3.78	3.73	3.67
9	7.21	5.71	5.08	4.72	4.48	4.32	4.20	4.10	4.03	3.96	3.87	3.77	3.67	3.61	3.56	3.51	3.45	3.39	3.33
10	6.94	5.46	4.83	4.47	4.24	4.07	3.95	3.85	3.78	3.72	3.62	3.52	3.42	3.37	3.31	3.26	3.20	3.14	3.08
11	6.72	5.26	4.63	4.28	4.04	3.88	3.76	3.66	3.59	3.53	3.43	3.33	3.23	3.17	3.12	3.06	3.00	2.94	2.88
12	6.55	5.10	4.47	4.12	3.89	3.73	3.61	3.51	3.44	3.37	3.28	3.18	3.07	3.02	2.96	2.91	2.85	2.79	2.72
13	6.41	4.97	4.35	4.00	3.77	3.60	3.48	3.39	3.31	3.25	3.15	3.05	2.95	2.89	2.84	2.78	2.72	2.66	2.60
14	6.30	4.86	4.24	3.89	3.66	3.50	3.38	3.29	3.21	3.15	3.05	2.95	2.84	2.79	2.73	2.67	2.61	2.55	2.49
15	6.20	4.76	4.15	3.80	3.58	3.41	3.29	3.20	3.12	3.06	2.96	2.86	2.76	2.70	2.64	2.58	2.52	2.46	2.40
16	6.12	4.69	4.08	3.73	3.50	3.34	3.22	3.12	3.05	2.99	2.89	2.79	2.68	2.63	2.57	2.51	2.45	2.38	2.32
17	6.04	4.62	4.01	3.66	3.44	3.28	3.16	3.06	2.98	2.92	2.82	2.72	2.62	2.56	2.50	2.44	2.38	2.32	2.25
18	5.98	4.56	3.95	3.61	3.38	3.22	3.10	3.01	2.93	2.87	2.77	2.67	2.56	2.50	2.44	2.38	2.32	2.26	2.19
19	5.92	4.51	3.90	3.56	3.33	3.17	3.05	2.96	2.88	2.82	2.72	2.62	2.51	2.45	2.39	2.33	2.27	2.20	2.13
20	5.87	4.46	3.86	3.51	3.29	3.13	3.01	2.91	2.84	2.77	2.68	2.57	2.46	2.41	2.35	2.29	2.22	2.16	2.09
21	5.83	4.42	3.82	3.48	3.25	3.09	2.97	2.87	2.80	2.73	2.64	2.53	2.42	2.37	2.31	2.25	2.18	2.11	2.04
22	5.79	4.38	3.78	3.44	3.22	3.05	2.93	2.84	2.76	2.70	2.60	2.50	2.39	2.33	2.27	2.21	2.14	2.08	2.00
23	5.75	4.35	3.75	3.41	3.18	3.02	2.90	2.81	2.73	2.67	2.57	2.47	2.36	2.30	2.24	2.18	2.11	2.04	1.97
24	5.72	4.32	3.72	3.38	3.15	2.99	2.87	2.78	2.70	2.64	2.54	2.44	2.33	2.27	2.21	2.15	2.08	2.01	1.94
25	5.69	4.29	3.69	3.35	3.13	2.97	2.85	2.75	2.68	2.61	2.51	2.41	2.30	2.24	2.18	2.12	2.05	1.98	1.91
26	5.66	4.27	3.67	3.33	3.10	2.94	2.82	2.73	2.65	2.59	2.49	2.39	2.28	2.22	2.16	2.09	2.03	1.95	1.88
27	5.63	4.24	3.65	3.31	3.08	2.92	2.80	2.71	2.63	2.57	2.47	2.36	2.25	2.19	2.13	2.07	2.00	1.93	1.85
28	5.61	4.22	3.63	3.29	3.06	2.90	2.78	2.69	2.61	2.55	2.45	2.34	2.23	2.17	2.11	2.05	1.98	1.91	1.83
29	5.59	4.20	3.61	3.27	3.04	2.88	2.76	2.67	2.59	2.53	2.43	2.32	2.21	2.15	2.09	2.03	1.96	1.89	1.81
30	5.57	4.18	3.59	3.25	3.03	2.87	2.75	2.65	2.57	2.51	2.41	2.31	2.20	2.14	2.07	2.01	1.94	1.87	1.79
40	5.42	4.05	3.46	3.13	2.90	2.74	2.62	2.53	2.45	2.39	2.29	2.18	2.07	2.01	1.94	1.88	1.80	1.72	1.64
60	5.29	3.93	3.34	3.01	2.79	2.63	2.51	2.41	2.33	2.27	2.17	2.06	1.94	1.88	1.82	1.74	1.67	1.58	1.48
120	5.15	3.80	3.23	2.89	2.67	2.52	2.39	2.30	2.22	2.16	2.05	1.94	1.82	1.76	1.69	1.61	1.53	1.43	1.31
∞	5.02	3.69	3.12	2.79	2.57	2.41	2.29	2.19	2.11	2.05	1.94	1.83	1.71	1.64	1.57	1.48	1.39	1.27	1.00

1 per cent points

df_2 \ df_1	1	2	3	4	5	6	7	8	9	10	12	15	20	24	30	40	60	120	∞
5	16.26	13.27	12.06	11.39	10.97	10.67	10.46	10.29	10.16	10.05	9.89	9.72	9.55	9.47	9.38	9.29	9.20	9.11	9.02
6	13.74	10.92	9.78	9.15	8.75	8.47	8.26	8.10	7.98	7.87	7.72	7.56	7.40	7.31	7.23	7.14	7.06	6.97	6.88
7	12.25	9.55	8.45	7.85	7.46	7.19	6.99	6.84	6.72	6.62	6.47	6.31	6.16	6.07	5.99	5.91	5.82	5.74	5.65
8	11.26	8.65	7.59	7.01	6.63	6.37	6.18	6.03	5.91	5.81	5.67	5.52	5.36	5.28	5.20	5.12	5.03	4.95	4.86
9	10.56	8.02	6.99	6.42	6.06	5.80	5.61	5.47	5.35	5.26	5.11	4.96	4.81	4.73	4.65	4.57	4.48	4.40	4.31
10	10.04	7.56	6.55	5.99	5.64	5.39	5.20	5.06	4.94	4.85	4.71	4.56	4.41	4.33	4.25	4.17	4.08	4.00	3.91
11	9.65	7.21	6.22	5.67	5.32	5.07	4.89	4.74	4.63	4.54	4.40	4.25	4.10	4.02	3.94	3.86	3.78	3.69	3.60
12	9.33	6.93	5.95	5.41	5.06	4.82	4.64	4.50	4.39	4.30	4.16	4.01	3.86	3.78	3.70	3.62	3.54	3.45	3.36
13	9.07	6.70	5.74	5.21	4.86	4.62	4.44	4.30	4.19	4.10	3.96	3.82	3.66	3.59	3.51	3.43	3.34	3.25	3.17
14	8.86	6.51	5.56	5.04	4.70	4.46	4.28	4.14	4.03	3.94	3.80	3.66	3.51	3.43	3.35	3.27	3.18	3.09	3.00
15	8.68	6.36	5.42	4.89	4.56	4.32	4.14	4.00	3.89	3.80	3.67	3.52	3.37	3.29	3.21	3.13	3.05	2.96	2.87
16	8.53	6.23	5.29	4.77	4.44	4.20	4.03	3.89	3.78	3.69	3.55	3.41	3.26	3.18	3.10	3.02	2.93	2.84	2.75
17	8.40	6.11	5.18	4.67	4.34	4.10	3.93	3.79	3.68	3.59	3.46	3.31	3.16	3.08	3.00	2.92	2.83	2.75	2.65
18	8.29	6.01	5.09	4.58	4.25	4.01	3.84	3.71	3.60	3.51	3.37	3.23	3.08	3.00	2.92	2.84	2.75	2.66	2.57
19	8.18	5.93	5.01	4.50	4.17	3.94	3.77	3.63	3.52	3.43	3.30	3.15	3.00	2.92	2.84	2.76	2.67	2.58	2.49
20	8.10	5.85	4.94	4.43	4.10	3.87	3.70	3.56	3.46	3.37	3.23	3.09	2.94	2.86	2.78	2.69	2.61	2.52	2.42
21	8.02	5.78	4.87	4.37	4.04	3.81	3.64	3.51	3.40	3.31	3.17	3.03	2.88	2.80	2.72	2.64	2.55	2.46	2.36
22	7.95	5.72	4.82	4.31	3.99	3.76	3.59	3.45	3.35	3.26	3.12	2.98	2.83	2.75	2.67	2.58	2.50	2.40	2.31
23	7.88	5.66	4.76	4.26	3.94	3.71	3.54	3.41	3.30	3.21	3.07	2.93	2.78	2.70	2.62	2.54	2.45	2.35	2.26
24	7.82	5.61	4.72	4.22	3.90	3.67	3.50	3.36	3.26	3.17	3.03	2.89	2.74	2.66	2.58	2.49	2.40	2.31	2.21
25	7.77	5.57	4.68	4.18	3.86	3.63	3.46	3.32	3.22	3.13	2.99	2.85	2.70	2.62	2.54	2.45	2.36	2.27	2.17
26	7.72	5.53	4.64	4.14	3.82	3.59	3.42	3.29	3.18	3.09	2.96	2.82	2.66	2.58	2.50	2.42	2.33	2.23	2.13
27	7.68	5.49	4.60	4.11	3.78	3.56	3.39	3.26	3.15	3.06	2.93	2.78	2.63	2.55	2.47	2.38	2.29	2.20	2.10
28	7.64	5.45	4.57	4.07	3.75	3.53	3.36	3.23	3.12	3.03	2.90	2.75	2.60	2.52	2.44	2.35	2.26	2.17	2.06
29	7.60	5.42	4.54	4.04	3.73	3.50	3.33	3.20	3.09	3.00	2.87	2.73	2.57	2.49	2.41	2.33	2.23	2.14	2.03
30	7.56	5.39	4.51	4.02	3.70	3.47	3.30	3.17	3.07	2.98	2.84	2.70	2.55	2.47	2.39	2.30	2.21	2.11	2.01
40	7.31	5.18	4.31	3.83	3.51	3.29	3.12	2.99	2.89	2.80	2.66	2.52	2.37	2.29	2.20	2.11	2.02	1.92	1.80
60	7.08	4.98	4.13	3.65	3.34	3.12	2.95	2.82	2.72	2.63	2.50	2.35	2.20	2.12	2.03	1.94	1.84	1.73	1.60
120	6.85	4.79	3.95	3.48	3.17	2.96	2.79	2.66	2.56	2.47	2.34	2.19	2.03	1.95	1.86	1.76	1.66	1.53	1.38
∞	6.63	4.61	3.78	3.32	3.02	2.80	2.64	2.51	2.41	2.32	2.18	2.04	1.88	1.79	1.70	1.59	1.47	1.32	1.00

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CHAPTER III

Plot or Pen Technique

The subject of "plot technique" or "pen technique" is defined herein as the topic dealing with the effect of individual items upon each other and of experimental units[1] upon each other (competition), the size and shape of the experimental unit, the number of experimental units per treatment, and methods of sampling the experimental unit. Although the term "pen technique" is not commonly used, it could be used to describe technique for animal experiments in much the same way that the term "plot technique" is used for plant experiments. The problems may be different, but the principles are usually related if not identical. Similarly, the term "laboratory technique" could be applied to procedural methods applicable to laboratory experiments.

All items described above, as well as others, are interrelated and should be discussed as one subject rather than as individual topics. For example, the required number of replicates for a specified degree of precision is dependent upon the size, shape, and relative cost of the experimental units; the extent and nature of competition; the number and nature of the treatments in the experiment; the nature of the experimental material; the percentage sampled; and the particular experimental design selected. Each item may be discussed individually by holding the other elements constant. For a particular design and set of treatments and a given size and shape of experimental unit with no competition present the relationship between the fraction of the experimental unit sampled and number of replicates could be studied. Likewise, we could hold all other items constant and study the effect on the standard error of the mean by changing one item, such as plot size.

In the following sections, several of the above items are discussed separately and collectively, but the reader should always bear in mind the interrelationships present among the various elements of plot, laboratory, or pen technique. A rather complete account of the above subjects as related to field husbandry is given by Love [196] and by Leonard and Clark [193].

[1]The experimental unit is the total amount of material to which one treatment is applied in a single replicate. The sampling unit is that fraction or part of the experiment unit selected for a single observation or sample.

III-1 Competition

Competition is defined as the effect of one individual or set of individuals upon another individual or set of individuals. The term as used in this text has a broad meaning. Similarly, the terms "treatment" and "yield" often have broad definitions in experimental design literature. In example I-1, "competition" was introduced into the experiment with the "balanced" arrangement that required that all sample means be equal; this could be called *induced competition*; the variation among individuals is larger than it should be; i.e., an additional component of variance has been added to that due to the random variation among individuals.

There are two kinds of competition of importance in designing experiments and in testing the significance of the results; these are intra-experimental unit competition and inter-experimental unit competition. The former term refers to the effect of individuals upon each other *within* the experimental unit, while the latter refers to the effect of experimental units upon each other.

III-1.1 INTRA-EXPERIMENTAL UNIT COMPETITION

Intra-experimental unit competition exists when individuals within the unit affect each other either advantageously or detrimentally. Examples of this type of competition abound in experimental work; for example, plants in a field plot are in competition for light, water, and nutrients; animals within a pen or cage are in competition for food and water; workers in the laboratory may have more results alike when working together than when working alone; some animals in a pen are more pugnacious than others, often causing some physical injury to other animals, etc. If the variance among experimental units is utilized to test treatment differences, the intra-plot competition has no effect upon the experimental technique as long as the sum of the competition effects within the unit is zero; this requires that one individual gains while another loses. If the variation among individuals or sampling units within the experimental units is utilized in tests of significance, the error variance may be wrongly estimated because of competition.

More space or more time between individuals or units may be utilized to overcome intra-experimental unit competition. In plant work, this may require more space between plants. In laboratory work the technicians could work on different samples, be placed in individual booths, or be given the proper training to eliminate "competition." It is often desired to estimate the environmental and the genetic components of variance among individuals within an experimental unit. If intra-experimental unit competition is present, it may be impossible to estimate the genetic component of variance. The objectives of the experiment and the choice of the experimental error variance will determine whether or not intra-plot competition is to be ignored.

Suppose that there are h sampling units or individuals per experimental unit and that k of the h units are drawn at random. The variance of the mean

of the k sampling units is $V_p + V_s/k$, where V_p represents the variation among experimental units treated similarly and V_s is the variance among sampling units within the experimental unit; V_s varies independently from experimental unit to experimental unit [335]. If competition exists, the effects, c_i, add to zero within the plot and have a variance equal to V_c. The variance among experimental units then becomes

$$V_p + \frac{V_s}{k} + \left(\frac{1}{k} - \frac{1}{h}\right)V_c = \left(V_p - \frac{V_c}{h}\right) + \frac{1}{k}(V_s + V_c) = V_p' + \frac{V_s'}{k}. \quad \text{(III-1)}$$

Sample estimates of V_s' and V_p' are obtained from data of the following form:

Source of variation	df	Mean square	
		Experimental	Average value
Among experimental units	$n - 1$	MS_0	$V_s' + kV_p'$
Among sampling units within experimental units	$n(k - 1)$	MS_s	V_s'

If all of the variation within the experimental unit is due to competition, equation (III-1) becomes

$$V_p + \left(\frac{1}{k} - \frac{1}{h}\right)V_c = V_p' + V_c/k. \quad \text{(III-2)}$$

With no competition, we obtain

$$V_p + V_s/k. \quad \text{(III-3)}$$

In the third limiting situation, if the whole of the variation is V_p, there is no competition and no variation among sampling units or individuals within the experimental units [335].

In experiments where subsampling is practiced, the effect of competition cannot be disentangled from the other components of variance, since only estimates of V_p' and V_s' are available. If V_p' is significantly negative, the effect of competition is demonstrated [335]. The variance due to competition cannot be estimated in ordinary sampling experiments unless the experiment is specifically designed to measure intra-experimental unit competition.

III-1.2 INTER-EXPERIMENTAL UNIT COMPETITION

As explained previously, inter-experimental unit competition refers to advantageous or deleterious influences of one experimental unit upon another one. In keeping with the general meaning of competition, "border" or "alley" effect (the effect on plants or animals caused by adjacent unplanted or uninhabited areas) has been included under inter-plot competition. The influence of borders and alleys on the experimental unit may be removed from an experiment by removing the borders and alleys or by using guard material which is discarded.

It has been found that some crop varieties adversely affect other varieties grown adjacent to them (e.g., tall and short varieties) [193, 196]. Some varieties apparently are better "competitors" than others. Plants on a plot with no fertilizer treatment or with no irrigation treatment may draw some of their nutrients or water from the treated plots. Animals in the end pens or cages of individual feeding trials may not fare as well, since they lack "company" [98]. Likewise, laboratory technicians may not perform as efficiently if completely isolated from other technicians.

The simple expedient of providing more or less separation (either through time or space) of experimental units is usually sufficient to remove the effect of competition. If competition effects are likely, the experimenter might widen the distance between rows and decrease the distance between plants within the rows. In this way the same number of plants per acre is maintained. The intra-row competition would be severe, but there would be negligible inter-row competition. If the row is the experimental unit and is harvested completely, the competition component of variance may be essentially eliminated from the experimental error variance. Also, such a procedure tends to correct for slight variations in stand. Competition may be controlled in the manner suggested above or by border or guard material which is discarded at harvest. However, the ratio of guard or border material to experimental material should always be considered relative to alternatives. The cost of maintaining the guard material relative to the cost of maintaining the experimental unit is considered in section III-2.2.2.

III-2 Size of Experimental Unit

III-2.1 FACTORS AFFECTING THE SIZE OF THE EXPERIMENTAL UNIT

The optimum or recommended size for an experimental unit cannot be given without first considering a number of factors. An attempt is made to include all factors, but undoubtedly some are omitted in the following list:

(i) Practical considerations: Certain practical aspects may dictate the size of an experimental unit. In animal experiments the pens or cages may be already constructed and not easily changed. The pasture or paddock size may be determined by those already available and fenced. The size of a field plot may be determined by the facilities available for handling plots. If grain combines and other power equipment are used, a fairly large plot may be essential. On the other hand, power equipment may be unavailable and the experimenter is forced either to use small plots or to subsample. In other experiments, e.g., greenhouse studies, the facilities may dictate the use of small experimental units. The total amount of experimental material available is a factor in determining the optimum size of a unit. The experimenter may have only a fixed number of animals, greenhouse pots or flats, or of plants or seed, or a fixed area of land available to him. If the size of the experimental unit and of the experi-

mental area is fixed, the number of experimental units is *automatically determined.*

(ii) Nature of experimental material: The type of experimental material is associated with the practical considerations discussed above, but some additional features are worth noting. The particular material under study will determine plot or pen size to a large extent. Certain types of variability and cultural operations are associated with the kind of crop or animal. The optimum plot size for a variety yield trial for oats is different from that for corn. Likewise, the pen or cage size for chickens is not the same as for beef cattle.

(iii) Number of treatments per block or per incomplete block: The number of treatments per block or per incomplete block may be kept within rather narrow limits simply by the choice of a design. If large numbers of treatments are to be tested, an incomplete block design (Chapters IX-XIII) may be used. For a given design, say the randomized complete block (Chapter V), and a variable number of treatments, smaller plots may be selected for the larger number of treatments.

(iv) Variability among individuals or units within the experimental unit relative to the variability among experimental units treated alike: The variance of a treatment mean, $s_{\bar{x}_i}^2$, is proportional (equal in most cases) to

$$\frac{V_p'}{r} + \frac{V_s'}{rk}, \tag{III-4}$$

where V_p', V_s', and k are defined in equation (III-1) and r represents the number of experimental units per treatment. The relative size of the two components of variance V_p' and V_s' has considerable effect upon the optimum experimental unit size.

(v) Cost per individual or unit relative to cost of experimental units: Let C_s be the cost of an individual item within the experimental unit which is independent of the cost of the experimental units, and let C_p be the cost of the experimental unit independent of the number of individuals in the unit. Then, the cost per treatment is

$$rkC_s + rC_p = C_t. \tag{III-5}$$

The total cost for an experiment involving v treatments is $vC_t = C$. The optimum size for an experimental unit will depend upon the ratio of the costs.

III-2.2 METHODS OF DETERMINING THE SIZE OF THE EXPERIMENTAL UNIT

Several methods for determining optimum size and shape of plot have been suggested from time to time [e.g., see references listed in 113, 193, 196, 258], but only two of these methods, "maximum curvature" and "Fairfield Smith's Variance Law," are discussed here.

III-2.2.1 Maximum curvature method. The maximum curvature method, so named because of the manner in which the optimum size of plot is

determined, has frequently been used to determine optimum plot size for various field crops. A field of the crop is harvested in small units of size x. The units are combined into various sizes. The sums of squares among the various plots for a given size are computed and divided by the degrees of freedom to obtain the variances. The various plot size means are also computed. From these results the coefficients of variation are computed. The coefficients of variation are plotted against the respective plot sizes. A freehand curve is drawn through the resulting coordinates, and the point of maximum curvature is located by inspection. The optimum plot size is the one just beyond the point of maximum curvature.

The method has two weaknesses: (i) the relative costs of various plot sizes are not considered and (ii) the point of maximum curvature is not independent of the smallest unit selected or of the scale of measurement used. For certain types of material, e.g., where the smallest unit within the experimental unit is a hill of corn, an animal, a single determination or reading, etc., the choice of the unit is fixed. The maximum curvature method is useful for this type of material if the scale of measurement is fixed. It is not useful for material with an arbitrary unit of observation (e.g., a fixed length of a row of a crop) or an arbitrary scale of measurement.

III-2.2.2 Fairfield Smith's Variance Law. An empirical relationship between plot size and plot variance was developed by Smith [267]. Mahalanobis [204] developed essentially the same equation in his studies on sample surveys in India. The variance law is expressed by the equation,

$$\log V_x = \log V_1 - b' \log x, \tag{III-6}$$

where V_x is the variance of yield per unit area among plots (experimental units) of size x units, V_1 is the variance among plots of size unity, and b', the regression coefficient, indicates the relationship between adjacent individuals or units. The limiting values of the regression coefficient are zero and one *unless inter-experimental unit competition is present.* If the experimental unit is composed of a random selection of x individuals, $b' = 1$, and if the x individuals are identical, $b' = 0$. In field work, there is likely to be a correlation between adjacent plots, resulting in values of b' less than unity. Smith [267] computed b''s for thirty-eight different sets of uniformity trial data and found that most of the regression coefficients fell within the range 0.2 to 0.8.

If shape is ignored and if a block of v treatments is selected, the following equation defines the relationship between the constants b and $(V_1)_\infty$ from a field with an infinite number of blocks and the estimates $b'$and V_1 obtained from the data (see formulae (III-13) to (III-16)):

$$(V_x)_v = \frac{(1 - v^{-b})(V_1)_\infty}{(1 - 1/v)x^b}, \tag{III-7}$$

where $(V_x)_v$ is the variance per unit of size x for a block of v treatments, b is

the value of b' adjusted for field size, and $(V_1)_\infty$ is the variance for an infinite field with experimental units of size unity. The adjustments for b' may be obtained from a table prepared by Smith [267]. The cost per experimental unit as defined by equation (III-5) is

$$xC_s + C_p = C_t/r. \tag{III-8}$$

From these relationships the optimum size of plot or experimental unit is found to be [267; see section III-2.2.3]:

$$x = bC_p/(1 - b)C_s. \tag{III-9}$$

The formula for estimating the most efficient plot size for any given experiment, i.e., for a given number of treatments and a particular experimental design, is unaffected by the number of plots. The number of plots or experimental units per treatment, r, depends upon the area or material available or the degree of precision desired.

If guard area is required around the experimental material and if the cost equation is of the form,

$$C_p + xC_s + C_g(A + Bx), \tag{III-10}$$

where C_g is the cost of maintaining the guard or border material, B is the ratio of guard material to test material, and A is any additional cost, Smith [267] has shown that the optimum plot size is given by the equation,

$$x = \frac{b(C_p + C_gA)}{(1 - b)(C_s + C_gB)}. \tag{III-11}$$

The value of b in the range .3 to .7 does not greatly affect the increase in cost or in variance when plots of size one-fourth to four times the optimum plot size are used. On the basis of these results, plot sizes of one-half to twice the optimum size result in small losses in efficiency. However, for plot sizes of one-fourth or four times the optimum size a loss in efficiency of about 20 per cent results because of the increased variance.

Robinson *et al.* [258] and Wassom and Kalton [304] have applied Smith's results to uniformity trial data on peanuts and on brome grass, respectively. These authors used percentage costs rather than actual costs. The use of percentage costs may be misleading if the size of the smallest unit is not taken into consideration.

In animal experimentation the value of b for a particular trait sometimes may be obtained from genetic theory if knowledge of the relationships among the animals in the experiment is available. In such cases the b values instead of the estimated b' values would be available and would not require adjustment for the size of the group of experimental animals.

III-2.2.3 Some mathematical developments related to "Fairfield Smith's Variance Law." It is deemed advisable to recapitulate some of the

mathematical results obtained by Smith [267]. These are not included in the previous section because they are not essential to users of the methods but may be of interest to students of statistics.

In equation (III-6), b' is the regression of $\log s_{x_i}^2$ on $\log x_i$. Since the $s_{x_i}^2$, the individual sample variances on a per unit basis, usually have variable numbers of degrees of freedom, and therefore different variances, a weighted regression is preferred. The weighted regression coefficient, b', is obtained from the formula,

$$\frac{\sum w_i \log s_{x_i}^2 \log x_i - \sum w_i \log s_{x_i}^2 \sum w_i \log x_i / \sum w_i}{\sum w_i (\log x_i)^2 - (\sum w_i \log x_i)^2 / \sum w_i}, \tag{III-12}$$

where w_i = the degrees of freedom associated with a given variance. The variance of $\log s_x^2$ to a first approximation[1] is $2/w_i$. The weights are proportional to the reciprocal of the variance.

Equation (III-6) may be written in the form,

$$\log (V_x)_{n/x} = \log (V_1)_n - b' \log x, \tag{III-13}$$

where $v = n/x$ = the number of plots per block, n = the size of the field, $(V_x)_v$ denotes that V_x is measured over v plots of size x, and x = the size of the plot. For an infinitely large field the law is

$$\log (V_x)_\infty = \log (V_1)_\infty - b \log x. \tag{III-14}$$

Since the block is a large plot, the variance between blocks may be estimated in the same way. The analysis of variance table for r blocks of size v may be represented as follows:

Source of variation	df	ms
Among blocks	$r - 1$	$v(V_{xv})_r$
Within blocks	$r(v - 1)$	$(V_x)_v$
Total	$rv - 1$	$(V_x)_{rv}$

If r tends to infinity and if (III-14) is used to relate $(V_{xv})_r$ and $(V_x)_{rv}$ we obtain

$$\lim_{r \to \infty}\left((V_x)_v = \frac{(rv - 1)(V_x)_{rv} - (r - 1)v(V_{xv})_r}{r(v - 1)}\right) = \left(\frac{1 - v^{-b}}{1 - 1/v}\right)\left(\frac{(V_1)_\infty}{x^b}\right). \tag{III-15}$$

For a given number of plots per block, v, and a given value of b the ratio $(V_x)_v/(V_x)_\infty$ is constant.

[1]The variance of $\log s_{x_i}^2$ is approximated by the first term in a Taylor series expansion. A closer approximation to a variance is obtained from using additional terms in the Taylor series expansion. Since the $s_{x_i}^2$ are not independent and since b' varies from year to year on the same field, even a graphical estimate of b may be sufficiently accurate.

Equation (III-13) is now rewritten as

$$\log (V_x)_{n/x} = \log (V_1)_\infty - b \log x + \log \frac{1 - v^{-b}}{1 - 1/v}, \qquad \text{(III-16)}$$

where $v = n/x$ is now variable indicating how V_x as usually evaluated from uniformity trial data is measured over variable block sizes in terms of plot numbers. Since the value of b is affected by the size of field and since equation (III-16) exhibits a tendency for curvature for values of $x/n \geq 0.1$, Smith [267] provides corrections for converting the estimated regression coefficients into equivalent regression coefficients. For $.01 > x/n > .001$ the corrections for $b' \geq .5$ are negligible but become larger for $b' < .5$. When $.01 < x/n < .1$ the corrections become important for $b' \leq .7$.

In determining the optimum plot size, we wish to maximize the amount of information per unit cost. The amount of information is defined to be the reciprocal of the variance. We could also minimize the relative cost per unit of information. Using the variance obtained in equation (III-15) and the cost function given in equation (III-8), the cost per unit of information is

$$C_I = \frac{(xC_s + C_p)}{1/(V_x)_v} = \frac{(xC_s + C_p)(1 - v^{-b})(V_1)_\infty}{(1 - 1/v)x^b}. \qquad \text{(III-17)}$$

If we take the derivative of C_I with respect to x and set the result equal to zero, we obtain

$$\frac{(V_1)_\infty(1 - v^{-b})}{(1 - 1/v)} \left\{ -b(C_p + xC_s)x^{-1-b} + x^{-b}C_s \right\} = 0. \qquad \text{(III-18)}$$

Solution of (III-18) for x results in equation (III-9). Likewise, if we use the cost relation described by equation (III-10) and proceed as above, the solution for x as given by (III-11) results.

If the optimum plot size is not utilized, it is desirable to know what losses in efficiency result [267]. The cost per unit of information, equation (III-17) is a minimum when $x_0 = bC_p/(1 - b)C_s$ = optimum plot size. When another plot size, x_1, is used, the relative cost per unit of information is

$$\frac{(C_p + x_1C_s)x_1^{-b}}{(C_p + x_0C_s)x_0^{-b}} = \left(\frac{x_0}{x_1}\right)^b \left\{1 - b + b\left(\frac{x_1}{x_0}\right)\right\} = \left(\frac{x_1}{x_0}\right)^{-b}(1 - b) + b\left(\frac{x_1}{x_0}\right)^{1-b}$$

$$= (1 - b)e^{-b \ln x_1/x_0} + be^{(1-b)\ln x_1/x_0}, \qquad \text{(III-19)}$$

where ln is the natural logarithm. Smith found that the last part of equation (III-19) is more convenient computationally.

III-2.2.4 Some additional results. If the material making up the experimental unit is composed of discrete units (e.g., animals in a pen or plants in a plot), if the number of units, k, included does not alter V_s', and if the variance among experimental units irrespective of number of sampling

units is V_p', the variance of a mean of r experimental units is

$$V_t = \frac{1}{r}\left\{V_p' + \frac{V_s'}{k}\right\}. \tag{III-20}$$

The optimum number of units, k, per experimental unit may be obtained with the procedure used to obtain equation (III-9). This is possible if the cost function is of the form $rf(k)$, where $f(k)$ is some function of k (e.g., equation (III-5)). If the cost function is not of this form, the cost per unit of information is not independent of the size of the experiment, and, hence, is not an appropriate criterion for fixing the size of the experiment.

The cost function usually considered is equation (III-5), where the cost per additional experimental unit is $C_p + kC_s$. In this case, maximization of the amount of information per unit of cost (or minimization of the reciprocal),

$$\frac{1}{C_tV_t} = k/(V_s' + kV_p')(C_p + kC_s), \tag{III-21}$$

leads to the solution of the optimum number of sampling units (animals [199] or plants) per experimental unit, thus:

$$k = \sqrt{C_pV_s'/C_sV_p'}. \tag{III-22}$$

In connection with the above, we could minimize cost for a fixed V_t or we could minimize V_t for a fixed C_t. Both procedures would lead to the same answer for r and k. If C_t is $rf(k)$, then r is fixed, and there will be a unique value of k which maximizes (III-21).

Another procedure is to use Lagrange multipliers to add the cost condition, differentiate, set equal to zero, and solve for the optimum plot size. For this case, we proceed as follows:

$$K^2\left(\frac{V_p'}{r} + \frac{V_s'}{rk}\right) - \lambda(C_t - rC_p - rkC_s) = \theta. \tag{III-23}$$

$$\frac{\partial\theta}{\partial k} = -K^2\frac{V_s'}{rk^2} + \lambda rC_s = 0. \tag{III-24}$$

$$\frac{\partial\theta'}{\partial r} = \frac{-K^2}{r^2}\left(V_p' + \frac{V_s'}{k}\right) + \lambda(C_p + kC_s) = 0. \tag{III-25}$$

$$\frac{\partial\theta}{\partial\lambda} = C_t - rC_p - rkC_s = 0. \tag{III-26}$$

Solving for k from equations (III-24 to 26) results in the solution given in equation (III-22).

Koch and Rigney [188] suggested that use be made of the mean squares from experiments with three or more sources of variation to determine optimum plot size. For example, the analysis of variance for a "split plot design" (Chapter X) contains three sources of variation that could be used,

i.e., the replicate or block mean square, the error (a) mean square, and the error (b) mean square. Likewise, a one-restrictional lattice design (Chapter XI) contains three mean squares: replicate or complete block, incomplete block (eliminating treatment effect), and intra-incomplete block; these may be utilized in a manner similar to that for the split plot design. Before computing the regression coefficient for Fairfield Smith's Variance Law, it is necessary to reconstruct the various mean squares in order to make them comparable to calculations for uniformity trial data [188].

In making use of the method of Koch and Rigney, two considerations are of importance. The first has to do with the composition of the experimental error mean square; if this mean square contains a replicate × treatment interaction component of variance in addition to a component of variance due to plots treated alike, the experimental error will be larger than a comparable mean square obtained from uniformity trial data. A replicate × variety interaction would invalidate this method for obtaining information on optimum plot size. The second consideration has to do with the selection of the replicate or blocks layout. Ordinarily an attempt is made to remove as much variation as possible with blocks. This is not the case for uniformity trial data. Thus, the optimum size would not be independent of the experimenter's ability to stratify the experimental material.

III-3 Shape of Experimental Unit

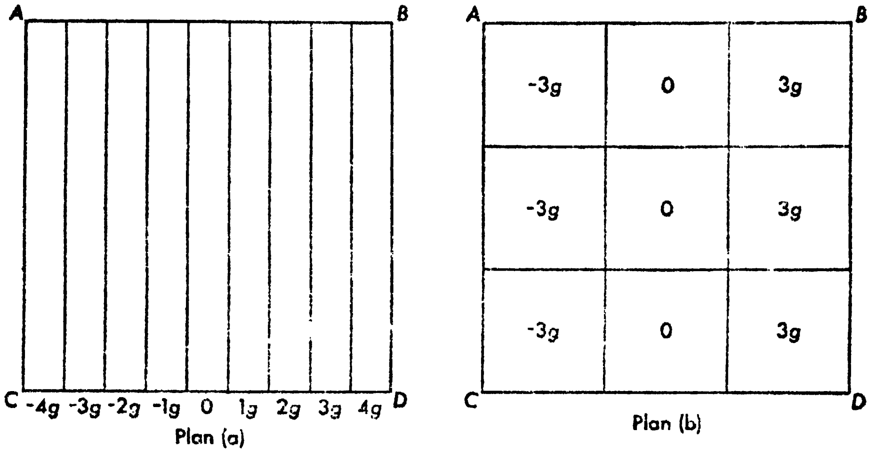
Theoretical considerations of the shape of an experimental unit may be found in the writings of Christidis [39, 40], Cochran [47], Ghosh [133], Jessen [166], and Taylor [286]. Jessen considered the shape as well as the size of the sampling unit in sample surveys; he obtained an empirical relation similar to Smith's [267].

For field work the results by Christidis [39, 40] and by Taylor [286] appear satisfactory. Taylor [286] presents a good literature review of the work on shape except that he failed to include the results by Christidis in 1939 [40]. As far as is known, shape of pen, paddock, or cage has not been studied. Therefore, the results discussed in this section apply only to field experiments.

For small sizes of plots the shape may have little or no effect, whereas for large plot sizes the effect of shape may be considerable. The optimum size and shape of plot will be the one resulting in the smallest variation among plots within a block.

Given a plot size and number of treatments, Cochran [47] has graphically presented the case for choosing the optimum plot shape (figure III-1). He considers that we have $v = 9$ treatments to compare and we wish to select the plot shape with the smallest average experimental error variance when the direction of the fertility gradient is unknown. The two extreme shapes selected are rectangular (plan (a)) and square (plan (b)) as shown in figure III-1. If we consider the case for which a linear gradient exists, we may picture the

results as shown. Suppose that the fertility gradient is parallel to AB or that the plots lie perpendicular to the gradient. The sum of squares among the nine plots would be $8\sigma_i^2 + 60g^2$. If the fertility gradient were parallel to AC, the plots would be parallel to the gradient and all plots would be equally affected by the gradient. In this case the sum of squares among the nine plots would be $8\sigma_i^2$.



If σ_i^2 is the random variance within blocks independent of the shape of plot, the mean squares for the two arrangements are

Fertility gradient	df	Average value of mean square plan (a)	plan (b)
Parallel to AC	8	σ_i^2	$\sigma_i^2 + \frac{54}{8}g^2$
Parallel to AB	8	$\sigma_i^2 + \frac{60}{8}g^2$	$\sigma_i^2 + \frac{54}{8}g^2$
Average	8	$\sigma_i^2 + \frac{30}{8}g^2$	$\sigma_i^2 + \frac{54}{8}g^2$

Figure III-1. Mean squares as affected by two plot shapes, rectangular and square [from Cochran, 47].

If the plots were square, as in plan (b), the sum of squares among plots would be the same for both cases. The average mean square for the long narrow plots is smaller than for square plots. The actual situation is probably somewhere in between the average values given in figure III-1, but the relationship between the two plot shapes still holds.

If the value of g is small, the selected plot shape is largely a matter of preference. For g large, long narrow plots should be selected, since the additional variance due to plots with other than optimum shape will need to be compensated for by additional replicates. It may prove impractical to use

long narrow plots because of the nature of the crop, and it may be less costly to increase plot size or the number of replicates to obtain the desired degree of precision.

The above discussion has assumed that the size of plot is given. If it is desired to obtain the optimum size and shape simultaneously, the reader is referred to the papers by Jessen [166] and Taylor [286].

III-4 Replication

Since variability is almost universal, replication (the repetition of the set of treatments in the experiment) is, or should be, practiced in nearly all experimental work. Fisher [126; Chapter I] states that the two conditions necessary to obtain a valid estimate of the experimental error are replication and randomization. Hence, replication is an important feature of experimental work. The need for replication was recognized by some workers as early as 1846 [193, 196].

Despite the fact that replication is used in experimental work, its meaning is not always clearly understood. If a treatment is applied to absolutely homogeneous material, there is no variation from experimental unit to experimental unit; in effect, only one replicate from the population of possible replicates is obtained even though several observations are made. Several readings on a single plot (or pen) do not result in replication on a treatment which is subject to environmental variation but merely result in decreasing the variance due to measuring or technique errors; the plot to plot or pen to pen variation has not been reduced. A single greenhouse, nutrient tank, or oven constitutes one replicate from a population of greenhouses, nutrient tanks, or ovens. In perennial pasture experiments, years do not represent true replications for perennial pastures grown on the same plot, whereas in annual pastures which are reseeded and reallocated every year the years constitute replication [46, 199].

A suitable criterion regarding the desired nature and extent of replication in experimental work might be formulated in the following terms. In the population to which we wish to make inferences concerning treatment differences, there will be a number of sources of variation affecting treatment differences. Some sources of variation affect the variability of treatment differences more than others. The variance of a treatment difference is reduced more rapidly by increasing the number of observations of treatment differences over the sources which contain a number of components of variance in the error variance. For example, the size of V_t in formula (III-20) may be reduced by increasing k or by increasing r. The latter value reduces both sources of variation, while increasing k does not affect the quantity V_p'/r.

III-4.1 SHAPE OF THE REPLICATE

Replicates or blocks should be set up to control as much of the variation as possible, resulting in the smallest experimental error variance. Not only the

location but also the shape of the block is of importance in field trials. In the absence of knowledge concerning gradients the average sum of squares among blocks will be largest for square blocks (see figure III-1); i.e., it is desired to have just the opposite effect for block shape as for plot shape. If any knowledge about variation is available, it should be utilized in setting up the blocks. If experiments are conducted on contoured land, it may be inadvisable to have square blocks if a block extends over more than one contour. A long narrow block may be the one containing the smallest within blocks variation.

The above consideration is directly applicable to pasture experiments. It is not known how shape of replicate affects the variation among cages or pens in other experiments on animals.

III-4.2 SIZE OF REPLICATE

The replicate size for a selected design is determined by the number of entries or treatments and the plot or pen size. The greater the number of entries the larger the replicate size unless the plot or pen size is reduced to compensate for the increased number of entries. The upper limit on replicate size will depend upon the nature of the material and treatments being tested as well as the character being measured. Since the experimental error usually increases with the number of entries per replicate or block, it is desirable not to have too many entries per block. Of course, the additional variation may be compensated for by increased replication, but this is costly. Another way of dealing with large numbers of entries is to use incomplete block designs which allow for removal of incomplete block variation within replicates. A study of the particular material under experimentation will allow the experimenter to formulate criteria for determining replicate size [199, 234, 258, 277, 332].

III-4.3 NUMBER OF REPLICATIONS

The number of replications for an experiment will be determined by

(i) degree of precision desired,
(ii) amount of variability present in the experimental material,
(iii) available resources, including personnel and equipment, and
(iv) size and shape of experimental unit.

The degree of precision desired depends upon the nature of the treatments and the characters observed as well as the expected magnitude of the treatment differences for a specified character. If differences are large, a low degree of precision may be accepted.

The degree of precision is defined to be the variability associated with the treatment mean (the variance of a treatment mean). Given that a specified treatment difference is of importance, the experimenter makes a decision concerning the risk he is willing to take for (i) asserting that a true difference of the stated size does not exist when it is actually present (type II error) and

(ii) asserting that a difference larger than the stated value exists when no true difference exists (type I error). The degree of precision desired may be expressed in per cent of the mean; i.e., it may be desired to have a standard error of a mean which is 3 per cent of the treatment mean.

In connection with determining the number of replicates for an experiment, the experimenter should list the characters of interest with their estimated standard deviations. The number of replicates is determined for the character of interest. If several characters are of importance, the number of replicates is determined for the most variable character. This number is larger than the required number of replicates for all other characters. If the size of treatment differences of interest varies and if different levels of confidence are desired for the various characters, it may be necessary to compute the required number of replicates for all characters individually and to choose a number large enough for the specified conditions.

The degree of variability present in the experimental material depends upon the treatment tested and the character measured. Some characters have a low degree of variability relative to other characters. For example, some chemical measurements may have a low degree of variation, whereas others are quite variable. The coefficient of variation is often a useful measure for yield characteristics, but is not useful for comparing the relative variation of two characteristics or two types of material since it is not independent of the location of the origin of measurements.

If the number of pens, the amount of material, or the area of land is fixed, the number of replicates is automatically determined. Also, the available personnel and equipment may be the limiting factors in determining the number of replicates.

However, if one wishes to compute the number of replicates required to detect a specified minimum difference between means at a specified significance level for a fixed plot size and shape or pen size, several methods are at his disposal. It is required that an estimate of the experimental variance be available and that the size of the minimum difference of interest be specified. Making use of the well-known statistic t, the number of replicates may be computed from the formula,

$$r = \frac{2t_\alpha^2 s^2}{d^2}, \tag{III-27}$$

where s^2 is the estimated experimental error variance, t_α is the t value at the α percentage level for the degrees of freedom associated with s^2, and d is the specified difference. Using the number of replicates, r, obtained from equation (III-27), a difference between two means as large as or larger than the specified difference d will be detected in approximately 50 per cent of the cases [147]. A 50-50 chance of detecting a specified difference is too low for most experimental situations. A number of procedures which give greater than a 50-50 chance of

detecting specified differences are available for determining sample size [20, 21, 60, 147, 192, 229, 277, 280, 285; 296, Ch. 18]. A theoretical discussion of the problem may be found in a number of references [147; 175, Ch. 12; 207, Ch. 6; 285]. Three of the procedures are described below.

III-4.3.1 The Harris-Horvitz-Mood method. Harris *et al.* present a method and the tables necessary for determining the sample size (number of replicates) required to obtain significance in a specified proportion of experiments, given that a difference as large as or larger than some values of d exists. It is assumed that the observations within each population are normally distributed, that all observations are subject to a common variance, and that an estimate of s_1^2 of the variance with df_1 degrees of freedom is available. Given the value of s_1^2, df_1, d (the specified size of the treatment difference), and the values of k' in tables III-1 and III-2, the number of replicates required to obtain significance at the α per cent level with a probability of approximately γ per cent when the true difference is d, is calculated from the following formula,

$$r = 2(k's_1/d)^2(df_2 + 1), \qquad \text{(III-28)}$$

where df_2 is the estimated degrees of freedom in the experiment to be conducted and k' is obtained from either table III-1 ($\gamma = 80$ per cent) or table III-2 ($\gamma = 95$ per cent), corresponding to the values of df_1 and df_2. If r is too small to give the estimated df_2, a smaller value of df_2 is used to find k' from the table. The following illustrative example indicates the procedure. From the data of example II-1 the experimental error variance is estimated to be $s_1^2 = 141.6$ with $40 = df_1$ degrees of freedom. Let us assume that $d = 20$ grams of fat is the difference which it is important to detect if present, that it is desired to detect a true difference of $d \geq 20$ in $\gamma = 80$ per cent of the experiments, and that six treatments are to be included in a completely randomized design. Then, from equation (III-28) we find

$$r = \frac{2(141.6)(.330)^2(60 + 1)}{400} \doteq 5,$$

where it is estimated that df_2 is 60 and the value $k' = .330$ is read from table III-1 for $df_1 = 40$ (actually for 32) and $df_2 = 60$. Since the number of degrees of freedom df_2 is overestimated, we try $df_2 = 24$, and

$$r = \frac{2(141.6)(.515)^2(24 + 1)}{400} \doteq 5 \text{ replicates.}$$

If a smaller true difference is to be detected in $\gamma = 80$ per cent of the cases or if $\gamma = 95$ per cent more replicates are required.

Occasionally the experimenter has no definite estimate of the variance in the form s_1^2, but he knows something about its order of magnitude. In order to relate the information concerning the upper and lower limits of a standard deviation into quantities like s_1 and df_1, Harris *et al.* [147] propose a rather

TABLE III-1. Values of k'; α = .05 for one-tailed tests, .10 for two-tailed tests*

df_2 \ df_1	1	2	3	4	5	6	8	12	16	24	32	∞
						γ = .80						
1	13.8	8.52	7.39	6.93	6.68	6.51	6.31	6.13	6.04	5.96	5.92	5.79
2	5.88	3.51	3.02	2.81	2.70	2.62	2.53	2.45	2.41	2.37	2.35	2.30
3	4.30	2.55	2.20	2.03	1.96	1.91	1.85	1.78	1.75	1.72	1.70	1.65
4	3.55	2.10	1.80	1.67	1.60	1.56	1.50	1.45	1.43	1.40	1.39	1.36
5	3.12	1.85	1.58	1.47	1.41	1.37	1.32	1.28	1.25	1.23	1.22	1.18
6	2.81	1.66	1.43	1.32	1.27	1.23	1.19	1.15	1.13	1.11	1.10	1.07
7	2.56	1.52	1.30	1.21	1.16	1.12	1.08	1.05	1.03	1.02	1.01	.979
8	2.37	1.41	1.21	1.12	1.07	1.04	1.00	.972	.956	.940	.932	.910
9	2.23	1.32	1.14	1.05	1.01	.978	.944	.913	.898	.883	.875	.854
10	2.11	1.25	1.07	.993	.952	.925	.893	.863	.849	.835	.828	.805
12	1.92	1.14	.975	.902	.865	.840	.811	.784	.771	.758	.752	.732
14	1.77	1.05	.899	.831	.797	.775	.748	.723	.710	.699	.693	.676
16	1.65	.976	.838	.775	.743	.722	.697	.673	.662	.651	.646	.631
18	1.56	.921	.790	.731	.701	.681	.658	.635	.624	.614	.609	.594
20	1.48	.873	.750	.693	.665	.646	.624	.602	.592	.583	.578	.563
25	1.32	.779	.669	.619	.593	.577	.557	.538	.529	.520	.515	.502
30	1.20	.708	.608	.563	.540	.525	.507	.489	.481	.473	.469	.456
40	1.04	.613	.526	.486	.467	.454	.438	.423	.416	.409	.405	.395
50	.925	.548	.471	.435	.417	.405	.391	.378	.371	.365	.362	.353
60	.844	.499	.429	.396	.380	.369	.356	.344	.338	.333	.330	.322
80	.730	.432	.371	.342	.328	.319	.308	.298	.292	.288	.285	.278
100	.652	.385	.331	.306	.293	.285	.275	.266	.261	.257	.255	.249

*This table was reproduced with the permission of the editor of the *Journal of the American Statistical Association* from the paper by Harris, M., Horvitz, D. G., and Mood, A. M., "On the determination of sample sizes in designing experiments," *J. Am. Stat. Assoc.* 43:391-402, 1948.

TABLE III-2. Values of k′; α = .05 for one-tailed tests, .10 for two-tailed tests*

df_2 \ df_1	1	2	3	4	5	6	8	12	16	24	32	∞
						Y = .95						
1	57.1	19.5	14.4	12.6	11.6	11.0	10.4	9.85	9.58	9.33	9.21	8.86
2	24.2	7.74	5.60	4.77	4.39	4.15	3.86	3.61	3.49	3.38	3.33	3.19
3	17.6	5.58	4.03	3.39	3.13	2.94	2.74	2.55	2.46	2.39	2.35	2.23
4	14.5	4.58	3.28	2.79	2.56	2.40	2.23	2.08	2.01	1.94	1.91	1.82
5	12.6	3.97	2.88	2.41	2.23	2.09	1.93	1.82	1.76	1.69	1.66	1.58
6	11.2	3.55	2.57	2.17	2.00	1.88	1.73	1.62	1.57	1.52	1.49	1.42
7	10.3	3.26	2.36	1.99	1.83	1.72	1.58	1.48	1.43	1.38	1.36	1.30
8	9.70	3.05	2.19	1.86	1.70	1.60	1.48	1.39	1.34	1.29	1.27	1.21
9	9.12	2.87	2.06	1.75	1.60	1.50	1.39	1.30	1.26	1.21	1.19	1.13
10	8.62	2.72	1.95	1.65	1.51	1.42	1.32	1.23	1.19	1.15	1.13	1.07
12	7.83	2.47	1.77	1.50	1.37	1.29	1.20	1.12	1.08	1.04	1.02	.971
14	7.22	2.28	1.63	1.38	1.26	1.19	1.11	1.03	.993	.959	.942	.893
16	6.73	2.13	1.52	1.29	1.18	1.11	1.03	.959	.924	.893	.878	.834
18	6.35	2.01	1.44	1.22	1.11	1.04	.972	.904	.872	.842	.828	.785
20	6.02	1.90	1.36	1.15	1.05	.991	.921	.858	.827	.798	.785	.744
25	5.37	1.70	1.22	1.03	.940	.884	.822	.765	.738	.712	.700	.663
30	4.89	1.54	1.11	.935	.855	.804	.748	.695	.671	.647	.636	.605
40	4.23	1.33	.962	.809	.739	.696	.646	.601	.580	.560	.550	.525
50	3.78	1.19	.854	.722	.661	.622	.577	.537	.518	.500	.492	.469
60	3.45	1.09	.778	.658	.602	.567	.525	.490	.472	.456	.448	.428
80	2.98	.940	.672	.569	.520	.490	.454	.423	.408	.395	.388	.369
100	2.67	.840	.600	.508	.465	.438	.405	.378	.365	.353	.347	.329

*This table was reproduced with the permission of the editor of the *Journal of the American Statistical Association* from the paper by Harris, M., Horvitz, D. G., and Mood, A. M., "On the determination of sample sizes in designing experiments," *J. Am. Stat. Assoc.* 43:391-402, 1948.

6

simple procedure. First the experimenter is asked for upper and lower limits, s_U and s_L, on the standard deviation. Suppose he estimates 7 per cent and 12 per cent of the mean to be the lower and the upper limits of the standard deviation for a mean of 30. Then, $s_L = .07(30) = 2.1$ and $s_U = .12(30) = 3.6$. The estimate of the standard deviation is taken as the average of the two estimates, or $s_1 = (s_U + s_L)/2 = (2.1 + 3.6)/2 = 2.85$. In order to find df_1, it is necessary to have the experimenter place some confidence on his estimates. Suppose that his confidence is such that he feels it would be a fair 4 to 1 bet (i.e., he would bet either way) that the standard deviation from the experiment will fall within the range 2.1 to 3.6 four out of five times (80 per cent of the time). In order to determine df_1, we compute the values of $\sqrt{\chi^2_{.10}/\chi^2_{.90}}$ for the various degrees of freedom. The value agreeing closest to the ratio $s_U/s_L = 3.6/2.1 = 1.714$ will indicate the degrees of freedom associated with $s_1 = 2.85$. For the present example the value of $\sqrt{\chi^2_{.10}(12df)/\chi^2_{.90}(12df)} = \sqrt{18.549/6.304} = 1.72$ is closest to 1.714. Therefore, we can assume that s_1 is associated with $df_1 = 12$ degrees of freedom (see table III-3). With these estimates, we are now in a position to determine the desired number of replicates.

III-4.3.2 Tang's method. Tang [285] developed a procedure and prepared the necessary tables for determining the number of replicates to be used in an experiment. As with the previous method, it is assumed that the error components of variation are normally distributed with zero mean and a common variance σ^2. A reasonably good estimate of σ^2 is required and the true treatment effect $(\mu_i - \mu)$ must be specified. In addition, it is necessary to state the sizes of the type I error and of the type II error before estimating the required sample size from the following formula,

$$\phi_{\beta,\alpha} = \sqrt{\frac{2\lambda}{v}}, \tag{III-29}$$

where

$$\lambda = \frac{r}{2\sigma^2} \sum_{i=1}^{v} (\mu_i - \mu)^2, \tag{III-30}$$

r = number of replicates, v = number of treatments, $\mu_i - \mu$ = treatment effect, α = size of the type I error, and β = size of the type II error.

The tables prepared by Tang [285] are not included in the present text but are available in various places [e.g., 175, 285]. Instead, the values of ϕ for $1 - \beta = .8$ and $\alpha = .05$ and .01 as computed by Lehmer [192] are presented in table III-4. The part of the table dealing with degrees of freedom less than 10 for the error mean square was omitted. Lehmer [192] has prepared a second table in which the size of a type II error is $1 - \beta = 70$ per cent. If tables of ϕ are desired for smaller type II errors, the reader is referred to Tang's original tables [175, 285], which give the value of β for specified values of $df_1 = f_1$, $df_2 = f_2$, α, and ϕ.

TABLE III-3. Square roots of ratios of chi-square for various degrees of freedom to use in determining degrees of freedom for estimated standard deviation[a]

Odds =	1:1	3:2	4:1	9:1
Ratio of square root of chi-squares	$\sqrt{\chi^2_{.25}/\chi^2_{.75}}$ [b]	$\sqrt{\chi^2_{.20}/\chi^2_{.80}}$	$\sqrt{\chi^2_{.10}/\chi^2_{.90}}$	$\sqrt{\chi^2_{.05}/\chi^2_{.95}}$
Degrees of freedom				
1	3.6	5.1	13.0	31.3
2	2.15	2.7	4.7	7.6
3	1.85	2.15	3.3	4.7
4	1.66	1.91	2.7	3.7
5	1.57	1.76	2.4	3.1
6	1.50	1.67	2.2	2.8
7	1.45	1.60	2.1	2.6
8	1.43	1.55	1.96	2.4
9	1.40	1.51	1.88	2.3
10	1.38	1.47	1.81	2.2
12	1.33	1.42	1.72	2.01
14	1.31	1.36	1.64	1.90
16	1.28	1.35	1.59	1.82
18	1.27	1.33	1.55	1.75
20	1.26	1.31	1.51	1.70
22	1.25	1.29	1.48	1.66
24	1.24	1.28	1.45	1.62
26	1.22	1.27	1.43	1.59
28	1.21	1.26	1.41	1.56
30	1.20	1.25	1.40	1.54
40	-	-	1.34	1.45
60	-	-	1.27	1.38
120	-	-	1.18	1.24

[a]An extension of the table (table III) by Harris, Horvitz and Mood [147].
[b]Values estimated from Figure II-4.

To illustrate the use of table III-4, suppose that $\sigma^2 = 14$, the design is a randomized complete block experiment, $v = 5$, $\alpha = .05$, and the $\mu_i - \mu$ are -5, -4, 0, 3, and 6. From equation (III-30),

$$\lambda = \frac{r(86)}{2(14)} = r(3.0714),$$

which for $r = 4$ is 12.2856. From formula (III-29),

$$\phi = \sqrt{\frac{2(12.2856)}{5}} = 2.217,$$

which is larger than the value of $\phi(1.88)$ in table III-4 for $df_1 = 4$ and $df_2 = 12$. If we set $r = 3$, the value for ϕ is 1.920, which is less than the tabulated value of $\phi = 2.066$ [from 192] for $df_1 = 4$ and $df_2 = 8$. Hence, we would use four replicates and would detect significance at the five per cent level for the effects specified in about 80 per cent of all experiments.

As a second illustrative example, suppose that $\sigma^2 = 141.6$, $\sum(\mu_i - \mu)^2 = 400$, $1 - \beta = 80$ per cent, $\alpha = 5$ per cent, $v = 6$, and a completely randomized design is to be used. For $r = 5$,

$$\phi = \sqrt{\frac{2(5)(400)}{2(6)(141.6)}} = 1.534.$$

The value of ϕ from the table III-4 for 5 and 24 degrees of freedom is 1.64. Since the calculated value is below the tabular value of ϕ, it is necessary to try a larger value of r. For $r = 6$,

$$\phi = \sqrt{\frac{2(6)(400)}{2(6)(141.6)}} = 1.681,$$

which is larger than the tabulated value of ϕ (1.60) for 5 and 30 degrees of freedom. Hence, six replicates would be required to detect the significance of a treatment sum of squares of 400 in four out of five experiments on the average.

Tang's [285] tables are quite useful for determining the size of the type II error, given that r replicates are to be used and given the other necessary values. If the type II error is relatively large and if the number of replicates cannot be increased, the experimenter may decide not to conduct the experiment with the available resources. If, on the other hand, the experimenter decides to conduct the experiment with the available number of replicates, he could increase the type I error to 10 per cent, say, and thereby reduce the type II error. For small experiments and for $(\mu_1 - \mu_2)/\sigma < 2$, the $\alpha = 10$ per cent level may advisedly be used in preference to any smaller level of significance.

III-4.3.3 Tukey's method. Harris *et al.* [147] give the following equation for determining the sample size, r, necessary to obtain an α per cent

TABLE III-4. Values of ϕ when the probability of detecting the falsehood of the hypothesis tested is $1 - \beta = .8$*

f_2 \ f_1	1	2	3	4	5	6	7	8	9	10	12	15	20	24	30	40	60	120	∞	$240/f_2$
	Level of significance, α = .05																			
10	2.20	2.10	2.01	1.95	1.90	1.86	1.84	1.81	1.79	1.77	1.74	1.71	1.68	1.66	1.64	1.62	1.60	1.58	1.55	
12	2.16	2.04	1.95	1.88	1.82	1.78	1.75	1.72	1.70	1.68	1.64	1.61	1.57	1.55	1.53	1.50	1.48	1.45	1.42	
14	2.13	2.00	1.90	1.83	1.77	1.72	1.69	1.66	1.63	1.61	1.57	1.54	1.49	1.47	1.44	1.42	1.39	1.36	1.33	
16	2.11	1.97	1.87	1.79	1.73	1.68	1.64	1.61	1.58	1.56	1.52	1.48	1.43	1.41	1.38	1.35	1.32	1.29	1.25	
18	2.10	1.95	1.84	1.76	1.70	1.65	1.61	1.57	1.54	1.52	1.48	1.44	1.39	1.36	1.33	1.30	1.27	1.23	1.19	
20	2.08	1.94	1.82	1.74	1.67	1.62	1.58	1.54	1.52	1.49	1.45	1.40	1.35	1.32	1.29	1.26	1.22	1.19	1.14	12
24	2.06	1.91	1.79	1.70	1.64	1.58	1.54	1.50	1.47	1.44	1.40	1.35	1.30	1.27	1.23	1.20	1.16	1.11	1.06	10
30	2.05	1.89	1.76	1.67	1.60	1.54	1.50	1.46	1.43	1.40	1.35	1.30	1.24	1.21	1.17	1.13	1.09	1.04	.98	8
40	2.03	1.86	1.73	1.64	1.57	1.51	1.46	1.42	1.38	1.35	1.30	1.25	1.18	1.15	1.11	1.06	1.01	.95	.88	6
60	2.01	1.84	1.71	1.61	1.53	1.47	1.42	1.38	1.34	1.31	1.25	1.19	1.12	1.08	1.04	.99	.93	.86	.77	4
80	2.01	1.83	1.69	1.59	1.51	1.45	1.40	1.35	1.32	1.28	1.23	1.17	1.09	1.05	1.00	.95	.88	.81	.70	3
120	2.00	1.81	1.68	1.58	1.50	1.43	1.38	1.33	1.30	1.26	1.21	1.14	1.06	1.02	.97	.91	.84	.75	.62	2
240	1.99	1.80	1.67	1.56	1.48	1.41	1.36	1.31	1.27	1.24	1.18	1.11	1.03	.98	.93	.87	.79	.68	.51	1
∞	1.98	1.79	1.65	1.54	1.46	1.40	1.34	1.29	1.25	1.22	1.16	1.08	1.00	.95	.89	.82	.73	.60	0	0
	Level of significance, α = .01																			
10	2.91	2.76	2.65	2.57	2.50	2.46	2.42	2.38	2.36	2.34	2.30	2.25	2.22	2.20	2.17	2.15	2.12	2.09	2.06	
12	2.81	2.64	2.52	2.42	2.35	2.30	2.26	2.22	2.19	2.17	2.12	2.08	2.03	2.01	1.98	1.95	1.92	1.89	1.85	
14	2.75	2.56	2.42	2.33	2.25	2.19	2.15	2.11	2.08	2.05	2.00	1.96	1.90	1.88	1.84	1.81	1.78	1.74	1.70	
16	2.70	2.50	2.36	2.26	2.18	2.12	2.07	2.03	1.99	1.96	1.92	1.87	1.81	1.78	1.74	1.71	1.67	1.63	1.59	
18	2.67	2.46	2.31	2.20	2.12	2.06	2.01	1.97	1.93	1.90	1.85	1.80	1.74	1.70	1.67	1.63	1.59	1.55	1.50	
20	2.64	2.43	2.27	2.16	2.08	2.01	1.96	1.92	1.88	1.85	1.80	1.74	1.68	1.64	1.61	1.57	1.52	1.48	1.42	12
24	2.60	2.38	2.22	2.10	2.02	1.95	1.89	1.85	1.81	1.77	1.72	1.66	1.59	1.56	1.52	1.47	1.42	1.37	1.31	10
30	2.56	2.33	2.16	2.05	1.96	1.88	1.83	1.78	1.74	1.70	1.64	1.58	1.51	1.47	1.42	1.37	1.32	1.26	1.19	8
40	2.52	2.28	2.11	1.99	1.89	1.82	1.76	1.71	1.67	1.63	1.57	1.50	1.42	1.38	1.33	1.27	1.21	1.14	1.06	6
60	2.48	2.24	2.06	1.94	1.84	1.76	1.70	1.64	1.60	1.56	1.49	1.42	1.34	1.29	1.23	1.17	1.10	1.02	.92	4
80	2.47	2.21	2.04	1.91	1.81	1.73	1.66	1.61	1.56	1.52	1.46	1.38	1.29	1.24	1.18	1.12	1.04	.95	.83	3
120	2.45	2.19	2.01	1.88	1.78	1.70	1.63	1.58	1.53	1.49	1.42	1.34	1.25	1.20	1.13	1.06	.98	.88	.73	2
240	2.43	2.17	1.99	1.86	1.75	1.67	1.60	1.55	1.50	1.46	1.38	1.30	1.20	1.15	1.08	1.00	.91	.79	.59	1
∞	2.42	2.15	1.97	1.83	1.73	1.64	1.57	1.51	1.46	1.42	1.35	1.26	1.16	1.10	1.03	.94	.84	.69	0	0

*This table was reproduced with the permission of the editor of the *Annals of Mathematical Statistics* from the paper by Lehmer, E., "Inverse tables of probabilities of errors of the second kind," *Ann. Math. Stat.* 15:388-398, 1944.

confidence interval of length less than or equal to $2d$ with an assurance of γ per cent:

$$r = 2(s_1 t_{\alpha.df_2}/d)^2 F_{1-\gamma}(df_2, df_1). \tag{III-31}$$

The values of $F_{1-\gamma}(df_2, df_1)$ are obtained from table II-8, and the other symbols are those defined in section III-4.3.1. The value for $t_{\alpha.df_2}$ is obtained from the ordinary t table. Since the value $t_{\alpha.df_2}$ is for two means, some adjustment is necessary for comparing the ranges within a set of v means. Tukey [296, Ch. 18] gives the following equation for determining the sample size, r, necessary to obtain an α per cent confidence interval of length $2d$ or less on the differences between any two means from a set of v means with an assurance of γ per cent:

$$r = s_1^2 q_{\alpha.df_2}^2 F_{1-\gamma}(df_2, df_1)/d^2, \tag{III-32}$$

where s_1^2, $F_{1-\gamma}(df_2, df_1)$, and d are defined above and where $q_{\alpha.df_2}$ is the tabular value of q_α for v treatments and df_2 degrees of freedom in table II-1.

To illustrate the procedure, suppose that Tukey's *hsd* procedure (section II-1.1.4) is to be used to compare differences among $v = 6$ means. Suppose that $s_1^2 = 141.6$ with $df_1 = 40$ degrees of freedom and that $d = 20$. The values of df_2 and $q_{\alpha.df_2}$ depend upon the design and the value of r. Suppose that the completely randomized design is to be used, that $r = 6$, and that $\gamma = 90$. Then, $df_2 = 30$, and from table II-8, $F_{10}(30, 40df)$ is 1.54. From table II-1, q_{05} for $v = 6$ and $df_2 = 30$ is equal to 4.30. From equation (III-32),

$$r = (141.6)(4.30)^2(1.54)/400 \doteq 10.$$

Since the value for df_2 was underestimated, try $r = 9$; then, $df_2 = 48$, $q_\alpha = 4.2$, $F_{10}(48, 40df) = 1.48$, and $r = 141.6(4.2)^2(1.48)/400 = 9.2$. Since the value of r is greater than 9, we next try $r = 10$ and find that $r = 10$ is sufficiently large for our purposes. With ten replicates, we would have the assurance that the 95 per cent confidence interval will be less than or equal to $2d$ in length in 90 per cent of all experiments.

III-5 Sampling the Experimental Unit

In certain field experiments, it is not feasible to harvest the entire plot, and it is impractical to have smaller plots. Also, for some experiments the amount of information required on certain characters may be less than for others; if so, sampling is indicated for the characters of less interest. The results presented herein are applicable to a wide class of experiments, although the emphasis is placed on field experiments; this results from the fact that sampling theory has been developed to the greatest extent for field experiments [44, 49, 61, 159, 334, 335] and for sample surveys [88, 166, 204, 330].[1]

[1]The literature on sampling is voluminous. The references listed here represent a selected list.

Whenever a plot or pen is sampled, a loss in information relative to the information obtained for complete recording results. An experiment may be designed with the optimum size of an experimental unit and with the required number of replicates and then be rendered inadequate because of sampling procedures. Before selecting a sampling procedure, the experimenter should determine if the suggested procedure results in the required degree of precision; if not, the sampling rate should be increased or sampling should be dispensed with.

III-5.1 LOSS IN INFORMATION DUE TO SAMPLING

Yates and Zacopanay [335] present the general theory for sampling experimental units. Since the sampling is within the experimental unit, the number of treatments or blocks need not be considered. The variation among the experimental units, excluding block and treatment variation, is considered in relation to that among sampling units. Consider that we have n experimental units each composed of h sampling units and that a random sample of k sampling units or individuals is selected from each experimental unit. The analysis of variance follows:

Source of variation	df	Mean square	
		Average value	Experimental
Among experimental units	$n-1$	$V_s' + kV_p'$	MS_e
Among sampling units within experimental units	$n(k-1)$	V_s'	MS_s

The variance of a plot mean of k sampling units is given by equation (III-1) as $V_p' + V_s'/k$. An estimate of V_p' is obtained from the difference of the two experimental mean squares divided by k, or $(MS_e - MS_s)/k$.

The efficiency of sampling [335] relative to complete recording is the ratio of the amounts of information from the two schemes,

$$\frac{1/(V_p' + V_s'/k)}{1/(V_p' + V_s'/h)} = \frac{V_p' + V_s'/h}{V_p' + V_s'/k}. \tag{III-33}$$

The fractional loss in information is one minus equation (III-33). In experimental work the loss in information, L, due to sampling may be estimated from the equation,

$$L = 1 - \frac{\frac{1}{k}(MS_e - MS_s) + \frac{1}{h}MS_s}{\frac{1}{k}(MS_e - MS_s) + \frac{1}{k}MS_s} = \left(1 - \frac{k}{h}\right)\frac{MS_s}{MS_e}, \tag{III-34}$$

where the factor $(h - k)/h$ is the ordinary finite population correction term [88, 330]. If the L values from several experiments are to be combined into an

average, a correction should be applied, since the L values are biased. The quantity $L(n - 3)/(n - 1) = L'$ is unbiased and may be averaged.[1]

Yates and Zacopanay [335] summarized the results from a number of experiments on cereals. The sampling rates in their experiments averaged about 6 per cent of the plot. The loss in information due to sampling relative to complete harvesting was 31.2 per cent, or about one-third. This means that 50 per cent more replication would be required for similar experiments in which 6 per cent sampling was practiced relative to experiments where complete harvesting of the experimental unit was performed. They show that 12 per cent sampling would have resulted in a loss in information of 18 per cent and that 18 per cent sampling would have resulted in a loss in information of 13 per cent.

In each type of experimental work, it is necessary to obtain factual evidence concerning the relative loss in information due to sampling. Armed with such evidence the experimenter may then decide, in light of cost and practical considerations, whether or not to sample the experimental units.

III-5.2 DETERMINATION OF THE OPTIMAL SAMPLING RATE

Yates and Zacopanay [335] present data bearing upon the time spent in sampling and in complete harvesting of field trials, separating time for general care of the experiment from harvest costs. With these data, either in hours or dollars, it is possible to estimate the optimal rate of sampling. The result may be that it is less costly to obtain a specified degree of precision by having more replicates and sampling than to have fewer replicates and use complete harvesting. To determine the optimal sampling rate, it is desired to minimize the total work (or cost) of the experiment, given that the quantity of information is a constant; i.e., the quantity of information is set equal to one:

$$Q(1 - L) = 1 = \frac{1 - f}{Q} + \frac{f}{S}, \tag{III-35}$$

where Q = necessary relative size of experiment (Q = one in the case of complete harvesting; $Q = r/r_0$, where r_0 is the number of replicates in the standard experiment), $S/Q = k/h = x$ = sampling rate, L = loss in information as defined by equation (III-34), and $f = V_s'/(V_s' + hV_p')$. The total amount of relative work is

$$\frac{r}{r_0}(C_p + kC_s) = aQ + bS, \tag{III-36}$$

where C_p and C_s are defined in equation (III-5), $a = C_p$, $b = hC_s$, and the other symbols are defined above.

We wish to find values of Q and S which will minimize the total amount

[1]Note that the correction term is the reciprocal of the first moment of the F distribution. Likewise, the variance of L is proportional to the variance of F, $EF^2 - (EF)^2$.

of work for a fixed quantity of information. Therefore, we wish to minimize the quantity,

$$\phi = \frac{r}{r_0}(C_p + kC_s) - \lambda\left[\frac{r_0(V_p' + V_s'/k)}{r(V_p' + V_s'/h)} - 1\right],$$

which is equal to

$$\phi = aQ + bS - \lambda\left[\frac{1-f}{Q} + \frac{f}{S} - 1\right]. \tag{III-37}$$

If we differentiate (III-37) with respect to Q, S, and λ (the Lagrangian multiplier), set the resulting equations equal to zero, and solve for Q and S, we find

$$Q = 1 - f + f\sqrt{\frac{b(1-f)}{af}}, \tag{III-38}$$

$$S = Q\Big/\sqrt{\frac{b(1-f)}{af}}, \tag{III-39}$$

and

$$\frac{S}{Q} = x = \frac{k}{h} = \sqrt{\frac{af}{b(1-f)}}. \tag{III-40}$$

The values for S and Q as obtained from equations (III-38) and (III-39) for $f = .03116$, $a = 1$, and $b = 2$ (these are values obtained by Yates and Zacopanay) are

$$Q_0 = 1 - .03116 + .03116\sqrt{\frac{2(1 - .03116)}{.03116}} = 1.215.$$

$$S_0 = 1.215/7.886 = .154.$$

Thus, $x_0 = .154/1.215 = 12.7$ per cent or approximately 13 per cent, which is the optimum sampling percentage. The total relative work required is $aQ_0 + bS_0 = C_0 = 1.215 + 2(.154) = 1.52$.

A procedure using any other sampling rate may be compared with the optimum sampling rate in a very simple manner. Suppose that we wish to compare $S_1/Q_1 = 6$ per cent sampling with the optimum. Then for $Q_1 = 1.49$,[1] $S_1 = .0894$, and the total work is (for $a = 1$ and $b = 2$) $Q_1 + 2S_1 = C_1 = 1.49 + 2(.0894) = 1.67$. The additional work necessary to obtain the same information with a 6 per cent sampling rate as with the optimum sampling rate is $(1.67 - 1.52)/1.52 = 10$ per cent.

Yates and Zacopanay [335] present a simple graphical method for obtaining the values Q_0, S_0, C_0, Q_1, S_1, and C_1. The first step is to plot the function given in equation (III-35) with Q as the abscissa and S as the ordinate (see figure III-2). The second step is to consider the total work in an equation of the form,

$$Q + \frac{b}{a}S = C. \tag{III-41}$$

[1]$Q_1 = 1/(1 - .328) = 1.49$; the correct loss in information is 31.2 per cent but the figure 32.8 per cent was used to obtain agreement with the results by Yates and Zacopanay.

For $C = 0$, plot the line $S + aQ/b = 0$. The line parallel to the line $S + aQ/b = 0$ and tangent to the curve given by equation (III-35) is the line of minimum work. The broken line in figure III-2 which crosses the Q axis at 1.52 is the line tangent to the curve and parallel to the line $S + aQ/b = 0$. The values $Q_0 = 1.215$ and $S_0 = .154$ are read from the graph as the coordinates of the point at which the broken line is tangent to the curve.

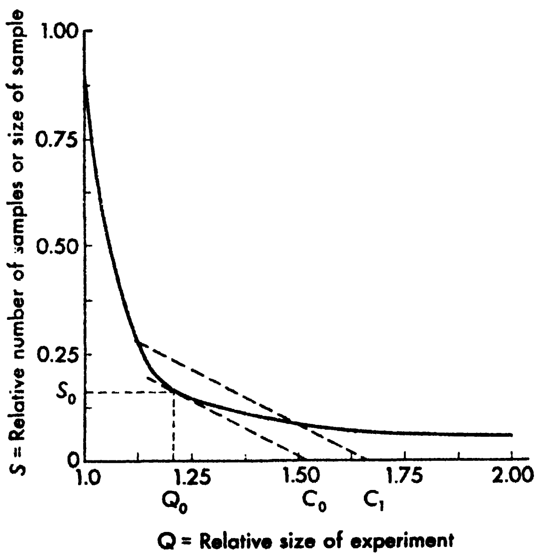


Figure III-2. Relation between number of samples and size of experiment required for a fixed amount of information.

In order to compare any other sampling scheme with the optimum procedure, select a value of Q (e.g., $Q_1 = 1.49$), draw a line which is parallel to the tangent of the curve at Q_0S_0 and which cuts the curve at the point $Q = Q_1$, and read the value of S_1 (for $Q_1 = 1.49$, $S_1 = .0894$) from the graph. Since the total work is $C_1 = bS_1/a + Q_1$, the point at which the straight line parallel to $S + aQ/b = 0$ cuts the Q axis gives the total work. Thus, the total relative work for two sampling procedures is read directly from the Q axis as C_0 and C_1.

The above argument gives the optimum sampling rate, x, subject to harvesting, threshing, etc. being done by methods appropriate for sampling procedures. If the plot is completely harvested, cheaper large-scale methods may be available. The decision rests on the comparison of the cost for harvesting, threshing, recording, etc. of the entire plot by large-scale methods in an experiment of r_0 replicates with the cost for sampling the optimum fraction, x or k, in a larger experiment of r replicates where r is large enough to make the variance of a treatment equal in the two cases. Other factors affecting the comparison and the use of sampling procedures relative to complete harvesting are discussed by Yates and Zacopanay [335].

The above discussion refers to the random selection of k sampling units from the h units in the whole plot. Other sampling procedures such as the stratified random and the systematic (a form of stratification) are usually more efficient than samples selected completely at random from the entire plot. The loss in information with such procedures will be less than the loss due to random sampling from the entire plot [335]. Homeyer and Black [159] present some systematic sampling procedures which tend to decrease the loss in information due to sampling a given fraction of the plot relative to the loss in information suffered by random sampling. Also, some data on this are presented by Yates and Zacopanay [335], who give some results by R. J. Kalamkar on the investigation of the variation among different sampling units in a field of wheat.

III-5.3 SAMPLING FOR ONE CHARACTER WHEN A RELATED CHARACTER OF THE EXPERIMENTAL UNIT IS KNOWN

In field experiments on cereals the total combined weight of grain and straw of the plot may be obtained without difficulty. A subsample of the total plot produce is taken and threshed; the weight of the grain = Y is observed as well as the total subsample weight of both straw and grain = X. Since Y is more costly to observe than X, i.e., threshing is involved, it may be advisable to make use of the total produce weight of the plot as well as X and Y in estimating grain per plot. The methods for performing this operation are described under the topic known as double sampling [88, 330]. Yates and Zacopanay [335] show that appreciable gains may be made by using total plot produce, whereas Cochran [49] observed somewhat smaller gains. Double sampling procedures are well known in certain types of research but could be used to advantage in a number of others.

The practice of making "eye estimates" to determine the total yield for an experimental unit has been in use for some time. Beef cattle have been appraised for quality by buyers. Legume content of pasture mixtures is often estimated by eye. Field plots are graded for population size of insects or for intensity of disease infection. That these "eye estimates" are useful will not be questioned, *but* they should be used only in conjunction with actual measurements or counts until their validity has been established. Otherwise, serious biases may result [61, 330, Ch. 6; 334].

CHAPTER IV

The Completely Randomized Design

The preceding chapters deal with subjects related to the design of experiments. The remainder of the text is devoted to topics related specifically to a particular design or to experimental designs in general. The concepts related to factorial experiments (see Chapters VII, VIII, IX) are extremely useful for the development of the analysis for several of the incomplete block designs. Although the factorial experiment refers to an arrangement of treatments and not to a design, the subject of factorial experiments is usually discussed coincidentally with the various experimental designs. Any one of the designs listed in Chapter I may be used for a factorial experiment.

The completely randomized design is the basic design. All other randomized designs stem from it by placing restrictions upon the allocation of the treatments within the experimental area. If the lot of experimental material or the experimental area or space is not homogeneous, one of the methods for controlling the variability is to stratify the material into homogeneous subgroups. It is important to grasp the relationships among experimental designs and between experimental designs and factorial experiments if one is to understand the analyses for complex designs as described in this text. A somewhat analogous situation in statistical methodology is illustrated by the relationship between regression and the analysis of variance; both subjects may be treated either as regression or as the analysis of variance. Also, understanding of the relationship between the various statistical procedures and experimental designs is basic to understanding and applying either subject.

The first part of this chapter is devoted to a discussion of the advantages and disadvantages, the experimental layout, and the analysis of the completely randomized design. This pattern is followed for other experimental designs in the following chapters. Computational procedures are illustrated with examples. The second part of most chapters contains a discussion of the expectations of mean squares and the estimation of effects using the linear model applicable to the design and the least squares method of estimation.

IV-1 Applications of the Completely Randomized Design

IV-1.1 INTRODUCTION

The simplest of all designs having a random arrangement is the completely randomized design. The design may be defined as one in which the treatments

are randomly arranged over the whole of the experimental material. No effort is made to confine treatments to any portion of the entire area, material, or space. The number of repetitions for any one treatment may vary. The completely randomized design is usually selected when the variation over all of the experimental material is relatively small.

The completely randomized design is considered to be most useful in laboratory technique and methodological studies, in some greenhouse studies, in experiments on certain animals, etc. As an example, the treatments may represent a number of different injections into chickens of a certain age. All treated chickens might be placed in the same pen. Likewise, certain treatments on sheep might be applied and all the sheep kept together in one flock. The treated animals would be marked in an easily distinguishable manner. This particular setup or experimental procedure would allow for relative equality of environmental effects upon all material within the experimental area or space.

IV-1.2 ADVANTAGES AND DISADVANTAGES

The chief advantages of the completely randomized design are

(i) It is easy to lay out the design.
(ii) The design allows for the maximum number of degrees of freedom in the error sum of squares.
(iii) A completely randomized design has the simplest analysis of all experimental designs subject to statistical analysis.
(iv) Unequal number of repetitions for the various treatments may be included without unduly complicating the analysis in most cases.

The chief disadvantage of the design is that it is usually suited only for small numbers of treatments and for homogeneous experimental material. When large numbers of treatments are included, a relatively large amount of experimental material must be used. This generally increases the variation among treatment responses. If the variation over the whole of the experimental material is relatively large, it is possible to select more efficient designs than the completely randomized one, resulting in more precise estimates of the treatment means for the same number of replicates. Completely randomized designs are seldom used for field experiments because experience has shown other designs to be more suitable.

IV-1.3 LAYOUT OF THE DESIGN

The term *layout* refers to the placement of experimental treatments on the experimental site whether it be over space, time, or type of material. The whole of the experimental area or material is partitioned into a number of experimental units, say N. A random selection of r_1 experimental units is made and one of the v treatments is applied to these units. A random selection of r_2 of the remaining $N - r_1$ experimental units is made and one, any one, of the remaining $v - 1$ treatments is applied to these particular units. This proc-

ess continues until all treatments have been applied. When each treatment is replicated an equal number of times, $r_1 = r_2 = \cdots = r_v = r$ and $\sum r_i = rv = N$ experimental units. Unless practical limitations dictate otherwise or unless some treatments are more variable than others or are of greater interest, equal replication on each treatment should be made.

An example of the layout of a completely randomized design for the five treatments A, B, C, D, and E replicated four times each is given below:

(E) (1)	(E) (8)	(C) (9)	(B) (16)	(E) (17)
(A) (2)	(D) (7)	(D) (10)	(B) (15)	(A) (18)
(B) (3)	(C) (6)	(A) (11)	(C) (14)	(B) (19)
(E) (4)	(D) (5)	(A) (12)	(D) (13)	(C) (20)

Such an experiment might be designed for twenty pots on a greenhouse bench, a series of twenty soil analyses, the twenty animals in a feeding trial, twenty cakepans in an oven, twenty successive bakings of single cakes in an oven, records on five litters of four pigs each, or other experimental material.

IV-1.4 ANALYSIS FOR EQUAL REPLICATION OF THE TREATMENTS

The statistical analysis of the results from an experiment laid out in a completely randomized design is presented in Chapter 10 of Snedecor's book [273]. The analysis of variance for this design is the one commonly described as the "between groups" and "within groups" analysis. Given that there are v treatments each repeated r times, the analysis of variance is

Source of variation	df	ss
Among treatments	$(v - 1)$	$\sum_i X_{i.}^2/r - X_{..}^2/rv$
Among experimental units within each treatment	$v(r - 1)$	$\sum_i \left\{ \sum_j X_{ij}^2 - X_{i.}^2/r \right\}$
Total (corrected for the mean)	$(rv - 1)$	$\sum_i \sum_j X_{ij}^2 - X_{..}^2/rv$

where $X_{i.}$ represents the ith treatment total, X_{ij} represents the individual measurement, count, or grade of the jth experimental unit from the ith treatment, and $X_{..}$ represents the total of the rv experimental units. The mean squares are obtained by dividing the sums of squares by their respective degrees of freedom. From these results, various other statistics may be computed,

e.g., Snedecor's F value, the standard error of a treatment mean, the coefficient of variation, the confidence intervals, etc.

Example IV-1. A set of data involving four "tropical feedstuffs" fed to chicks [275] was selected to illustrate the computational procedure and the calculation of various statistics for the completely randomized design. Although the details of the layout of the experiment are not given, the completely randomized design is assumed and is necessary to evaluate treatment differences in the manner presented. For this design, suppose that we have twenty chicks which are *from* a single uniform lot. We draw a random sample of five chicks to receive feeding treatment A. A second random sample is drawn for treatment B, and so on for treatments C and D. The twenty chicks are treated alike in all respects except for the feeding treatments. The differences between lots of five chicks represent only feeding treatment differences plus random sampling differences. If the four lots of five chicks each are from different breeds of chickens or are grown under different environmental conditions, the differences between the lots will contain not only treatment and random sampling components of variation but also a breed or environmental component of variance as well. If, on the other hand, the lot differences represent only feeding treatment differences plus a component due to random sampling from a homogeneous population, then the analysis of variance presented in table IV-1 is appropriate. First compute the totals $X_{1\cdot}$, $X_{2\cdot}$, $X_{3\cdot}$, and $X_{4\cdot}$ for the four treatments A, B, C, and D. The treatment means $\bar{x}_{i\cdot}$ are computed next. The individual sums of squares by treatments, $\sum_{j=1}^{5} X_{ij}^2$, need not be written down but may be obtained as a single figure in the calculating machine unless it is desired to compute the individual variances, s_i^2, or to use them in checking computations. The individual correction terms $X_{i\cdot}^2/5$ also need not be written down but may be accumulated in the calculating machine.

The sum of squares among feeding treatments is obtained by adding the individual correction terms, $(X_{1\cdot}^2 + X_{2\cdot}^2 + X_{3\cdot}^2 + X_{4\cdot}^2)/5 = 9592.2 + \cdots + 101959.2 = 169886.2$, and subtracting the correction term for the experiment, $X_{\cdot\cdot}^2/rv = 1695^2/20 = 143651$, or $169886 - 143651 = 26235$.

The sum of squares representing the variation among chicks within treatments is obtained either by adding the four sums of squares $742.8 + 3850.0 + 2093.2 + 4872.8 = 11558.8$ or by subtracting the treatment sum of squares (uncorrected for the mean) from the total sum of squares (uncorrected for the mean), which is equal to $181445 - 169886.2 = (181455 - 143651) - (169886.2 - 143651) = 37794 - 26235.2 = 11558.8$.

Before pooling the individual within lot sums of squares into a sum of squares and obtaining a generalized error variance, s^2, such as 722.42, we should know something about the homogeneity of the individual variances. Some such test as Bartlett's chi-square test may be used to test for homogeneity [14; section II-3]; thus:

$$\chi^2(v - 1df) = \sum_{i=1}^{v} df_i \log_e s^2 - \sum_{i=1}^{v} df_i \log_e s_i^2, \qquad \text{(IV-1)}$$

which for this example is equal to

$$2.3026\{16 \log_{10} 722.42 - 4(\log_{10} 185.7 + \log_{10} 962.5 + \log_{10} 523.3 + \log_{10} 1218.2)\} = 3.49.$$

TABLE IV-1. Weight gains of baby chicks fed different feeding materials composed of tropical feedstuffs

	Feeding treatment				Total
	A	B	C	D	
	55	61	42	169	
	49	112	97	137	
	42	30	81	169	
	21	89	95	85	
	52	63	92	154	
Totals $= X_{i.}$	219	355	407	714	1695
Means $= \bar{x}_{i.}$	43.8	71.0	81.4	142.8	84.75
$\sum_j X_{ij}^2$	10335	29055	35223	106832	181445
$X_{i.}^2/5$	9592.2	25205.0	33129.8	101959.2	169886.2
$\sum_j X_{ij}^2 - X_{i.}^2/5$	742.8	3850.0	2093.2	4872.8	11558.8
s_i^2	185.7	962.5	523.3	1218.2	—
Range	34	82	55	84	255

Analysis of variance

Source of variation	df	ss	ms
Among feeding treatments	3	26235.2	8745.07
Among chicks within treatments	16	11558.8	722.42
Total	19	37794	
Correction term $= X_{..}^2/20$	1	143651	

Since $\chi^2(3df) \geq 3.49$ has a rather large probability of occurrence (figure II-4, $P \doteq .32$), we need not correct the chi-square value by the following formula:

$$\chi_c^2 = \frac{\chi^2}{1 + \dfrac{1}{3(v-1)}\left\{\sum_{i=1}^{v} \dfrac{1}{df_i} - \dfrac{1}{\sum df_i}\right\}}, \qquad \text{(IV-2)}$$

which for the above example is

$$\frac{3.49}{1 + \frac{1}{9}\{\frac{1}{4} + \frac{1}{4} + \frac{1}{4} + \frac{1}{4} - \frac{1}{16}\}} = \frac{144(3.49)}{159} = 3.16.$$

If the individual variances, s_i^2, are considered to be estimates of the same population variance, σ^2, use is made of the generalized error mean square equal to 722.42 in comparing the treatment means, $\bar{x}_{i.}$.

Snedecor's F test is used to test the hypothesis that the treatment means are estimates of the same parameter, say μ,

$$F(3, 16df) = \frac{8745}{722.42} = 12.1.$$

A value of 12.1 or larger with 3 and 16 degrees of freedom associated with the mean squares in the numerator and in the denominator, respectively, is exceeded less than one per cent of the time ($F_{01}(3, 16df) = 5.29$ from table II-8) in random sampling from homogeneous populations. Therefore, we would reject the hypothesis of no difference among the means. Lacking any information concerning the nature of the treatments the means are considered to be a single set of means and are ranked from highest to lowest, and one of the tests presented in section II-1 should be utilized.

Various statistics may be computed from the analysis of variance table (table IV-1) for the chick data. The standard error of a single observation, or the standard deviation, is the square root of the error mean square, $\sqrt{s^2} = \sqrt{722.42} = 26.9 = s$. The standard error of a treatment mean is $s/\sqrt{r} = s_{\bar{x}} = \sqrt{722.42/5} = 12.0$. The standard error of the difference between two means is $s\sqrt{\frac{1}{r_1} + \frac{1}{r_2}} = \sqrt{\frac{2(722.42)}{5}} = 17.0 = s_{\bar{d}}$. The least significant difference is $t_{05.16df}s_{\bar{d}} = 2.12(17.0) = 36.0$. The coefficient of variation is equal to $s/\bar{x} = 26.9/84.75 = 32$ per cent; to judge whether or not 32 per cent is unusually large requires other knowledge and experience on experimental data of this nature.

IV-1.5 ANALYSIS FOR UNEQUAL REPLICATION OF THE TREATMENTS

Some experimenters are not always so fortunate as to obtain equal numbers for each class. If the experimenter is working with animals, some of the experimental animals may become sick or die, leaving the experimenter with unequal numbers. In the laboratory, an assistant may unwittingly bulk items, may forget to record the data, or may inadvertently lose some results in one way or another, and the experimenter is left with unequal numbers of individuals. Also, it may be necessary to start an experiment with unequal numbers since the experimenter may wish to use all available material and does not have equal amounts for the various treatments. The analysis for unequal numbers in a completely randomized design is little affected; the only real effects are that comparisons among treatments with fewer numbers are less precise than among treatments with larger numbers and the computations are slightly more difficult. The symbolical representation of the analysis of variance for a completely randomized design with unequal replication follows:

Source of variation	df	ss
Among treatments	$v - 1$	$\sum_{i=1}^{v} X_{i.}^2/r_i - X_{..}^2/\sum r_i$
Within treatments	$\sum_{i=1}^{v} r_i - v$	$\sum_{i=1}^{v}\left\{\sum_{j=1}^{r_i} X_{ij}^2 - X_{i.}^2/r_i\right\}$
Total (corrected for mean)	$\sum r_i - 1$	$\sum_{i=1}^{v}\sum_{j=1}^{r_i} X_{ij}^2 - X_{..}^2/\sum r_i$

The symbols are defined in the previous section; the number of replicates, r_i, differs from treatment to treatment.

Example IV-2. A variety, 109, of guayule (a rubber-producing plant) was planted in a field. After the plants were approximately one year old, a sample was selected to observe several characteristics of the variety, such as uniformity of type, dry weight of the plant, rubber yield, etc. From the entire area planted to variety 109, fifty-four plants were selected at random. Of these plants, twenty-seven were normals, fifteen offtypes, and twelve aberrants. The dry weights of shrub for the plants of the three types are given in table IV-2 along with the means and sums of squares.

TABLE IV-2. Dry weight of shrub (without leaves) in grams for the 54 plants from variety 109

	Normals		Offtypes	Aberrants	Total
	58	100	105	34	
	109	117	84	12	
	87	82	88	20	
	101	133	109	5	
	105	172	134	48	
	94	133	106	32	
	167	165	86	21	
	141	[illegible]	149	19	
	112	116	64	24	
	104	120	[illegible]	20	
	58	[illegible]	108	17	
	98	[illegible]	[illegible]	65	
	106	-	[illegible]	-	
	120	-	129	-	
	65	-	22	-	
Totals, $X_{i.}$	3122		1496	317	4935
Means, $\bar{x}_i$	115.6		99.7	26.4	91.4
$\sum_j X_{ij}^2$	390344		162632	11345	564321
$X_{i.}^2/r_i$	360995.7		149201.1	8374.1	518970.9
$s_{\bar{x}_i}$	6.5		8.0	4.7	-

Analysis of variance

Source of variation	df	ss	ms
Among types	2	67566.7	33783.4
N vs O	1	2436.7	-
N + O vs 2A	1	65130.0	-
Within types	51	45750.1	897.1
Within N	26	29348.3	1128.8
" O	14	13430.9	959.4
" A	11	2970.9	270.1
Total	53	113316.8	-
Correction for mean	1	451004.2	-

The analysis of variance for these data is presented at the bottom of table IV-2. The total sum of squares is obtained by squaring all individual weights and subtracting the over-all sum squared divided by the total number,

$$\sum_{i=1}^{v}\sum_{j=1}^{r_i} X_{ij}^2 - X_{..}^2/\sum r_i$$
$$= 58^2 + 109^2 + \cdots + 17^2 + 65^2 - \frac{4935^2}{54}$$
$$= 564321.0 - 451004.2 = 113316.8 \text{ with } 53df.$$

The sum of squares among types with $2df$ is

$$\sum_{i=1}^{v} \frac{X_{i.}^2}{r_i} - \frac{X_{..}^2}{\sum_i r_i} = \frac{3122^2}{27} + \frac{1496^2}{15} + \frac{317^2}{12} - \frac{4935^2}{54}$$
$$= 518570.9 - 451004.2 = 67566.7.$$

The sums of squares among plant weights within normals, offtypes, and aberrants are obtained from the formula,

$$\sum_{j=1}^{r_i} X_{ij}^2 - X_{i.}^2/r_i$$

and are given in table IV-2.

The sums of squares for the two orthogonal comparisons given in the anaylsis of variance table are

$$\frac{3122^2}{27} + \frac{1496^2}{15} - \frac{4618^2}{42} = 2436.7 = \frac{[15(3122) - 27(1496)]^2}{27(15)(27 + 15)}$$

and

$$\frac{4618^2}{42} + \frac{317^2}{12} - \frac{4935^2}{54} = 65130.0 = \frac{[12(4618) - 42(317)]^2}{42(12)(54)}.$$

The standard error of a mean (table IV-2) is computed from the formula,

$$s_{\bar{x}_i} = \sqrt{\frac{\sum_j X_{ij}^2 - X_{i.}^2/r_i}{r_i(r_i - 1)}}.$$

The individual within-type variances were expected to be different. The means might be expected to be linearly related to the variances in material of this type. Bartlett's test for homogeneity of variances results in the following chi-square value (formula IV-1 and IV-2):

$$\chi^2 = 2.3026(51 \log_{10} 897.1 - 26 \log_{10} 1128.8 - 14 \log_{10} 959.4 - 11 \log_{10} 270.1) = 6.29;$$
$$\chi_c^2 = \frac{6.29}{1 + \frac{1}{6}\{\frac{1}{26} + \frac{1}{14} + \frac{1}{11} - \frac{1}{51}\}} = \frac{6.29}{1.030} = 6.11,$$

with $2df$. The probability of obtaining a chi-square value as large as or larger than 6.11 occurs less than 5 per cent of the time in random sampling. Hence, it is concluded that the variances differ. The χ_c^2 value $= 6.11$ with $2df$ may be partitioned into 2 single degree of freedom comparisons with corrected chi-square values of 0.12 and 5.99 for comparisons among plant variances for normals versus offtypes and for the pooled among plant variances for normals and offtypes versus aberrants. Apparently the variation in individual plant weights is much smaller for aberrants than for the other plant

types, but the variation among plant weights for normals and offtypes is approximately equal. One could use the pooled within-plant variance, 1069.5 with 26 + 14 = 40 degrees of freedom, for testing the difference between the means of the normals and offtypes, thus:

$$F = \frac{2436.7}{1069.5} = 2.28.$$

The corresponding F value for 1 and 40 degrees of freedom at the 5 per cent level is 4.08; we conclude that the difference in means of normals and offtypes could be obtained fairly frequently in random sampling. In view of the evidence the experimenter may not wish to assume that the error variances for normals and offtypes are estimates of the same parameter. He may have observed that the weight for the last offtype plant, 22, was unusually low and that for the last aberrant plant, 65, was unusually high. These values tend to increase the variances of both types. A copying error was suspected, but a check showed this not to be the case. A misclassification was suspected but could not be verified. In view of this, then, one may wish to assume that the variances are different. The means may be compared by using a t test with the correct degrees of freedom. An approximate significance level of t may be computed from a formula given by Cochran and Cox [60, p. 92; 273, p. 84]. The computed α per cent t value is obtained from the formula,

$$t_\alpha = \frac{t_{\alpha, r_1 - 1 df} s_{\bar{x}_1}^2 + t_{\alpha, r_2 - 1 df} s_{\bar{x}_2}^2}{s_{\bar{x}_1}^2 + s_{\bar{x}_2}^2}. \tag{IV-3}$$

From formula (IV-3) the 5 per cent significance level for comparing normals and offtypes is

$$t_{05} = \frac{2.056\left\{\frac{1128.8}{27}\right\} + 2.145\left\{\frac{959.4}{15}\right\}}{\frac{1128.8}{27} + \frac{959.4}{15}} = 2.11,$$

which is approximately equivalent to t_{05} with 17 degrees of freedom. The experimental t value is

$$t = \frac{115.6 - 99.7}{\sqrt{\frac{1128.8}{27} + \frac{959.4}{15}}} = \frac{15.9}{10.28} = 1.55,$$

which is somewhat smaller than the calculated 5 per cent value, 2.11. In this instance the means agree sufficiently well so that the same conclusion concerning equality of means is reached regardless of the degrees of freedom associated with t. An illustration of the opposite situation is given by Snedecor [273, sec. 4.6].

Similarly, the mean difference of aberrants and offtypes may be tested by the statistic,

$$t = \frac{99.7 - 26.4}{\sqrt{\frac{959.4}{15} + \frac{270.1}{12}}} = \frac{73.3}{9.30} = 7.88.$$

The 5 per cent level of t for this comparison is

$$t_{05} = \frac{2.145\left\{\dfrac{959.4}{15}\right\} + 2.201\left\{\dfrac{270.1}{12}\right\}}{\dfrac{959.4}{15} + \dfrac{270.1}{12}} = 2.16,$$

and the 1 per cent level of t is

$$t_{01} = \frac{2.977\left\{\dfrac{959.4}{15}\right\} + 3.106\left\{\dfrac{270.1}{12}\right\}}{\dfrac{959.4}{15} + \dfrac{270.1}{12}} = 3.01.$$

The mean weight difference for the two types of plants, offtypes and aberrants, is much larger than could be logically attributed to chance sampling fluctuations.

The coefficient of variation for the whole experiment has little meaning, but for illustrative purposes it is equal to

$$cv = \frac{s}{\bar{x}} = \frac{\sqrt{897.1}}{91.4} = 33 \text{ per cent.}$$

The coefficients of variations for the three types of plants, normals, offtypes, and aberrants, are

$$\frac{\sqrt{1128.8}}{115.6} = 29 \text{ per cent,}$$

$$\frac{\sqrt{959.4}}{99.7} = 31 \text{ per cent, and}$$

$$\frac{\sqrt{270.1}}{26.4} = 62 \text{ per cent.}$$

The above computations have been carried through for illustrative purposes. In practice, one would select a transformation to stabilize the variances. In this case the individual variances, s_i^2, are almost ten times the corresponding means, $\bar{x}_i$. From equation (II-7), we note that the square root transformation is indicated. The square roots of the individual plant weights (table IV-2) are given in table IV-3. The means, variances, and the analysis of variance are presented in the table. The individual variances are remarkably uniform and appear to be unrelated to the means. This allows use of the generalized error variance in making the various tests of significance and in computing various other statistics.

In making tests of significance and in constructing confidence intervals, it is important that the assumptions underlying the analysis of variance and tests of significance be fulfilled. If the usual tests of significance are to be strictly valid, it is necessary that the error deviations (the deviation of the observations from the class or group means in the completely randomized design) be independently and normally distributed about zero with a common variance [100]. Cochran [54] discusses the consequences

when the assumptions are not true. If anormality is not serious, the usual tests of significance are little affected. Also, a transformation of the data to another scale of measurement may satisfy the requirements for making tests of significance [15]. Further discussion of the validity of tests of significance on data from experiments is given in Chapter II.

TABLE IV-3. Square roots of dry weights of shrub in table IV-2

	Normals		Offtypes	Aberrants	Total
	7.616	10.000	10.149	5.831	
	10.440	10.817	9.165	3.464	
	9.327	9.055	9.381	4.472	
	10.050	11.533	10.440	2.236	
	10.247	13.115	11.576	6.928	
	9.695	11.533	10.296	5.657	
	12.923	12.845	9.274	4.583	
	11.874	12.247	12.207	4.359	
	10.583	10.770	8.000	4.899	
	10.198	10.954	10.954	4.472	
	7.616	13.856	10.392	4.123	
	9.899	10.817	9.055	8.062	
	10.296	-	10.583	-	
	10.954	-	11.358	-	
	8.062	-	4.690	-	
Totals, $X_{i.}$	287.322		147.520	59.086	493.928
Means, $\bar{x}_{i.}$	10.64		9.83	4.92	9.15
$\sum_j X_{ij}^2$	3122		1496	317	4935
$X_{i.}^2/r_i$	3057.55		1450.81	290.93	4799.29
$\sum_j X_{ij}^2 - X_{i.}^2/r_i$	64.45		45.19	26.07	135.71
s_i^2	2.48		3.23	2.37	-

Analysis of variance

Source of variation	df	ss	ms
Among types	2	281.42	140.71
N vs O	1	6.278	-
N + O vs 2A	1	275.147	-
Within types	51	135.71	2.66
Total	53	417.13	-
Correction for mean	1	4517.87	-

IV-1.6 HIERARCHICAL CLASSIFICATIONS

In certain experimental situations the experimental unit may be subsampled or several readings or determinations made on each experimental unit.

For this particular situation an additional line appears in the analysis of variance table,

Source of variation	df	ss
Among treatments	$v-1$	$\sum \frac{X_{i..}^2}{rk} - \frac{X_{...}^2}{rkv}$
Experimental error	$v(r-1)$	$\sum_i \left\{ \sum_j \frac{X_{ij.}^2}{k} - \frac{X_{i..}^2}{rk} \right\}$
Sampling error	$rv(k-1)$	$\sum_i \sum_j \left\{ \sum_g X_{ijg}^2 - \frac{X_{ij.}^2}{k} \right\}$
Total	$rvk-1$	$\sum_i \sum_j \sum_g X_{ijg}^2 - \frac{X_{...}^2}{rkv}$

There are k samples per experimental unit. Each treatment is applied to r of the rv experimental units. The other symbols represent sums of quantities. For unequal numbers the values r_i and k_{ij} vary, as denoted by the subscripts. The treatment degrees of freedom and sum of squares may be partitioned into meaningful components as in example IV-2. Likewise, other sources of variation may be partitioned provided that the partitions are meaningful.

For certain types of experimental material the treatments form parts of a hierarchy of items. The v treatments may be composed of s species, n_i strains within each species, and r_{ij} individuals per strain. The analysis of variance table illustrating the sources of variation and degrees of freedom is

Source of variation	df	ss
Treatments	$v-1$	———
Among species	$s-1$	$\sum X_{i..}^2/r_{i.} - X_{...}^2/r_{..}$
Among strains within species	$v-s$	$\sum_i \left\{ \sum_j X_{ij.}^2/r_{ij} - X_{i..}^2/r_{i.} \right\}$
Within treatments	$r_{..}-v$	$\sum_i \sum_j \left\{ \sum_h X_{ijh}^2 - X_{ij.}^2/r_{ij} \right\}$
Total	$r_{..}-1$	$\sum\sum\sum X_{ijh}^2 - \frac{X_{...}^2}{r}$

where $r_{i.} = \sum_j r_{ij}$ and $r_{..} = \sum\sum r_{ij}$.

Analyses of the above types are discussed by Snedecor [273], Cochran and Cox [60], Crump [78,80], Ganguli [132], Kendall [179, p. 173-181], Mann [207], Wald [299], and Wilks [308].

IV-2 Least Squares Estimates and Expectation of Mean Squares

It is possible to use a variety of estimates of the population parameters in summarizing the data from a sample. The estimates will have various properties, such as minimum variance, biased, unbiased, etc. Of the several estimation procedures, the *least squares* estimate of a population parameter is "best" in the sense (i) that the sum of squares of the differences between the observed values and the least squares estimate is a minimum and (ii) that among all *unbiased* estimates which are linear functions of the sample data, the least squares estimate has the smallest sampling variance. A number of other methods of estimation, maximum likelihood, moment, minimum chi-square, etc., are available and may or may not give the same estimate of a parameter as the method of least squares.

Before obtaining the least squares estimates and their variances for a completely randomized design, the least squares estimate, $\hat{\mu}$, of the mean, μ, is obtained. The least squares estimate of the linear regression coefficient is presented after the sections on expectations of mean squares for completely randomized designs.

IV-2.1 SAMPLES OF *n* OBSERVATIONS

IV-2.1.1 Least squares estimate of the mean. The population mean is designated as the parameter μ and the least squares estimate as $\hat{\mu}$. It is desired to choose $\hat{\mu}$ such that the sum of squares of the deviations from $\hat{\mu}$ is a minimum. Graphically, this may be represented as the point on the curve at which the tangent to the curve has a slope of zero (figure IV-1). Borrowing a

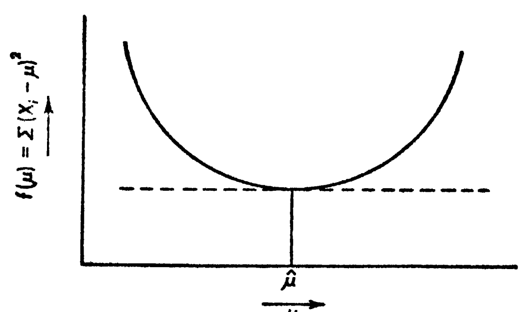


Figure IV-1. Sum of squares for various values assigned to μ.

method from differential calculus, the point at which $f(\mu)$ has a minimum is easily found. Since μ is the variable, the differentiation is with respect to μ, thus;

$$\frac{df(\mu)}{d\mu} = \text{slope of curve at any point } \mu = \mu_0. \qquad \text{(IV-4)}$$

Equation (IV-4) is set equal to zero, and the solution for μ is obtained, thus:

$$\frac{d\sum(X_i - \mu)^2}{d\mu} = -2\sum_{i=1}^{n}(X_i - \hat{\mu}) = 0. \tag{IV-5}$$

Therefore,

$$\hat{\mu} = \frac{\sum X_i}{n} = \bar{x}. \tag{IV-6}$$

The symbol $\hat{\mu}$ replaces μ whenever the n individuals observed represent a random sample of the total population. If all individuals in the population were observed, μ could be calculated exactly, and there would be no need for estimating the population mean.

From equation (IV-6), we see that the ordinary arithmetic mean is the least squares estimate of the population parameter μ. This makes the sum of the sample deviations, $\sum(X_i - \bar{x})$, equal to zero. Also, the sum of squares of the observed values X_i from $\bar{x}$ is a minimum.

The mean, $\bar{x}$, is subject to sampling variation. If another random sample were drawn from the population and an estimate of μ obtained, it is highly improbable that these values would be identical. The redeeming feature is that the estimates fall within a calculable interval in a specifiable proportion of the cases. To calculate the interval, an estimate of the variance of the population is needed. By the method of moments, it is possible to calculate the sampling variance.

IV-2.1.2 Variance of the sample mean. The first moment about the origin is defined as $E(X_i) = \mu$, which is the expected value or average value of any randomly drawn element from the population under observation. The above agrees with the least squares solution for μ; i.e., if all items in the population were averaged, the result would be μ. The second moment about the origin is defined as $E(X^2)$, and the second moment about the mean is defined as $E(X - \mu)^2 = \sigma^2$, where σ^2 is defined as the variance. In the normal distribution, $\mu \pm \sigma$ represent the points of inflection on the normal curve. Now the following is true:

$$\begin{aligned} E(X - \mu)^2 &= E(X^2 - 2X\mu + \mu^2) = EX^2 - 2\mu EX + \mu^2 \\ &= EX^2 - (EX)^2 = \sigma^2, \end{aligned} \tag{IV-7}$$

by use of the theorems (i) that the expected value of a sum equals the sum of the expected values and (ii) that the expected value of a constant is the constant.

The observation X_i may be represented as the mean of the population plus some random deviation (plus or minus) in the form of a *linear model*,

$$X_i = \mu + \epsilon_i, \tag{IV-8}$$

where ϵ_i is a random deviation which is estimated by $e_i = X_i - \bar{x}$. The mean of n observations may be represented by the linear model,

$$\bar{x} = \mu + \frac{\sum \epsilon_i}{n}. \tag{IV-9}$$

By definition the variance equals

$$E[X_i - \mu]^2 = E[\mu + \epsilon_i - \mu]^2 = E[\epsilon_i^2] = \sigma_\epsilon^2 = \sigma^2. \qquad \text{(IV-10)}$$

Therefore,

$$E\left[\sum_{i=1}^{n}(X_i - \bar{x})^2\right] = E\left[\sum_{i=1}^{n}X_i^2 - \frac{(\sum X)^2}{n}\right]$$

$$= \sum_{i=1}^{n}E[\mu + \epsilon_i]^2 - \frac{1}{n}E[n\mu + \sum\epsilon_i]^2$$

$$= n\mu^2 + n\sigma_\epsilon^2 - n\mu^2 - \sigma_\epsilon^2 = (n-1)\sigma_\epsilon^2, \qquad \text{(IV-11)}$$

if $\quad E[\mu\epsilon_i] = 0 = E[\epsilon_i\epsilon_j], j \neq i, \;$ and $E[\epsilon_i^2] = \sigma_\epsilon^2.$

The deviations are random independent variables. From equation (IV-11), we note that division by $n - 1$ results in an unbiased estimate of the population variance $\sigma_\epsilon^2 = \sigma^2$.:

The variance of the sample mean is

$$E(\bar{x} - \mu)^2 = E\left(\mu + \frac{\sum\epsilon_i}{n} - \mu\right)^2 = E\left(\frac{\sum\epsilon_i}{n}\right)^2$$

$$= \frac{1}{n^2}E[\epsilon_1 + \cdots + \epsilon_n]^2 = \frac{\sigma_\epsilon^2}{n}. \qquad \text{(IV-12)}$$

Therefore, the variance of the sample mean is estimated from the following formula:

$$\hat{\sigma}_{\bar{x}}^2 = \frac{\sum(X_i - \bar{x})^2}{n(n-1)} = s_{\bar{x}}^2. \qquad \text{(IV-13)}$$

The variance of the sample may be viewed in another manner. The sum of squares due to the mean, $\bar{x}$ = an estimate of μ, has the average value

$$E(\bar{x}\sum X_i) = \frac{E(\sum X_i)^2}{n} = \frac{E[\sum(\mu + \epsilon_i)]^2}{n} = \frac{n^2\mu^2 + n\sigma_\epsilon^2}{n} = n\mu^2 + \sigma_\epsilon^2, \qquad \text{(IV-14)}$$

where $\bar{x}\sum X_i$ is the ordinary correction for the mean. The total sum of squares has the expectation:

$$E\sum X_i^2 = \sum_{i=1}^{n}E(\mu + \epsilon_i)^2 = n\mu^2 + n\sigma_\epsilon^2. \qquad \text{(IV-15)}$$

Therefore, the expectation of the residual sum of squares after fitting the regression coefficient $\bar{x}$ is

$$n\mu^2 + n\sigma_\epsilon^2 - n\mu^2 - \sigma_\epsilon^2 = (n-1)\sigma_\epsilon^2, \qquad \text{(IV-16)}$$

with $n - 1$ degrees of freedom.

IV-2.2 COMPLETELY RANDOMIZED DESIGN WITH AN EQUAL NUMBER OF REPLICATES PER TREATMENT

For a completely randomized design, assume that the yield of an observation is represented by the linear model,

$$\begin{aligned} X_{ij} &= \mu_i + \epsilon_{ij} = \text{treatment mean} + \text{random deviation} \\ &= \mu + (\mu_i - \mu) + \epsilon_{ij} = \mu + \tau_i + \epsilon_{ij}, \end{aligned} \tag{IV-17}$$

where μ represents the population mean, $\tau_i = \mu_i - \mu$ = additive effect of the ith treatment or the partial regression coefficient for the ith treatment, ϵ_{ij} represents the random deviation of the jth member of the ith treatment, and $i = 1, 2, \cdots, v(v$ = number of treatments). The subscript $j = 1, 2, \cdots, r_i(r_i$ = number of replicates or individuals in the ith treatment) denotes the particular individual in a treatment. For the first situation, consider that all r_i are equal.

IV-2.2.1 Least squares estimates of effects.

The partial regression coefficients to be estimated are $\mu, \tau_1, \tau_2, \cdots, \tau_v$. In order to obtain a unique solution, the restriction that the sum of the t_i (t_i is the least squares estimate of τ_i) must equal zero,

$$\sum_{i=1}^{v} t_i = 0, \tag{IV-18}$$

is imposed, since we postulate that $E[\tau_i] = 0$ in the population.

The sum of squares to be minimized is[1]

$$R = \sum_i \sum_j (X_{ij} - \hat{\mu} - t_i)^2, \tag{IV-19}$$

which is the within treatment sum of squares. The partial derivatives of equation (IV-19) with respect to $\hat{\mu}$ and the t_i are set equal to zero; thus:

$$\frac{\partial R}{\partial \hat{\mu}} = -2\sum_i \sum_j (X_{ij} - \hat{\mu} - t_i) = 0, \tag{IV-20}$$

$$\frac{\partial R}{\partial t_1} = -2\sum_j (X_{1j} - \hat{\mu} - t_1) = 0, \tag{IV-21}$$

$$\frac{\partial R}{\partial t_2} = -2\sum_j (X_{2j} - \hat{\mu} - t_2) = 0, \tag{IV-22}$$

$$\vdots$$

$$\frac{\partial R}{\partial t_v} = -2\sum_j (X_{vj} - \hat{\mu} - t_v) = 0. \tag{IV-23}$$

[1]Strictly speaking, the sum of squares to be differentiated should be $\sum\sum(X_{ij} - \mu - \tau_i)^2$, and the differentiation should be with respect to $\mu, \tau_1, \tau_2, \ldots, \tau_v$. The resulting equations are then set equal to zero, and $\hat{\mu}, t_1, t_2, \ldots, t_v$ are the estimates of the parameters which satisfy these equations. From an operational standpoint, we use the estimates which minimize equation (IV-19).

From the above, $v+1$ normal equations are obtained; thus:

$$X_{..} = \sum_{i=1}^{v} \sum_{j=1}^{r} X_{ij} = r(\hat{t}_1 + \hat{t}_2 + \hat{t}_3 + \cdots + \hat{t}_v) + rv\hat{\mu}, \tag{IV-24}$$

$$X_{1.} = \sum_{j=1}^{r} X_{1j} = r\hat{t}_1 + r\hat{\mu}, \tag{IV-25}$$

$$X_{2.} = \sum X_{2j} = r\hat{t}_2 + r\hat{\mu}, \tag{IV-26}$$

$$\vdots$$

$$X_{v.} = \sum X_{vj} = r\hat{t}_v + r\hat{\mu}. \tag{IV-27}$$

Using the restriction that $\sum \hat{t}_i = 0$, the least squares estimate of the mean is

$$\hat{\mu} = \bar{x} = \frac{\sum\sum X_{ij}}{rv} = \frac{X_{..}}{rv}. \tag{IV-28}$$

Using this estimate of μ, the remaining constants are estimated as

$$\hat{t}_1 = \frac{X_{1.}}{r} - \hat{\mu} = \bar{x}_1 - \bar{x}, \tag{IV-29}$$

$$\hat{t}_2 = \frac{X_{2.}}{r} - \hat{\mu} = \bar{x}_2 - \bar{x}, \tag{IV-30}$$

$$\vdots$$

$$\hat{t}_v = \frac{X_{v.}}{r} - \hat{\mu} = \bar{x}_v - \bar{x}. \tag{IV-31}$$

The variances of the estimates may be obtained by the procedure described in section IV-2.1.2.

IV-2.2.2 Expectation of mean squares. Eisenhart [100] describes the analysis of variance under two different models, Model I and Model II. The nature of the data determines the appropriate model. For Model I, it is assumed that the treatment effects are fixed (i.e., $E[\tau_i] = \tau_i$) and that the ϵ_{ij} are random independent variates distributed around zero with a common variance. For Model II, it is assumed that the treatment effects, τ_i, and the ϵ_{ij} are random independent variates distributed around zero. The variance of the τ_i is σ_τ^2 and the variance of the ϵ_{ij} is σ_ϵ^2. The expectations of the mean squares under Model II are given in the present section, and the expectations of the mean squares under Model I are given in the last part of this section. Other models [78, 80, 169] are appropriate for the data from certain experiments, but these are not discussed here.

If $E(\tau_i^2) = \sigma_\tau^2$, $E(\epsilon_{ij}^2) = \sigma_\epsilon^2$, $E(\mu^2) = \mu^2$, and the expected value of all cross products is zero, the expectation of the sum of squares due to $\hat{\mu} = \bar{x}$ is

$$E[\hat{\mu}X..] = \frac{E(\sum\sum X_{ij})^2}{rv} = \frac{1}{rv}E[rv\mu + r\sum\tau_i + \sum\sum\epsilon_{ij}]^2$$
$$= rv\mu^2 + r\sigma_\tau^2 + \sigma_\epsilon^2. \qquad \text{(IV-32)}$$

The expectation of the sum of squares due to the t_i is[1]

$$E[t_1X_1. + t_2X_2. + t_3X_3. + \cdots + t_vX_v.]$$
$$= E\left[\frac{\sum X_i.^2}{r} - \hat{\mu}\sum X_i.\right] = E\left[\frac{\sum X_i.^2}{r} - \frac{X..^2}{rv}\right]$$
$$= \sum_{i=1}^{v}\frac{E[r\mu + r\tau_i + (\epsilon_{i1} + \epsilon_{i2} + \cdots + \epsilon_{ir})]^2}{r} - \frac{EX..^2}{rv}$$
$$= (v - 1)(\sigma_\epsilon^2 + r\sigma_\tau^2). \qquad \text{(IV-33)}$$

The residual sum of squares after fitting the constants μ and t_i has the expectation:

$$E\left[\sum_i\sum_j X_{ij}^2 - \hat{\mu}X.. - \sum_{i=1}^{v} t_iX_i.\right]$$
$$= \sum_{i=1}^{v}\sum_{j=1}^{r} E(\mu + \tau_i + \epsilon_{ij})^2 - E\left[\frac{\sum X_i.^2}{r} - \frac{X..^2}{rv} + \frac{X..^2}{rv}\right]$$
$$= rv\mu^2 + rv\sigma_\tau^2 + rv\sigma_\epsilon^2 - (rv\mu^2 + rv\sigma_\tau^2 + v\sigma_\epsilon^2)$$
$$= v(r - 1)\sigma_\epsilon^2. \qquad \text{(IV-34)}$$

The total sum of squares corrected for the mean has the expectation:

$$E\left[\sum_i\sum_j X_{ij}^2 - \frac{X..^2}{rv}\right] = rv\mu^2 + rv\sigma_\tau^2 + rv\sigma_\epsilon^2 - rv\mu^2 - r\sigma_\tau^2 - \sigma_\epsilon^2$$
$$= (v - 1)r\sigma_\tau^2 + (rv - 1)\sigma_\epsilon^2 = (v - 1)(\sigma_\epsilon^2 + r\sigma_\tau^2) + v(r - 1)\sigma_\epsilon^2 \qquad \text{(IV-35)}$$
$$= \text{treatment sum of squares} + \text{within treatment sum of squares.}$$

The estimated variance components in an experiment are obtained from the various mean squares in the analysis of variance. The estimated variance components are subject to error variation. Tukey [294] describes the method for obtaining the variance of a variance component.

The above expectations were obtained under the assumption that Model

[1]The sum of products rule used to obtain the sum of squares due to the t_i regression coefficients is as follows: (sum of squares due to $\hat{\mu}$ and t_i) − (sum of squares due to $\hat{\mu}'$), where $\hat{\mu}$ and t_i are the least squares estimates obtained from formulae (IV-28) to (IV-31) and $\hat{\mu}'$ is obtained by differentiating $\sum\sum(X_{ij} - \mu')^2$ with respect to μ', setting the result equal to zero, and obtaining the estimate, $\hat{\mu}'$, which satisfies this equation. For the case of equal numbers, $\hat{\mu}' = \hat{\mu}$. However, this is not the case for unequal numbers of observations or for non-orthogonal classifications. In the following, the above procedure is not always explicity stated but the reader should bear it in mind.

II is appropriate. The expectations are changed to some extent if Model I is appropriate for the data. The expectations are presented below.

Given the linear model described by equation (IV-17), let us assume that (i) the ϵ_{ij} are random independent variables from populations with zero means and that (ii) the τ_i are completely enumerated and fixed. From the above assumptions, $E(\epsilon_{ij}^2) = \sigma_\epsilon^2$, $E(\tau_i^2) = \tau_i^2$, the cross product of any two ϵ's has an expected value of zero, $E(\tau_i) = \tau_i$, and $E(\epsilon_{ij}) = 0$. We will also require that the sum of the τ_i's equals zero, $\sum_{i=1}^{v} \tau_i = 0$, although this is not necessary if we redefine τ_i. The present definition of τ_i is: the true treatment mean minus the true population mean equals the treatment effect τ_i. The average of the true treatment means is the population mean μ.

With the above restrictions on the linear model, we set up the following table of expectations of sums of squares: (This tabular form becomes increasingly useful as the number of classifications increases.)

	Coefficient of the parameters (Model I)		
Sum of squares	μ^2	σ_ϵ^2	$\sum_i \tau_i^2$
(1) $\sum_{i=1}^{v}\sum_{j=1}^{r} X_{ij}^2$	rv	rv	r
(2) $\sum_{i=1}^{v} X_{i.}^2/r$	rv	v	r
(3) $X_{..}^2/rv$	rv	1	0
Among treatments = (2) − (3)	0	(v − 1)	r
Within treatments = (1) − (2)	0	v(r − 1)	0

Therefore the expectation of the treatment mean square is $\sigma_\epsilon^2 + \frac{r}{v-1}\sum_{i=1}^{v} \tau_i^2$ as contrasted to the expectation $\sigma_\epsilon^2 + r\sigma_\tau^2$ obtained under Model II.

IV-2.3 COMPLETELY RANDOMIZED DESIGN WITH UNEQUAL NUMBERS OF REPLICATES PER TREATMENT

IV-2.3.1 Least squares estimates of effects. For the case where the $r_i \neq r$, the normal equations become

$$X_{..} = \sum r_i\hat{\mu} + \sum r_i\hat{t}_i, \qquad \text{(IV-36)}$$
$$X_{1.} = r_1\hat{t}_1 + r_1\hat{\mu}, \qquad \text{(IV-37)}$$
$$X_{2.} = r_2\hat{t}_2 + r_2\hat{\mu}, \qquad \text{(IV-38)}$$
$$\vdots$$
$$X_{v.} = r_v\hat{t}_v + r_v\hat{\mu}. \qquad \text{(IV-39)}$$

Applying the preceding method for estimating μ, the mean is estimated as

$$\hat{\mu} = \frac{X_{..}}{\sum r_i} - \frac{r_1 t_1 + r_2 t_2 + \cdots + r_v t_v}{\sum r_i}. \tag{IV-40}$$

This is not free of the t_i. Now any class mean, μ_i, minus the general mean, μ, is τ_i. An estimate of the τ_i is obtained as follows:

$$t_i = \frac{X_{i.}}{r_i} - \hat{\mu}. \tag{IV-41}$$

Summing the above v equations, an estimate of μ may be obtained from the following equation:[1]

$$\sum_i t_i = 0 = \sum\left(\frac{X_{i.}}{r_i}\right) - v\hat{\mu}; \tag{IV-42}$$

thus,

$$\hat{\mu} = \frac{1}{v}\sum\left(\frac{X_{i.}}{r_i}\right) = \frac{\sum \bar{x}_i}{v} \tag{IV-43}$$

$$= \text{arithmetic average of the treatment means.}$$

IV-2.3.2 Expectation of mean squares. The expectations of the total sum of squares, correction term for the mean, treatment sum of squares, and error sum of squares are, respectively:

$$E\left[\sum_{i=1}^{v}\sum_{j=1}^{r_i} X_{ij}^2\right] = \sum_{i=1}^{v} r_i(\mu^2 + \sigma_\tau^2 + \sigma_\epsilon^2), \tag{IV-44}$$

$$E\left[\frac{X_{..}^2}{\sum r_i}\right] = \left(\sum_{i=1}^{v} r_i\right)\mu^2 + \frac{\sum_{i=1}^{v} r_i^2\sigma_\tau^2}{\sum r_i} + \sigma_\epsilon^2, \tag{IV-45}$$

$$E\left[\sum_{i=1}^{v}\frac{X_{i.}^2}{r_i} - \frac{X_{..}^2}{\sum r_i}\right] = (v-1)\sigma_\epsilon^2 + \left(\sum r_i - \frac{\sum r_i^2}{\sum r_i}\right)\sigma_\tau^2, \tag{IV-46}$$

and

$$E\left[\sum_{i=1}^{v}\sum_{j=1}^{r_i} X_{ij}^2 - \sum_{i=1}^{v}\frac{X_{i.}^2}{r_i}\right]$$

$$= \sum_{i=1}^{v} r_i(\mu^2 + \sigma_\tau^2 + \sigma_\epsilon^2) - \left(v\sigma_\epsilon^2 + \sum r_i(\sigma_\tau^2 + \mu^2)\right)$$

$$= \sigma_\epsilon^2\left(\sum_{i=1}^{v} r_i - v\right). \tag{IV-47}$$

If the $r_i = r$, the above coefficients are the same as those obtained above for equal numbers of individuals per treatment. The expectations of the mean

[1] If the restriction $\sum r_i t_i = 0$ is used, the estimates t_i' will not be the same as those obtained in (IV-41). However, the difference $t_i - t_j$ is equal to the difference $t_i' - t_j'$. If one were dealing only with estimated differences between effects, the restriction $\sum r_i t_i$ might validly be used.

squares under Model I may be obtained in the manner described in the last part of section IV-2.2.2.

IV-2.4 EXPECTATION OF MEAN SQUARES IN HIERARCHICAL CLASSIFICATIONS

IV-2.4.1 Three categories of variation. As described previously the treatment degrees of freedom may be partitioned into several parts. Suppose that the v treatments are composed of k A classifications with n_g B classifications in the gth A class and that there are r_{gi} individuals within each of the B classifications. We assume the linear model,

$$\begin{aligned} X_{gij} &= \mu_{gi} + \epsilon_{gij} = \mu + (\mu_{g\cdot} - \mu) + (\mu_{gi} - \mu_{g\cdot}) + \epsilon_{gij} \\ &= \mu + \alpha_g + \beta_{gi} + \epsilon_{gij}, \end{aligned} \qquad \text{(IV-48)}$$

where $g = 1, 2, \cdots, k$; $i = 1, 2, \cdots, n_g$; $j = 1, 2, \cdots, r_{gi}$; $\alpha_g = \mu_{g\cdot} - \mu$ = additive effect of gth A class; $\beta_{gi} = \mu_{gi} - \mu_{g\cdot}$ = additive effect of ith B class in the gth A class; ϵ_{gij} = a random error associated with an individual; and μ = population mean. Furthermore, assume that the expectation of cross products of different effects equals zero, that each effect has expectation zero, and that

$$E(\alpha_g^2) = \sigma_\alpha^2,\ E(\beta_{gi}^2) = \sigma_\beta^2,\ E(\epsilon_{gij}^2) = \sigma_\epsilon^2,$$

$$\sum_{i=1}^{n_g} r_{gi} = r_{g\cdot},\ \sum_g\sum_i r_{gi} = r_{\cdot\cdot},\ \text{and} \sum_{g=1}^{k} n_g = n_\cdot = v.$$

With these definitions and assumptions the expectations of the various sums of squares are

Sums of squares	Coefficients of the parameters (Model II) μ^2	σ_α^2	σ_β^2	σ_ϵ^2
$\sum_{g=1}^{k}\sum_{i=1}^{n_g}\sum_{j=1}^{r_{gi}} X_{gij}^2$	$r_{\cdot\cdot}$	$r_{\cdot\cdot}$	$r_{\cdot\cdot}$	$r_{\cdot\cdot}$
$\sum_g\sum_i X_{gi\cdot}^2/r_{gi}$	$r_{\cdot\cdot}$	$r_{\cdot\cdot}$	$r_{\cdot\cdot}$	$n_\cdot = v$
$\sum_g X_{g\cdot\cdot}^2/r_{g\cdot}$	$r_{\cdot\cdot}$	$r_{\cdot\cdot}$	$\sum_g\sum_i \frac{r_{gi}^2}{r_{g\cdot}}$	k
$X_{\cdot\cdot\cdot}^2/r_{\cdot\cdot}$	$r_{\cdot\cdot}$	$\sum_g r_{g\cdot}^2/r_{\cdot\cdot}$	$\sum_g\sum_i r_{gi}^2/r_{\cdot\cdot}$	1

From the above table of coefficients the expectations of the various sums of squares in the analysis of variance table are [132]

Source of variation	df	Average value of sum of squares
Among A classes	$k - 1$	$(k-1)\sigma_\epsilon^2 + \left\{\sum\sum r_{gi}^2\left[\frac{1}{r_{g\cdot}} - \frac{1}{r_{\cdot\cdot}}\right]\right\}\sigma_\beta^2 + \left\{r_{\cdot\cdot} - \frac{1}{r_{\cdot\cdot}}\sum r_{g\cdot}^2\right\}\sigma_\alpha^2$
Among B classes within A classes	$v - k$	$(v-k)\sigma_\epsilon^2 + \{r_{\cdot\cdot} - \sum\sum r_{gi}^2/r_{g\cdot}\}\sigma_\beta^2$
Within B classes	$r_{\cdot\cdot} - v$	$(r_{\cdot\cdot} - v)\sigma_\epsilon^2$

For an illustrative example of the above analysis the reader is referred to the data presented in tables 10.12 and 10.18 of Snedecor's book [273]. The average value of the mean square for litters is (in Snedecor's notation) $\sigma^2 + 7.00\sigma_L^2$ and of the mean square for litter sizes is $\sigma^2 + 6.76\sigma_L^2 + 13.52\sigma_S^2$.

In the above analysis of variance, it should be noted that there is no appropriate error mean square for testing the A class mean square in an F test. The disagreement in the coefficients of σ_L^2 in the two mean squares from table 10.18 [273] is not serious in this case, 7.00 vs 6.76, but it could be in certain cases. This is another undesirable feature of unequal numbers in the completely randomized design [270].

IV-2.4.2 Five categories of variation. A more complex classification is presented by the data of example 10.19 in Snedecor's book [273]. The linear model for this example is

$$X_{ijkgm} = \mu + \alpha_i + \beta_{ij} + \gamma_{ijk} + \delta_{ijkg} + \epsilon_{ijkgm}, \tag{IV-49}$$

where $i = 1, 2, \cdots, a$; $j = 1, 2, \cdots, b_i$; $k = 1, 2, \cdots, c_{ij}$; $g = 1, 2, \cdots, d_{ijk}$; $m = 1, 2, \cdots, r_{ijkg}$; $r_{i\cdots} = \sum_j\sum_k\sum_g r_{ijkg}$; $r_{ij\cdot\cdot} = \sum_k\sum_g r_{ijkg}$; $r_{ijk\cdot} = \sum_g r_{ijkg}$;

and the totals of the X's are obtained by summations over the X's similar to those above for the r's. The effects α, β, γ, δ, and ϵ are considered to be random independent variables with zero means; this corresponds to Eisenhart's Model II [80, 100]. However, a mixed form of Models I and II is probably more appropriate for these data. The sums of squares and expectations are presented in tables IV-4 and IV-5.

IV-2.5 REGRESSION COEFFICIENTS AND THE INTERCEPT

IV-2.5.1 Least squares estimate of the regression coefficient and of the intercept. The method of least squares for estimating the regression of one variable, say Y, on another, say X, and the intercept of the line from the equation,

$$\hat{Y}_i = a + bX_i, \tag{IV-50}$$

is discussed by Wilks [310, sec. 13:21 to 13:24] and by several other authors. Suppose that some variable is observed on n individuals randomly drawn from a population and that the measurements or counts are recorded as Y_1, Y_2, $\cdots$, Y_n. Also, suppose that an additional observation is made on each of the n individuals; i.e., $X_1, X_2, \cdots, X_n$. The X_i are called the independent variates; they are known without error and are often selected values. This means that on each individual item a pair of observations, Y_i and X_i, is made. If the paired data are plotted in the XY plane, the n pairs of observations form n points or coordinates. The resulting plot is a *dot* or *scatter diagram*. Now, it is desired to determine the best-fitting straight line to the n points or coordinates in the sense that the sum of squares of the deviations of the observed values, Y_i, from the corresponding calculated value, $\hat{Y}_i$, on the line will be a minimum. In

TABLE IV-4. Coefficients of parameters for the various sums of squares

Sum of squares	Coefficients of parameters					
	μ^2	σ_α^2	σ_β^2	σ_γ^2	σ_δ^2	σ_ϵ^2
$\sum\sum\sum\sum\sum_{ijkgm} X^2_{ijkgm}$	$r_{....}$	$r_{....}$	$r_{....}$	$r_{....}$	$r_{....}$	$r_{....}$
$\sum\sum\sum\sum_{ijkg} X^2_{ijkg.}/r_{ijkg.}$	$r_{....}$	$r_{....}$	$r_{....}$	$r_{....}$	$r_{....}$	$\sum\sum\sum_{ijk} d_{ijk}$
$\sum\sum\sum_{ijk} X^2_{ijk..}/r_{ijk.}$	$r_{....}$	$r_{....}$	$r_{....}$	$r_{....}$	$\sum\sum\sum\sum_{ijkg} \frac{r^2_{ijkg}}{r_{ijk.}}$	$\sum\sum_{ij} c_{ij}$
$\sum\sum_{ij} X^2_{ij...}/r_{ij..}$	$r_{....}$	$r_{....}$	$r_{....}$	$\sum\sum\sum_{ijk} \frac{r^2_{ijk.}}{r_{ij..}}$	$\sum\sum\sum\sum_{ijkg} \frac{r^2_{ijkg}}{r_{ij..}}$	$\sum_i b_i$
$\sum_i X^2_{i....}/r_{i...}$	$r_{....}$	$r_{....}$	$\sum\sum_{ij} \frac{r^2_{ij..}}{r_{i...}}$	$\sum\sum\sum_{ijk} \frac{r^2_{ijk.}}{r_{i...}}$	$\sum\sum\sum\sum_{ijkg} \frac{r^2_{ijkg}}{r_{i...}}$	a
$X^2_{.....}/r_{....}$	$r_{....}$	$\sum_i \frac{r^2_{i...}}{r_{....}}$	$\sum\sum_{ij} \frac{r^2_{ij..}}{r_{....}}$	$\sum\sum\sum_{ijk} \frac{r^2_{ijk.}}{r_{....}}$	$\sum\sum\sum\sum_{ijkg} \frac{r^2_{ijkg}}{r_{....}}$	1

TABLE IV-5. Analysis of variance

Source of variation	df	Sum of squares	Average value of sum of squares
Among A classes	$a - 1$	$\sum_i \frac{X^2_{i\ldots.}}{r_{i\ldots}} - \frac{X^2_{\ldots\ldots}}{r_{\ldots.}}$	$(a-1)\sigma^2_\epsilon + \sum_i \left[\frac{1}{r_{i\ldots}} - \frac{1}{r_{\ldots.}}\right] \left[\sigma^2_\delta \sum\sum\sum_{jkg} r^2_{ijkg} + \sigma^2_\gamma \sum\sum_{jk} r^2_{ijk.} + \sigma^2_\beta \sum_j r^2_{ij..}\right] + \left[r_{\ldots.} - \frac{1}{r_{\ldots.}} \sum_i r^2_{i\ldots}\right] \sigma^2_\alpha$
Among B classes within A classes	$\Sigma\, b_i - a$	$\sum_i \left[\sum_j \frac{X^2_{ij\ldots}}{r_{ij..}} - \frac{X^2_{i\ldots.}}{r_{i\ldots}}\right]$	$(\Sigma\, b_i - a)\sigma^2_\epsilon + \sum\sum_{ij} \left[\frac{1}{r_{ij..}} - \frac{1}{r_{i\ldots}}\right] \left[\sigma^2_\delta \sum\sum_{kg} r^2_{ijkg} + \sigma^2_\gamma \sum_k r^2_{ijk.}\right] + \left[r_{\ldots.} - \sum\sum_{ij} \frac{r^2_{ij.}}{r_{i\ldots}}\right] \sigma^2_\beta$
Among C classes within B classes	$\Sigma\Sigma\, c_{ij} - \Sigma\, b_i$	$\sum\sum_{ij} \left[\sum_k \frac{X^2_{ijk..}}{r_{ijk.}} - \frac{X^2_{ij\ldots}}{r_{ij..}}\right]$	$(\Sigma\Sigma\, c_{ij} - \Sigma\, b_i)\sigma^2_\epsilon + \sigma^2_\delta \sum\sum\sum\sum_{ijkg} r^2_{ijkg} \left[\frac{1}{r_{ijk.}} - \frac{1}{r_{ij..}}\right] + \left[r_{\ldots.} - \sum\sum\sum_{ijk} \frac{r^2_{ijk.}}{r_{ij..}}\right] \sigma^2_\gamma$
Among D classes within C classes	$\Sigma\Sigma\Sigma\, d_{ijk} - \Sigma\Sigma\, c_{ij}$	$\sum\sum\sum_{ijk} \left[\sum_g \frac{X^2_{ijkg.}}{r_{ijkg}} - \frac{X^2_{ijk..}}{r_{ijk.}}\right]$	$(\Sigma\Sigma\Sigma\, d_{ijk} - \Sigma\Sigma\, c_{ij})\sigma^2_\epsilon + \left[r_{\ldots.} - \sum\sum\sum\sum_{ijkg} \frac{r^2_{ijkg}}{r_{ijk.}}\right] \sigma^2_\delta$
Among individuals	$r_{\ldots.} - \Sigma\Sigma\Sigma\, d_{ijk}$	$\sum\sum\sum\sum_{ijkg} \left[\sum_m X^2_{ijkgm} - \frac{X^2_{ijkg.}}{r_{ijkg}}\right]$	$(r_{\ldots.} - \Sigma\Sigma\Sigma\, d_{ijk})\sigma^2_\epsilon$

order to do this, it is necessary to determine the intercept, a, and the slope of the line, b. Since these values are determined to make the sum of squares of residuals a minimum in the sample, the partial differentiation is with respect to a and to b. The value for any randomly drawn value Y_i may be expressed in a linear form in terms of the population parameters α and β or in terms of the sample values; thus:

$$Y_i = \alpha + \beta X_i + \epsilon_i = a + bX_i + e_i, \tag{IV-51}$$

where ϵ_i is a random variable and e_i is the deviation between the observed Y_i and the calculated $\hat{Y}_i = a + bX_i$ and where a and b are the least squares estimates of α and β, respectively. The proper choice of a and b makes the sum of squares,

$$\sum_{i=1}^{n}(Y_i - \hat{Y}_i)^2 = \sum_{i=1}^{n}(Y_i - a - bX_i)^2 \tag{IV-52}$$

a minimum. Now,

$$\frac{\partial[\sum(Y_i - a - bX_i)^2]}{\partial a} = -2\sum(Y_i - a - bX_i) = 0, \tag{IV-53}$$

and

$$na = \sum Y_i - b\sum X_i, \tag{IV-54}$$

or

$$a = \bar{y} - b\bar{x}. \tag{IV-55}$$

$$\frac{\partial[\sum(Y_i - a - bX_i)^2]}{\partial b} = -2\sum X_i(Y_i - a - bX_i) = 0. \tag{IV-56}$$

$$b\sum X_i^2 = \sum X_iY_i - a\sum X_i. \tag{IV-57}$$

The two equations and two unknowns may be solved as follows:

Multiply equation (IV-57) by n and (IV-54) by $\sum X_i$ to obtain

$$[b\sum X_i^2 + a\sum X_i = \sum X_iY_i]n \tag{IV-58}$$

and

$$[b\sum X_i + na = \sum Y_i]\sum X_i. \tag{IV-59}$$

Subtract (IV-59) from (IV-58) and obtain

$$b[n\sum X_i^2 - (\sum X_i)^2] = n\sum X_iY_i - \sum X_i\sum Y_i. \tag{IV-60}$$

The least squares estimate of the slope is

$$b = \frac{n\sum X_iY_i - \sum X_i\sum Y_i}{n\sum X_i^2 - (\sum X_i)^2} = \frac{\sum(X_i - \bar{x})(Y_i - \bar{y})}{\sum(X_i - \bar{x})^2} \tag{IV-61}$$

and of the intercept is

$$a = \bar{y} - b\bar{x} = \bar{y} - \bar{x}\frac{\sum(X_i - \bar{x})(Y_i - \bar{y})}{\sum(X_i - \bar{x})^2}. \tag{IV-62}$$

The values a and b define the "best-fitting" straight line to the n pairs of points in the scatter diagram.

In order to introduce the reader to the solution of simultaneous equations by the method of determinants, the solutions of a and b from the two normal equations,

$$b\sum X_i^2 + a\sum X_i = \sum X_i Y_i \tag{IV-63}$$

and

$$b\sum X_i + na = \sum Y_i, \tag{IV-64}$$

are obtained in the following manner:

$$b = \frac{\begin{vmatrix} \sum X_i Y_i & \sum X_i \\ \sum Y_i & n \end{vmatrix}}{\begin{vmatrix} \sum X_i^2 & \sum X_i \\ \sum X_i & n \end{vmatrix}} = \frac{n\sum X_i Y_i - \sum X_i \sum Y_i}{n\sum X_i^2 - (\sum X_i)^2}$$

$$= \frac{\sum (X_i - \bar{x})(Y_i - \bar{y})}{\sum (X_i - \bar{x})^2}, \tag{IV-65}$$

and

$$a = \frac{\begin{vmatrix} \sum X_i^2 & \sum X_i Y_i \\ \sum X_i & \sum Y_i \end{vmatrix}}{\begin{vmatrix} \sum X_i^2 & \sum X_i \\ \sum X_i & n \end{vmatrix}} = \frac{\sum Y_i \sum X_i^2 - \sum X_i \sum X_i Y_i}{n\sum X_i^2 - (\sum X_i)^2}$$

$$= \bar{y} - \bar{x}\frac{\sum (X_i - \bar{x})(Y_i - \bar{y})}{\sum (X_i - \bar{x})^2}. \tag{IV-66}$$

For more complicated situations the notion of a matrix and of an inverse matrix [116] is of considerable importance for obtaining the variances of least squares estimates. For the preceding example the matrix of coefficients is

$$\begin{vmatrix} \sum X_i^2 & \sum X_i \\ \sum X_i & n \end{vmatrix}, \tag{IV-67}$$

and the determinant of the matrix is

$$\begin{vmatrix} \sum X_i^2 & \sum X_i \\ \sum X_i & n \end{vmatrix} = n\sum X_i^2 - (\sum X_i)^2 = D. \tag{IV-68}$$

The inverse of the matrix of coefficients, equation (IV-67), is

$$\begin{vmatrix} \sum X_i^2 & \sum X_i \\ \sum X_i & n \end{vmatrix}^{-1} = \begin{vmatrix} \dfrac{n}{D} & \dfrac{-\sum X_i}{D} \\ \dfrac{-\sum X_i}{D} & \dfrac{\sum X_i^2}{D} \end{vmatrix}$$

$$= \frac{1}{D}\begin{vmatrix} n & -\sum X_i \\ -\sum X_i & \sum X_i^2 \end{vmatrix}. \tag{IV-69}$$

The predicted values of the dependent variate for a given X_i in terms of the least squares estimates are given by the equation,

$$\hat{Y}_i = a + bX_i = \bar{y} + b(X_i - \bar{x}). \tag{IV-70}$$

IV-2.5.2 Variances and covariances of the estimates. The variance of the least squares estimate of the population intercept α is $E(a^2) - \alpha^2$. Now,

$$\begin{aligned} E(a^2) &= E\left[\frac{\sum Y_i}{n} - \frac{\sum X_i}{n}\left\{\frac{n\sum X_iY_i - \sum X_i\sum Y_i}{n\sum X_i^2 - (\sum X_i)^2}\right\}\right]^2 \\ &= E\left[\alpha + \beta\bar{x} + \frac{\sum \epsilon_i}{n} - \bar{x}\frac{(n\beta\sum X_i^2 - \beta(\sum X_i)^2 + n\sum X_i\epsilon_i - \sum X_i\sum \epsilon_i)}{n\sum(X - \bar{x})^2}\right]^2 \\ &= \alpha^2 + E\left[\left(\frac{\sum \epsilon_i}{n}\right)^2 + \bar{x}^2\left(\frac{\sum(X - \bar{x})(\epsilon_i - \bar{\epsilon})}{\sum(X_i - \bar{x})^2}\right)^2 + \text{cross products}\right] \\ &= \alpha^2 + \frac{\sigma_\epsilon^2\sum X_i^2}{n\sum(X_i - \bar{x})^2}. \end{aligned} \tag{IV-71}$$

The variance of the least squares estimate a is $E(a - \alpha)^2$; the estimated variance is

$$\hat{\sigma}_a^2 = \frac{\hat{\sigma}_\epsilon^2\sum X_i^2}{n\sum(X_i - \bar{x})^2}, \tag{IV-72}$$

where $\hat{\sigma}_\epsilon^2$ is the sample estimate of the population parameter $\sigma_\epsilon^2 = \sigma^2$.

Also, the variance of the estimate b is $E(b^2) - \beta^2$

$$\begin{aligned} E(b^2) &= E\left[\frac{\sum(X - \bar{x})(Y - \bar{y})}{\sum(X - \bar{x})^2}\right]^2 = E\left[\frac{\sum XY - \sum X\sum Y/n}{\sum(X - \bar{x})^2}\right]^2 \\ &= \beta^2 + 0 + E\left[\frac{\sum(X - \bar{x})(\epsilon_i - \bar{\epsilon})}{\sum(X - \bar{x})^2}\right]^2 = \beta^2 + \frac{\sigma_\epsilon^2}{\sum(X - \bar{x})^2}. \end{aligned} \tag{IV-73}$$

Therefore,

$$E(b)^2 - \beta^2 = \frac{\sigma_\epsilon^2}{\sum(X - \bar{x})^2} = \sigma_\beta^2, \tag{IV-74}$$

and is estimated by

$$\hat{\sigma}_\beta^2 = \frac{\hat{\sigma}_\epsilon^2}{\sum(X - \bar{x})^2}. \tag{IV-75}$$

The covariance of a and b is

$$E(a - \alpha)(b - \beta) = \sigma_{\alpha\beta} = \frac{-\sigma_\epsilon^2\sum X_i}{n\sum(X_i - \bar{x})^2} \tag{IV-76}$$

and is estimated by

$$\frac{-\hat{\sigma}_\epsilon^2\sum X_i}{n\sum(X_i - \bar{x})^2}. \tag{IV-77}$$

The elements of the inverse matrix times $\hat{\sigma}_\epsilon^2$ give the sampling variance of the least squares estimates a and b and their covariance; thus:

$$\hat{\sigma}_\beta^2 = \frac{n}{D}\hat{\sigma}_\epsilon^2 = \frac{\hat{\sigma}_\epsilon^2}{\sum(X - \bar{x})^2}, \tag{IV-78}$$

$$\hat{\sigma}_\alpha^2 = \frac{\sum X_i^2}{D}\hat{\sigma}_\epsilon^2 = \frac{\sum X_i^2 \hat{\sigma}_\epsilon^2}{n\sum X_i^2 - (\sum X_i)^2} = \frac{\sum X_i^2 \hat{\sigma}_\epsilon^2}{n\sum(X_i - \bar{x})^2}, \tag{IV-79}$$

and

$$\hat{\sigma}_{\alpha\beta} = \frac{-\sum X_i}{D}\hat{\sigma}_\epsilon^2 = \frac{-\hat{\sigma}_\epsilon^2 \sum X_i}{n\sum(X_i - \bar{x})^2}. \tag{IV-80}$$

The above use of the inverse matrix is extremely helpful in obtaining the variance of least squares estimates in the more complex cases.

With these results in mind, it is possible to obtain the reduction in the sum of squares due to the intercept, a (in reality a regression coefficient), and to the linear regression coefficient or the slope, b. The reduction is the value of the quantity,

$$a\sum Y_i + b\sum X_i Y_i.$$

It will be noted that the totals $\sum Y_i$ and $\sum X_i Y_i$ are the right-hand sides of equations (IV-63) and (IV-64). The total sum of squares, $\sum Y_i^2$, minus the sums of squares due to the regression coefficients a and b, has the expectation:

$$\begin{aligned} E\left[\sum_{i=1}^{n} Y_i^2 - a\sum Y_i - b\sum X_i Y_i\right] &= E\left[\sum_{i=1}^{n} Y_i^2 - \frac{(\sum Y_i)^2}{n} - b^2\sum(X_i - \bar{x})^2\right] \\ &= \sum_{i=1}^{n} E(\alpha + \beta X_i + \epsilon_i)^2 - \frac{1}{n}E(n\alpha + \beta\sum X_i + \sum\epsilon_i)^2 - E\sum(X_i - \bar{x})^2 b^2 \\ &= n\alpha^2 + \beta^2\sum X_i^2 + n\sigma_\epsilon^2 - n\alpha^2 - \frac{\beta^2(\sum X_i)^2}{n} - \sigma_\epsilon^2 - \sum(X_i - \bar{x})^2(\sigma_\beta^2 + \beta^2) \\ &= (n-1)\sigma_\epsilon^2 - \sigma_\beta^2\sum(X_i - \bar{x})^2 = (n-2)\sigma_\epsilon^2. \end{aligned} \tag{IV-81}$$

Hence, division of the residual sum of squares by $n - 2$ results in an unbiased estimate of the variance in the population.

The expectation of the residual sum of squares may be obtained similarly for multiple regression, that is, for each observation Y_i, some related values X_{1i}, X_{2i}, etc. are observed. Their effect on the variation in the Y_i may be removed and the residual sum of squares may be obtained. For the case of two independent variables, X_1 and X_2, the sum of squares to be minimized is

$$\sum(Y_i - a - bX_{1i} - cX_{2i})^2, \tag{IV-82}$$

the three normal equations are

$$an + b\sum X_{1i} + c\sum X_{2i} = \sum Y_i, \tag{IV-83}$$

$$a\sum X_{1i} + b\sum X_{1i}^2 + c\sum X_{1i}X_{2i} = \sum X_{1i}Y_i, \tag{IV-84}$$

$$a\sum X_{2i} + b\sum X_{1i}X_{2i} + c\sum X_{2i}^2 = \sum X_{2i}Y_i, \tag{IV-85}$$

and the reduction in the total sum of squares due to the three estimated regression coefficients a, b, and c, is

$$a\sum Y_i + b\sum X_{1i}Y_i + c\sum X_{2i}Y_i. \tag{IV-86}$$

The residual sum of squares has the expectation $(n - 3)\sigma_\epsilon^2$.

CHAPTER V

Randomized Complete Block Design

V-1 Applications of the Randomized Complete Block Design

V-1.1 INTRODUCTION

If the whole of the experimental material, area, or time is not homogeneous, it may be possible to stratify or group the material into homogeneous subgroups. As explained in Chapter I, this is one of the methods for controlling the variability of experimental material. If the treatments are applied to the relatively homogeneous material within each stratum or group and replicated on the other strata, the design is a randomized complete block. For the completely randomized design, no stratification of the experimental site (space, material, or time) is made. The treatments are randomly allotted to the experimental unit. In the randomized complete block design the treatments are randomly allotted *within each stratum*, i.e., the randomization is restricted. Also, the variation among strata (replicates or blocks) is removed from the variation among replicates within treatments. Therefore, if it is desired to control one source of variation by stratification, the experimenter should select the randomized complete block rather than the completely randomized design.

Since the development of the randomized complete block design about 1925 [125, 127], the design has become extremely popular in a large number of fields. Its flexibility and ease of adaptation and analysis have made it the most popular of all designs, with the latin square being its closest rival.

The blocks or replicates used may be days, observers, batches of material, animals, pens, patients, schools, classes, laboratories, ovens, etc., provided that these categories do not interact with the treatments (see section IX-3.5). In other words, the design may be used to control a source of variation in the experimental material and not solely the variation among blocks in a field.

V-1.2 ADVANTAGES AND DISADVANTAGES

The chief advantages of the randomized complete block design are:

(i) Accuracy. This design has been shown to be more accurate than the completely randomized design for most types of experimental work. The elimination of

the block sum of squares from the error sum of squares usually results in a decrease in the error mean square.

(ii) Flexibility. No restrictions are placed on the number of treatments or on the number of replicates in the experiment. In general, at least two replicates are required to obtain tests of significance (see chapters on factorial experiments for exceptions). In addition, the check or other treatments may be included more than once with little complication to the analysis.

(iii) Ease of analysis. The statistical analysis is simple and rapid. Moreover, the error of any treatment comparison may be isolated and any number of treatments may be omitted from the analysis without complicating it. These facilities may be useful when certain treatment differences turn out to be very large, when some treatments produce failures, or when the experimental errors for the various comparisons are heterogeneous.

The chief disadvantage of the randomized complete block design is that it is not suitable for large numbers of treatments or for cases in which the complete block contains considerable variability.

Cochran [45, 47] found for experiments carried out at the Rothamsted Experimental Station and associated centers between 1927 and 1934 that the error mean square for a randomized complete block design was 60 per cent of the error for completely randomized designs. Assuming equal costs of conducting the experiments, it would require ten replicates of a completely randomized design to obtain an amount of information equal to that from a randomized complete block design with six replicates.

V-1.3 LAYOUT AND ANALYSIS

Using the same example as for the completely randomized design the five treatments A, B, C, D, and E may be included in each of the four blocks. The following diagram illustrates an experimental layout for the field, laboratory, or greenhouse:

Block I	(E) (1)	(A) (2)	(C) (3)	(B) (4)	(D) (5)
Block II	(A) (10)	(D) (9)	(B) (8)	(C) (7)	(E) (6)
Block III	(B) (11)	(C) (12)	(A) (13)	(E) (14)	(D) (15)
Block IV	(E) (20)	(D) (19)	(A) (18)	(B) (17)	(C) (16)

The breakdown of the total degrees of freedom is

Source of variation	df	Mean square
Among 5 treatments	4	T
Among 4 blocks	3	R
Remainder or error	12	E
Total	19	—

As is apparent from the analysis, 3 of the degrees of freedom are segregated from the error degrees of freedom for a completely randomized design. These 3 degrees of freedom are associated with the sum of squares attributable to the differences among the means of the four blocks.

For the general case with v treatments and r replicates the breakdown of the total degrees of freedom for a randomized complete block design is

Source of variation	df	ss	ms
Replicates	$r-1$	$\sum_j X_{.j}^2/v - X_{..}^2/rv$	R
Treatments	$v-1$	$\sum_i X_{i.}^2/r - X_{..}^2/rv$	T
Residual	$(r-1)(v-1)$	$\sum_i\sum_j X_{ij}^2 - \sum X_{i.}^2/r - \sum X_{.j}^2/v + X_{..}^2/rv$	E
Total (corrected for the mean)	$rv-1$	$\sum_i\sum_j X_{ij}^2 - X_{..}^2/rv$	—
Correction for the mean	1	$X_{..}^2/rv$	CT

$X_{i.}$ = ith treatment total, $X_{.j}$ = jth replicate total, $X_{..}$ = total of rv experimental units, and X_{ij} = yield of the experimental unit from the ith treatment in the jth replicate.

If an experiment has been conducted as a randomized complete block design, it is possible to determine the efficiency for the same experiment conducted as a completely randomized design [319]. The calculated variance for the latter design is obtained from the sum of squares for replicates plus the sum of squares obtained by multiplying the error mean square by the treatment plus error degrees of freedom and dividing by the total degrees of freedom. Symbolically, this is

$$E' = \frac{(\text{treatment plus error degrees of freedom})E + (\text{block degrees of freedom})R}{\text{treatment} + \text{error} + \text{block degrees of freedom}}$$

$$= \frac{[v-1+(r-1)(v-1)]E + [r-1]R}{[(v-1)+(r-1)(v-1)+(r-1)] = rv-1}. \tag{V-1}$$

The efficiency of the randomized complete block design relative to the completely randomized design is the ratio of the relative amounts of information on the two designs. The amount of information [126, 273] is defined as the reciprocal of the error variance. The efficiency of the randomized complete block design relative to what it would be had a completely randomized design been used is

$$\frac{(r-1)(v-1)+1}{[(r-1)(v-1)+3]E}\bigg/\frac{v(r-1)+1}{[v(r-1)+3]E'} = \frac{(rv-r-v+2)(rv-v+3)E}{(rv-r-v+4)(rv-v+1)E'}, \tag{V-2}$$

where the coefficients represent the corrections for the difference in the degrees of freedom associated with E' and E [126, and formula (I-1)]. The

increase in efficiency due to the use of the randomized complete block design rather than the completely randomized design is obtained from equation (V-2) minus one expressed in per cent.

With reference to the suggested replicate shape and the suggested size and shape of experimental units within the replicate the reader is referred to Chapter III. The amount of replication required will depend upon the precision with which the experimenter wishes to measure the treatment means. He usually has some idea regarding the coefficient of variation in the material under observation. Also, he has some idea of the size of the difference between two treatments, which is of practical significance. From this information the approximate number of replicates to use may be obtained by one of the methods presented in Chapter III.

The layout, computational procedure, and efficiency of randomized complete block designs are illustrated in the following examples.

V-1.4 ANALYSIS FOR ONE OBSERVATION PER EXPERIMENTAL UNIT

The general analysis for complete "harvesting" of the experimental unit is presented above. In this case the total for an experimental unit is used; this results in a single observation per experimental unit. The computational procedure for a randomized complete block design with one observation per experimental unit is illustrated below for a particular example.

Example V-1. Lathwell and Evans [191] present yield data from soybean plants for five treatments grown in six replicates of a randomized complete block design. The experiment was conducted in the greenhouse, using sand cultures in pots. The five treatments are *LLL*, *LLH*, *LHH*, *HLL*, and *HHL*, where *L* refers to a light application of nitrogen (20 parts per million), *H* refers to a heavy application of nitrogen (100 ppm), and the position of the letters refers to the time of application, i.e., first, second, and third dates. The additional treatments, *LHL*, *HLH*, and *HHH*, would have resulted in a factorial arrangement of treatments, but they were not included in this experiment.

The yield data and analysis of variance for the soybean experiment are presented in tables V-1 and V-2. The total sum of squares corrected for the mean is obtained as $8.8^2 + 12.9^2 + \cdots + 36.6^2 - 924.9^2/5(6) = 35675.03 - 28514.67 = 7160.36$. The treatment sum of squares is $(96.5^2 + 134.7^2 + 170.0^2 + 220.1^2 + 303.6^2)/6 - 924.9^2/30 = 4314.21$. The replicate sum of squares is $(169.3^2 + \cdots + 136.0^2)/5 - 924.9^2/30 = 466.44$. The error sum of squares is obtained by subtracting the treatment and replicate sums of squares from the total sum of squares; thus, $7160.36 - 4314.21 - 466.44 = 2379.71$. The error sum of squares may also be obtained as the sum of squares of the deviations of observed values, X_{ij}, from expected values, $\bar{x}_{i.} + \bar{x}_{.j} - \bar{x} = \hat{X}_{ij}$, i.e., the error sum of squares equals $\sum_{i=1}^{v} \sum_{j=1}^{r} (X_{ij} - \hat{X}_{ij})^2$.

Snedecor's F is used to test the hypothesis of no difference among the treatment means; $F = 1078.55/118.986 = 9.06 > F_{.01}(4,20df) = 4.43$. Therefore, we would reject the hypothesis of no difference among the five treatment means. If it is not possible to partition the degrees of freedom for treatments, we should have applied one

TABLE V-1. Yields of soybeans in grams per pot

Treatments	Blocks I	II	III	IV	V	VI	Totals	Means
1 = LLL	8.8	12.9	11.7	31.2	22.0	9.9	96.5	16.1
2 = LLH	23.5	26.3	21.6	15.6	24.4	23.3	134.7	22.4
3 = HLL	41.2	22.5	21.8	46.3	15.6	22.6	170.0	28.3
4 = LHH	28.4	48.4	16.4	44.5	38.8	43.6	220.1	36.7
5 = HHL	67.4	33.2	59.5	49.8	57.1	36.6	303.6	50.6
Total	169.3	143.3	131.0	187.4	157.9	136.0	924.9	30.83

TABLE V-2. Analysis of variance

Source of variation	df		ss		ms	
Block	5		466.44		93.29	
Treatment	4		4314.21		1078.55	
Linear component		1		3492.29		3492.29
Residual		3		821.92		273.97
Error	20		2379.71		118.986	
Total	29		7160.36		-	
Correction for mean	1		28514.67		-	
Total uncorrected	30		35675.03		-	

of the tests in Chapter II. For example, Duncan's multiple comparisons test indicates that mean number 5, *HHL*, is significantly higher than any of the other means. Also, the mean for treatment number 4, *LHH*, is larger than $\bar{x}_1$, treatment *LLL*. The means for treatments 2, 3, and 4 and for treatments 1, 2, and 3 form sets declared not to be heterogeneous. It should be noted that means $\bar{x}_4$, $\bar{x}_3$, and $\bar{x}_2$ yield a sum of squares and range almost large enough to exceed the required value at the 5 per cent level.

It may be of interest to partition the treatment sum of squares into individual components, each based upon a single degree of freedom. The particular set of components used depends entirely upon the objectives of the experiment and upon the nature of the treatments. In this particular experiment, it *may* be of interest to determine the relationship between amount of nitrogen applied and the yield obtained. If L equals 20 units and $H = 100$ units, then treatment *LLL* receives 60 units, *LLH* and *HLL* each receive 140 units, and *LHH* and *HHL* each receive 220 units of nitrogen. The linear component of the treatment sum of squares is

$$\frac{[-6(96.5) - 134.7 - 170.0 + 4(220.1) + 4(303.6)]^2}{6[(-6)^2 + (-1)^2 + (-1)^2 + 4^2 + 4^2]}$$

$(1211.1)^2/420 = 3492.29$, which represents a sizeable portion of the treatment sum

of squares. The coefficients $c(N_i - \bar{n})$, of the treatment totals, $X_{i\cdot}$, are the deviations of the amounts of nitrogen, N_i, per treatment from the mean nitrogen application where $(60 + 140 + 140 + 220 + 220)/5 = 156 = \bar{n}$; the deviations are coded by dividing by 16. Since the sum of the cross products is squared, and since the code number is squared in the denominator, no decoding is necessary; thus:

$$\frac{[\sum c(N_i - \bar{n})X_{i\cdot}]^2}{\sum c^2(N_i - n)^2} = \frac{[\sum (N_i - \bar{n})X_{i\cdot}]^2}{\sum (N_i - \bar{n})^2}, \tag{V-3}$$

where c is the constant used to code each N_i.

Furthermore, the sum of squares for the other components, quadratic, cubic, and quartic, could be computed if so desired [273]. Since only three levels of nitrogen, 60, 140, and 220, were used, it was deemed inadvisable to compute any of the curvilinear regressions for the particular range of levels explored. The sum of squares attributable to linear regression represents the largest portion of the total treatment sum of squares (table V-2). The test of the hypothesis of zero relationship between nitrogen and yield is $F = 3492.29/118.986 = 29.4 > F_{01}(1,20df) = 8.10$. The mean square for residuals = the deviations from linear regression is not significantly larger than the error mean square, since $F = 273.97/118.986 = 2.30 < F_{10}(3,20df) = 2.38$ (table II-8).

For the mean difference, $\dfrac{-6(96.5) - 134.7 - 170.0 + 4(220.1) + 4(303.6)}{6[(-6)^2 + (-1)^2 + (-1)^2 + 4^2 + 4^2]}$ $= \dfrac{1211.1}{420} = 2.8836$, the standard error is obtained from the formula,

$$s_{\bar{d}} = \frac{s}{\sqrt{r\sum k_i^2}}, \tag{V-4}$$

where the k_i are the coefficients of the treatment totals, s^2 = error mean square, and r = number of replicates. The standard error of the mean difference 2.8836 is

$$\sqrt{\frac{118.986}{6}\left\{\frac{1}{(-6)^2 + (-1)^2 + (-1)^2 + 4^2 + 4^2}\right\}} = 0.53226.$$

Therefore, $t = 2.8836/.53226 = 5.418$, and $t^2 = F = 29.4$. The number of significant figures carried is larger than warranted by the accuracy of the data but is required to obtain agreement between t^2 and F.

The standard error of the mean difference between two treatments is $s_{\bar{d}} = \sqrt{2(118.986)/6} = 6.30$. The *lsd* $= s_{\bar{d}}t_{05}(20df) = 13.14$. The coefficient of variation is $s/\bar{x} = 30\sqrt{118.986}/924.9 = 10.91/30.83 = 35$ per cent, which appears to be rather large. Since the replicate mean square is less than the error mean square, the present design is less efficient than a completely randomized design would have been. If it is desired to compute the efficiency of the randomized complete block design relative to the completely randomized design, formula (V-2) is used. For the data of table V-2, $E' = \dfrac{466.44 + 24(118.986)}{29} = 114.555$. Therefore, the relative efficiency is estimated to be

$$\frac{(30 - 6 - 5 + 2)(30 - 5 + 3)(114.555)}{(30 - 6 - 5 + 4)(30 - 5 + 1)(118.986)} \times 100 = 95 \text{ per cent.}$$

V-1.5 ANALYSIS FOR MORE THAN ONE OBSERVATION PER EXPERIMENTAL UNIT

For k observations, readings, or determinations per experimental unit the following analysis of variance table is appropriate:

Source of variation	df	ss
Replicates	$r-1$	$\sum_{j=1}^{r} X_{.j.}^2/vk - X_{...}^2/rvk$
Treatments	$v-1$	$\sum_{i=1}^{v} X_{i..}^2/rk - X_{...}^2/rvk$
Experimental error	$(r-1)(v-1)$	$\sum_i\sum_j \frac{X_{ij.}^2}{k} - \sum_i \frac{X_{i..}^2}{rk} - \sum_j \frac{X_{.j.}^2}{vk} + \frac{X_{...}^2}{rvk}$
Sampling error	$rv(k-1)$	$\sum_i\sum_j\left(\sum_{h=1}^{k} X_{ijh}^2 - X_{ij.}^2/k\right)$
Total	$rvk-1$	$\sum_i\sum_j\sum_h X_{ijh}^2 - X_{...}^2/rvk$

where the $X_{i..}$, $X_{.j.}$, $X_{...}$, and $X_{ij.}$ equal totals for the treatments, replicates, the grand total, and the experimental unit, respectively. The detailed computations are presented in example V-2.

Example V-2. The data presented in table V-3 represent the grams of rubber obtained from two randomly selected plants in a plot for each of the seven varieties of guayule planted in the five replicates. The allocation of the varieties to the seven plots in each replicate was random. The plot size was twenty-eight plants long by twelve rows wide, with 20 inches between plants within a row and 24 inches between rows, resulting in a plot of $(1/12)[(28 \times 20) \times (12 \times 24)] = 46\frac{2}{3}' \times 24'$. The replicate size was $7 \times 24'$ by $46\frac{2}{3}' = 168' \times 46\frac{2}{3}'$. The shape of the replicate might not have been the most desirable. Plots six rows wide by fifty-six plants long might have resulted in a better replicate shape in this experiment.

The sums, means, and sums of squares for the data in table V-3 are presented in table V-4. The results are summarized in table V-5. The mean squares are obtained by dividing the sums of squares by the appropriate degrees of freedom.

In table V-5, two errors are listed, experimental and sampling. The experimenter may often be in a quandary as to which one to use [126, sec. 65; 47, p. 28-35]. The answer depends upon the hypothesis to be tested and upon the assumptions made about the data. If the worker wishes to confine his remarks to the particular five replicates used above, the sampling error is used for testing the variation among variety means. If, on the other hand, the experimenter wishes to make an inference about the true differences among the seven varieties from the *random sample* of five replicates, the experimental error is used. The last cited instance is the one of practical importance in most cases.

The sampling error is larger than the experimental error, but not significantly so. If the variation of plot means from plot to plot after removing replicate and variety

TABLE V-3. Field arrangement of 7 varieties of guayule in 5 randomized complete blocks and weight (grams) of rubber for 2 randomly selected plants

[a] 7 - 130 4.06, 3.75	6 - 406 6.65, 6.17	5 - 593 6.85, 4.94	4 - 109 1.46, 6.39	3 - 416 2.96, 2.71	2 - 405 2.53, 6.93	1 - 407 2.06, 6.12
8 - 109 4.07, 7.73	9 - 593 5.92, 5.00	10 - 405 1.85, 6.44	11 - 406 4.06, 6.65	12 - 416 4.35, 5.85	13 - 130 9.27, 6.64	14 - 407 5.00, 5.12
21 - 593 3.88, 6.22	20 - 407 2.59, 4.79	19 - 406 7.77, 6.91	18 - 416 2.03, 5.08	17 - 130 6.42, 4.72	16 - 405 5.20, 0.90	15 - 109 6.29, 4.77
22 - 130 4.43, 7.31	23 - 109 6.84, 0.89	24 - 405 6.49, 8.55	25 - 416 5.41, 0.87	26 - 593 6.71, 6.67	27 - 407 6.46, 10.66	28 - 406 6.12, 8.21
35 - 593 5.82, 5.08	34 - 130 6.64, 5.92	33 - 416 0.48, 1.97	32 - 405 7.30, 4.19	31 - 406 8.11, 5.95	30 - 109 7.35, 5.33	29 - 407 7.66, 5.00

5 x 46 2/3 = 233 1/3 ft.

24 x 7 = 168 ft.

[a]First no. = plot no.; second no. = varietal designation, and last two numbers = weight of rubber in grams from the two plants.

TABLE V-4. Totals of plot yields and sums of squares

Variety	Replicate Number I	II	III	IV	V	Total	Mean
109	7.85	11.80	11.06	7.73	12.68	51.12	5.112
130	7.81	15.91	11.14	11.74	12.56	59.16	5.916
405	9.46	8.29	6.10	15.04	11.49	50.38	5.038
406	12.82	10.71	14.68	14.33	14.06	66.60	6.660
407	8.18	10.12	7.38	17.12	12.66	55.46	5.546
416	5.67	10.20	7.11	6.28	2.45	31.71	3.171
593	11.79	10.92	10.10	13.38	10.90	57.09	5.709
Total	63.58	77.95	67.57	85.62	76.80	371.52	5.307

Total sum of squares with 69 df:

$$2.06^2 + 6.12^2 + 2.53^2 + \cdots + 5.82^2 + 5.08^2 - \frac{(371.52)^2}{70}$$

$$= 2287.489 - 1971.816 = 315.673.$$

Sum of squares for replicates with 4 df:

$$\frac{63.58^2 + \cdots + 76.80^2}{14} - \frac{(371.52)^2}{70} = 1993.811 - 1971.816 = 21.995.$$

Sum of squares for varieties with 6 df:

$$\frac{51.12^2 + \cdots + 57.09^2}{10} - \frac{(371.52)^2}{70} = 2042.747 - 1971.816 = 70.931.$$

Sum of squares of plot totals with 34 df:

$$\frac{7.85^2 + 11.80^2 + \cdots + 10.90^2}{2} - \frac{(371\ 52)^2}{70} = 2148.102 - 1971.816 = 176.286.$$

Sum of squares for interaction of replicates and varieties (by subtraction):

$$176.286 - 70.931 - 21.995 = 83.360 \text{ with 24 df.}$$

Within plot sum of squares:

$$\frac{(6.12 - 2.06)^2}{2} + \cdots + \frac{(5.82 - 5.08)^2}{2}$$

$$= 6.12^2 + 2.06^2 - \frac{(6.12 + 2.06)^2}{2} + \cdots + 5.82^2 + 5.08^2 - \frac{(10.90)^2}{2}$$

$$= 2287.489 - 2148.102 = 139.387 \text{ with 35 df.}$$

TABLE V-5. Analysis of variance for the data of table V-3

Source of variation	df	ss	ms	F
Replicate	4	21.995	5.4988	
Variety	6	70.931	11.8218	3.40
109 vs others	1	0.4456		-
130 + 406 vs 593	1	2.2349		-
130 vs 406	1	2.7677		-
130, 406, 593 vs 405, 407, 416	1	34.2015		9.85
Among 405, 407, 416	2	31.2813	15.6406	4.50
Experimental error	24	83.360	3.4733	-
Sampling error (between plants)	35	139.387	3.9825	-
Total	69	315.673	-	-

effect is zero in the population, it would be expected that the experimental error would be smaller in about 50 per cent and larger in 50 per cent of the samples. If the latter error is significantly smaller than the sampling error, it would be concluded that a significant negative intraclass correlation [127, 273] exists. The explanation would depend upon the particular type of biological material involved.

Even though the experimental error is the smaller of the two variances, it is the best estimate of the error term for testing the significance of the difference among treatment means. The experimenter may wish to be more "conservative" and to use the sampling error and the degrees of freedom associated with the experimental error. Other schemes could be followed, but the most logical one is to use the experimental error as the estimate of error variation in making various tests of hypotheses, since this is not a result-guided procedure.

The F test of the differences among the seven treatment means is

$$F = \frac{11.8218}{3.4733} = 3.40.$$

For 6 and 24 degrees of freedom the F values at the 2.5 and 1 per cent points are 2.99 and 3.67, respectively.

The next question of importance would be to determine which, if any, of the seven *presumably unrelated* varieties are significantly different with respect to yield of rubber at the end of one growing season. To make these comparisons, several tests are suggested in Chapter II. Tukey's test (sec. II-1.3) indicates that variety 416 is significantly lower than the others in yield of rubber and that the variation in yield among the remaining six varieties might logically be ascribed to chance.

The methods in Chapter II are applicable to a group of *unrelated* varieties or treatments. In this case, considerable information concerning the relationships of the seven varieties was available from past experiments. Variety 109 is the only 54 ± chromosome variety in the group; the remaining are in the 72 ± category. A logical comparison is the mean of the 72's versus the mean of the 54-chromosome variety:

$$\frac{[6(51.12) - 59.16 - 50.38 - \cdots - 57.09]^2}{10(36 + 1 + 1 + 1 + 1 + 1 + 1)}$$

$$= \frac{51.12^2}{10} + \frac{(59.16 + \cdots + 57.09)^2}{60} - \frac{371.52^2}{70} = 0.4456.$$

Also, it is known that varieties 406 and 130 are selections from 593. The 2 degrees of freedom among these three means could logically be partitioned into two single degrees of freedom representing the comparison of the two selections with the parent variety and the comparison between the selections;

Among 130, 406, and 593:

$$\frac{59.16^2 + 66.60^2 + 57.09^2}{10} - \frac{182.85^2}{30} = 5.0026.$$

130 + 406 vs 593:

$$\frac{[59.16 + 66.60 - 2(57.09)]^2}{10(1 + 1 + 4)} = \frac{57.09^2}{10} + \frac{(59.16 + 66.60)^2}{20}$$

$$- \frac{(59.16 + 66.60 + 57.09)^2}{30} = 2.2349.$$

130 vs 406:

$$\frac{(59.16 - 66.60)^2}{10(1+1)} = 2.7677.$$

Furthermore, varieties 130, 406, and 593 are phenotypically different from the remaining three varieties, 405, 407, and 416. The former have round greenish leaves and short branching habit, while the latter group have long serrated grayish-green leaves and longer branches. A logical comparison would be between the means of the two groups,

$$\frac{(59.16 + 66.60 + 57.09 - 50.38 - 55.46 - 31.71)^2}{10(1+1+1+1+1+1)} = 34.2015.$$

The remaining 2 degrees of freedom make up the comparisons among the three varieties 405, 407, and 416, with the following sum of squares:

$$\frac{50.38^2 + 55.46^2 + 31.71^2}{10} - \frac{137.55^2}{30} = 31.2813.$$

It was not known what relationship existed among the three varieties, and without this information the partitioning of the variety sum of squares is finished. Tukey's test (section II-1.3) may be applied to these three means, resulting in two subgroups, 405 and 407 in one group and 416 in the other. The sums of squares are summarized in table V-5, and as a partial check they should add up to the total 70.931.

$F = 9.85$ exceeds the tabulated F at the one per cent point, and $F = 4.50$ exceeds the F value at the 5 per cent point. The means of the three varieties, 130, 406, and 593, and of the three varieties, 405, 407, and 416, cannot be considered as coming from the same general population. Upon examination of the latter three varieties, it is found that they do not represent a homogeneous group and that the very low yield of variety 416 accounts for the large F values in both instances.

The amount of variability relative to the mean in this experiment was much higher than desired. The coefficient of variation is

$$\frac{\sqrt{3.4733/2}}{5.307} = \frac{1.318}{5.307} = 25 \text{ per cent.}$$

The standard deviation *per plant mean yield* is $\sqrt{3.4733/2}$, which equals the standard deviation resulting from an analysis of the plot means. The verification that division of the error mean square by two (equals number of items from each plot) results in the same value as that obtained from using the *plot means* in the analysis is left as an exercise for the student.

The efficiency of this design relative to a completely randomized design is estimated to be

$$Eff = \frac{\dfrac{21.995 + 3.4733(6+24)}{4+6+24}}{3.4733} = \frac{3.7116}{3.4733} = 107 \text{ per cent,}$$

or a gain in efficiency of 7 per cent.

V-1.6 UNEQUAL NUMBERS OF OBSERVATIONS PER EXPERIMENTAL UNIT AND UNEQUAL REPLICATION PER TREATMENT

Missing experimental units or unequal numbers of units within the experimental unit often create analytical difficulties. Regardless of precautions

taken, disproportionate results are occasionally inevitable. The experimenter may start with an equal number of replicates or with experimental units of equal size, but some of the animals may become sick and die; the technician may accidentally omit or mix up some of the results; a part or all of a field plot may not germinate or may be cultivated out; or any of several other things may happen to a part or all of the experimental unit. When faced with such results, the experimenter would still like to obtain all the information possible from the remaining observations. In response to this need, statisticians have developed several analytical procedures for handling disproportionate results from experiments [1, 17, 18, 33, 111, 139, 164, 189, 240, 276, 316, 318a].

Before discussing the various methods and situations, it should be pointed out that the method of calculation does not contribute any more information than is present in the data themselves. In other words, when a "missing plot" value is computed, no *new* datum is added. The procedure is merely a calculational dodge for circumventing a more complex procedure.

Also, some computational procedures are exact, while others are approximate. The investigator must know the assumptions underlying a particular procedure [54, 100, 175, 276, 290] before he is assured of the correctness of the procedure.

V-1.6.1 Missing experimental units. Allan and Wishart [1] were the first to present a formula for computing the value for one missing or extremely divergent value for a randomized complete block experiment. Yates [316] showed that their [1] formula resulted in minimizing the error sum of squares. He presented an iterative procedure for calculating the values for several missing experimental units. The validity of the analysis of variance procedure was investigated, and it was shown that there is little disturbance provided that the proportion of missing values is not large and that the number of degrees of freedom for the ordinary randomized block experiment is reduced by the number of missing plot values computed. The expectation of the treatment mean square is too large in that the coefficient for the error variance component for the error variation is larger than one [111, 316]. This means that the F ratio is too large relative to the correct ratio and too many significant results are obtained. It has been shown that the correct mean squares may be obtained without too much difficulty [111, 316].[1]

The value of a missing experimental unit from the ith treatment in the jth replicate is computed from the formula,

$$\hat{X}_{ij} = \frac{vX_{i.} + rX_{.j} - X_{..}}{(v-1)(r-1)}, \tag{V-5}$$

where the $X_{i.}$ equals the total of treatment i in the $(r-1)$ replicates in which it is present, $X_{.j}$ equals the total of the $(v-1)$ treatments in the jth replicate, and $X_{..}$ equals the total of the $rv - 1$ observations. The applica-

[1]The treatment mean with the missing plot is equal to $\bar{X}_i = (X_{i.} + \hat{X}_{ij})/r$.

tion of the above formula is illustrated in several places in the literature [e.g., 1, 60, 273, 316].

Formulae have been developed for various numbers and arrangements of missing plot values [14, 17, 18, 111, 189] and are useful in special cases. Use of these special formulae often results in a considerable saving of computational labor, but if only one procedure is to be followed, the iterative procedure proposed by Yates [316] is recommended. If more than one value is missing, we guess-estimate the values for all but one of the missing units; this one is computed from formula (V-5). Then, we use formula (V-5) to compute the value for one of the other missing values. This procedure is continued until the computed values become stabilized for each of the missing units. Usually, three cycles will suffice, but the number of cycles depends upon the closeness of the guess-estimates to the computed values. The procedure is illustrated by Yates [316] and Snedecor [273].

V-1.6.2 Disproportionate numbers per experimental unit. Snedecor and Cox [276] list the references on analyses for disproportionate numbers per experimental unit. The computational analysis is illustrated by them [276] and by Snedecor [273]. The particular type of analysis used will depend upon the assumptions about the data and the amount of disproportionateness. If the interaction or the experimental error variance component is negligible or nonexistent, the "method of fitting constants" analysis [273, 276, 318a] is appropriate. If the experimental error or the interaction mean square is considered to be different from the mean square for individuals within the experimental unit and if there are no missing subclasses, the "weighted squares of means" analysis is appropriate [273, 276, 318a]. The expectations of the mean squares have been obtained by Federer [111] and Henderson [155].

Two approximate procedures are available [79, 276]. In the first procedure the means per individual or unit are obtained for each experimental unit, and the analysis of variance is performed on the means. The second procedure involves obtaining expected subclass numbers and completing the analysis utilizing the expected values. Crump [79] has obtained the expectations of the mean squares for these two approximate methods.

Another approximate procedure is to observe the experimental unit with the smallest number of units, say k_1. Then discard at random units from all other experimental units until k_1 units remain. Equal numbers are obtained, and the analysis proceeds as illustrated in section V-1.5. This procedure is not efficient because all material has not been utilized in the analysis, but it may be used for preliminary analyses or for cases in which the numbers of individuals per experimental unit are nearly equal or for cases in which the numbers per experimental unit are large.

V-1.6.3 Other situations. It may happen that the yields of two or more experimental units are available *in toto* but not individually. The ex-

perimenter may unwittingly bulk the yields from two or more treatments before weighing, but is able to obtain the total weight of the bulked items. Bose and Mahalanobis [33] provide formulae for computing the experimental unit yields making use of the combined weight of the mixed-up experimental units. An application is made in their paper.

In other situations the experimenter may be short of material for one or more treatments but has an excess of material for other treatments. The experiment may be designed with "missing plots" for some of the treatments [164], and the "missing plots" filled in with the excess material of the other treatments. The missing experimental units are designed into the experiment at random. One analytical procedure is to run an analysis on only the replicates for which all treatments are present, and a second analysis on only the treatments which are present in all replicates. The procedure does not make use of the substituted plots. Pearce [240] presents a procedure for utilizing all experimental units and illustrates the method with an example. In the same paper, Pearce [240] presents a procedure for analyzing an experiment in which one treatment appears twice in one replicate but does not appear in the second replicate, with the reverse situation being true for a second treatment. An interchanging of the treatments in this manner occasionally occurs in laboratory work. The analytical procedure is not difficult and is recommended for experiments in which the treatments have been interchanged.

In certain experiments a treatment is applied in sequence to an experimental unit. The experimenter may inadvertently apply the wrong treatment at some stage in the sequence. Grundy [139] developed the statistical analysis appropriate for analyzing data from an experiment of this sort.

V-2 Least Squares Estimates and Expectation of Mean Squares

V-2.1 ONE UNIT PER EXPERIMENTAL UNIT

If a single observation is made on each experimental unit of a randomized complete block design and if the effects are additive, the linear model,

$$X_{ij} = \mu + \tau_i + \rho_j + \epsilon_{ij}, \tag{V-6}$$

may be assumed to represent the yield of any plot, where μ represents the population mean value, τ_i = effect common to ith treatment, ρ_j = effect common to jth replicate, and ϵ_{ij} is a random error component. $i = 1, 2, \cdots, v$; $j = 1, 2, \cdots, n$. The above linear equation may be put in the form of a multiple regression equation if written in the form,

$$X_{ij} = \mu + \tau_i X_{1i}' + \rho_j X_{2j}' + \epsilon_{ij}, \tag{V-7}$$

where X_1' and X_2' have the values of zero or 1. X_{1i}' takes on the value 1 in all cells of the two-way classification where τ_i is present and zero elsewhere; likewise, X_{2j}' has the value 1 in replicate j and zero elsewhere.

The least squares estimates of $\mu, \tau_1, \tau_2, \cdots, \tau_v, \rho_1, \rho_2, \cdots, \rho_n$ (n = number of replicates) are obtained as before; i.e., by differentiating the residual sum of squares with respect to the variables and equating the results to zero. The sum of squares to be minimized is

$$\sum_{i=1}^{v}\sum_{j=1}^{n}(X_{ij} - \hat{\mu} - t_i - r_j)^2 = R. \tag{V-8}$$

Partial differentiation of equation (V-8) with respect to $\hat{\mu}$, t_i, and r_j results in the following system of equations:

$$\frac{\partial R}{\partial \hat{\mu}} = -2\sum\sum(X_{ij} - \hat{\mu} - t_i - r_j) = 0; \tag{V-9}$$

$$\frac{\partial R}{\partial t_i} = -2\sum_j(X_{ij} - \hat{\mu} - t_i - r_j) = 0; \tag{V-10}$$

$$\frac{\partial R}{\partial r_j} = -2\sum_i(X_{ij} - \hat{\mu} - t_i - r_j) = 0. \tag{V-11}$$

$\hat{\mu}$, t_i, and r_j, the least squares solutions of the equations, make the residual sum of squares a minimum. The set of differential equations leads to the following normal equations:

Equation for $\hat{\mu}$:

$$\sum\sum X_{ij} = X_{..} = n\sum_i t_i + v\sum_j r_j + nv\hat{\mu}. \tag{V-12}$$

Equations for treatment effects $t_1, \cdots, t_v$:

$$\sum_j X_{1j} = X_{1.} = nt_1 + \sum_j r_j + n\hat{\mu}, \tag{V-13}$$

$$\sum_j X_{2j} = X_{2.} = nt_2 + \sum_j r_j + n\hat{\mu}, \tag{V-14}$$

$$\vdots$$

$$\sum_j X_{vj} = X_{v.} = nt_v + \sum_j r_j + n\hat{\mu}. \tag{V-15}$$

Equations for replicate effects, $r_1, r_2, \cdots, r_n$:

$$X_{.1} = \sum_i t_i + vr_1 + v\hat{\mu}, \tag{V-16}$$

$$X_{.2} = \sum_i t_i + vr_2 + v\hat{\mu}, \tag{V-17}$$

$$\vdots$$

$$X_{.n} = \sum_i t_i + vr_n + v\hat{\mu}. \tag{V-18}$$

In order to obtain unique solutions for the $v + n + 1$ partial regression coefficients, the following restrictions are imposed:

$$\sum_i t_i = 0 = \sum_j r_j. \tag{V-19}$$

Since $E\tau_i = 0$ and $E\rho_j = 0$ in the population, this is a reasonable restriction. Now, the least squares estimate of the experimental mean is

$$\hat{\mu} = \frac{1}{nv}(-n\sum t_i - v\sum r_j + X_{..}) = \frac{X_{..}}{nv} = \bar{x}, \tag{V-20}$$

of t_i is

$$t_i = \frac{X_{i.}}{n} - \bar{x} = \bar{x}_{i.} - \bar{x}, \tag{V-21}$$

and of r_j is

$$r_j = \frac{X_{.j}}{v} - \bar{x} = \bar{x}_{.j} - \bar{x}. \tag{V-22}$$

The variance of $\hat{\mu}$ is

$$E[\hat{\mu} - \mu]^2 = E[\hat{\mu}^2] - \mu^2 = E\left[\frac{X_{..}}{nv}\right]^2 - \mu^2$$

$$= E\left[\frac{nv\mu + n\sum\tau_i + v\sum\rho_j + \sum\sum\epsilon_{ij}}{nv}\right]^2 - \mu^2$$

$$= \mu^2 + \frac{\sigma_\epsilon^2}{nv} - \mu^2 = \frac{\sigma_\epsilon^2}{nv} \tag{V-23}$$

if it is assumed that the $\sum\tau_i = \sum\rho_j = 0$.

The variance of any t_i is

$$E(t_i - \tau_i)^2 = E\left[\frac{X_{i.}}{n} - \hat{\mu}\right]^2 - E(\tau_i^2)$$

$$= E\left[\frac{n\mu + n\tau_i + \sum\rho_j + \sum_j\epsilon_{ij}}{n} - \frac{nv\mu + n\sum\tau_i + v\sum\rho_j + \sum\sum\epsilon_{ij}}{nv}\right]^2 - E(\tau_i^2)$$

$$= E(\tau_i)^2 + \frac{\sigma_\epsilon^2}{n} - \frac{\sigma_\epsilon^2}{nv} - E(\tau_i^2) = \frac{(v-1)}{nv}\sigma_\epsilon^2. \tag{V-24}$$

In a like manner the variance of any r_j is $\dfrac{\sigma_\epsilon^2(n-1)}{nv}$, the covariance of any $t_it_{i'}, (i \neq i')$ or of any $r_jr_{j'}, (j \neq j')$ is $-\sigma_\epsilon^2/nv$, and the covariance of t_ir_j, $t_i\hat{\mu}$, or of $r_j\hat{\mu}$ is equal to zero.

Instead of the variances or covariances of the least squares estimates the expectation, or average value, of a sum of squares after fitting certain constants may be desired. For this case, it is assumed that the τ_i, ρ_j, and ϵ_{ij} are random variates from populations with mean zero and variances equal to $E(\tau_i^2) = \sigma_\tau^2$, $E(\rho_j^2) = \sigma_\rho^2$ and $E(\epsilon_{ij}^2) = \sigma_\epsilon^2$, respectively [79, 111, 155, 169, 175, 290]. In obtaining the expectation or average value of various mean squares, the restriction that $\sum\tau_i = \sum\rho_j = 0$ is not imposed, but the $\sum t_i = 0 = \sum r_j$ still holds.

The expectation of the total sum of squares after fitting the estimated constants $\hat{\mu}$, t_i, and r_j is

$$E\left(\sum\sum X_{ij}^2 - \hat{\mu}X.. - \sum t_i X_{i.} - \sum r_j X_{.j}\right)$$
$$= E\left(\sum\sum X_{ij}^2 - \frac{\sum X_{i.}^2}{n} - \frac{\sum X_{.j}^2}{v} + \frac{X..^2}{nv}\right)$$
$$= E\left[\sum\sum(\mu + \tau_i + \rho_j + \epsilon_{ij})^2 - \sum_i \frac{(n\mu + n\tau_i + \rho_1 + \cdots + \rho_n + \epsilon_{i1} + \cdots + \epsilon_{in})^2}{n}\right.$$
$$- \sum_j \frac{(v\mu + \tau_1 + \cdots + \tau_v + v\rho_j + \epsilon_{1j} + \cdots + \epsilon_{vj})^2}{v}$$
$$\left. + \frac{[nv\mu + n(\tau_1 + \cdots + \tau_v) + v(\rho_1 + \cdots + \rho_n) + \epsilon_{11} + \cdots + \epsilon_{nv}]^2}{nv}\right]$$
$$= nv\mu^2 + nv\sigma_\tau^2 + nv\sigma_\rho^2 + nv\sigma_\epsilon^2 - nv\mu^2 - nv\sigma_\tau^2 - v\sigma_\rho^2 - v\sigma_\epsilon^2$$
$$- nv\mu^2 - n\sigma_\tau^2 - nv\sigma_\rho^2 - n\sigma_\epsilon^2 + nv\mu^2 + n\sigma_\tau^2 + v\sigma_\rho^2 + \sigma_\epsilon^2$$
$$= (nv - v - n + 1)\sigma_\epsilon^2 = (n - 1)(v - 1)\sigma_\epsilon^2. \tag{V-25}$$

The expectation of the sum of squares due to fitting the t_i only is obtained as the difference of the expectations for the residual sum of squares after fitting the constants $\hat{\mu}'$ and r_j' and for the residual sum of squares after fitting $\hat{\mu}$, t_i, and r_j; thus:[1]

$$E\left[\left(\sum\sum X_{ij}^2 - \hat{\mu}'X.. - \frac{\sum r'_j X_{.j}}{v}\right) - \left(\sum\sum X_{ij}^2 - \hat{\mu}X.. - \frac{\sum r_j X_{.j}}{v} - \frac{\sum t_i X_{i.}}{n}\right)\right]$$
$$= E[\sum t_i X_{i.}] = E\left[\frac{\sum X_{i.}^2}{n} - \frac{X..^2}{nv}\right]$$
$$= nv\mu^2 + nv\sigma_\tau^2 + v\sigma_\rho^2 + v\sigma_\epsilon^2 - nv\mu^2 - n\sigma_\tau^2 - v\sigma_\rho^2 - \sigma_\epsilon^2$$
$$= (v - 1)(\sigma_\epsilon^2 + n\sigma_\tau^2), \tag{V-26}$$

which is the expectation of the treatment sum of squares. Likewise, the expectation of the replicate sum of squares is

$$E\left[\frac{\sum X_{.j}^2}{v} - \frac{X..^2}{nv}\right] = (n - 1)(\sigma_\epsilon^2 + v\sigma_\rho^2) \tag{V-27}$$

and of the total sum of squares is

$$E\left[\sum\sum X_{ij}^2 - \frac{X..^2}{rv}\right] = n(v - 1)\sigma_\tau^2 + v(n - 1)\sigma_\rho^2 + (nv - 1)\sigma_\epsilon^2. \tag{V-28}$$

It is not necessary to assume that the τ_i and (or) the ρ_j are random variates. If the τ_i are assumed to be fixed effects, then $E\tau_i^2 = \tau_i^2 \neq \sigma_\tau^2$, and $\sum_i \tau_i = 0$. The expectation of the treatment sum of squares is $(v - 1)\sigma_\epsilon^2 + n\sum_i \tau_i^2$. The latter may be a more appropriate form of the expectation for the treatment sum of squares in many experiments than is equation (V-26). Like-

[1]See footnote on page 103.

wise, the expectation of the replicate mean square for fixed ρ_j is obtained as $\sigma_e^2 + v\sum_j \rho_j^2/(n-1)$.

V-2.2 *k* UNITS PER EXPERIMENTAL UNIT

If k observations are made on each cell of a two-way classification, the linear model,

$$X_{ijh} = \mu + \tau_i + \rho_j + \tau\rho_{ij} + \epsilon_{ijh} \tag{V-29}$$

may be assumed to represent the yield of any observation, where μ represents the population mean value, τ_i = effect common to the ith treatment, ρ_j = effect common to the jth replicate, $\tau\rho_{ij}$ = an effect common to the ith treatment in the jth replicate, ϵ_{ijh} = a random error associated with the ijhth observation, $i = 1, 2, \cdots, v$; $j = 1, 2, \cdots, n$; and $h = 1, 2, \cdots, k$.

The least squares solutions of μ, τ_i, ρ_j, and $\tau\rho_{ij}$ are obtained as before; thus:

$$\frac{\partial R}{\partial \hat{\mu}} = -2\sum_i\sum_j\sum_h (X_{ijh} - \hat{\mu} - t_i - r_j - rt_{ij}) = 0, \tag{V-30}$$

$$\frac{\partial R}{\partial t_i} = -2\sum_j\sum_h (X_{ijh} - \hat{\mu} - t_i - r_j - rt_{ij}) = 0, \tag{V-31}$$

$$\frac{\partial R}{\partial r_j} = -2\sum_i\sum_h (X_{ijh} - \hat{\mu} - t_i - r_j - rt_{ij}) = 0, \tag{V-32}$$

and

$$\frac{\partial R}{\partial rt_{ij}} = -2\sum_h (X_{ijh} - \hat{\mu} - t_i - r_j - rt_{ij}) = 0, \tag{V-33}$$

where

$$R = \sum_i\sum_j\sum_h (X_{ijh} - \hat{\mu} - t_i - r_j - rt_{ij})^2. \tag{V-34}$$

$\hat{\mu}$, t_i, r_j, and rt_{ij} are the estimates which make equation (V-34) a minimum. The above set of differential equations leads to the following set of normal equations:

Equation for $\hat{\mu}$:

$$X_{\cdots} = nk\sum t_i + vk\sum r_j + k\sum\sum rt_{ij} + nvk\hat{\mu}. \tag{V-35}$$

Equations for t_i:

$$X_{i\cdot\cdot} = nkt_i + k\sum_i (r_j + rt_{ij}) + nk\hat{\mu}. \tag{V-36}$$

Equations for r_j:

$$X_{\cdot j\cdot} = k\sum_i (t_i + rt_{ij}) + vkr_j + vk\hat{\mu}. \tag{V-37}$$

Equations for rt_{ij}:

$$X_{ij\cdot} = k(t_i + r_j + rt_{ij} + \hat{\mu}). \tag{V-38}$$

In order to obtain unique solutions for the $n + v + nv + 1$ partial regression coefficients, the following restrictions are imposed:

$$\sum t_i = 0, \tag{V-39}$$

$$\sum r_j = 0, \tag{V-40}$$

and

$$\sum_i rt_{ij} = 0 = \sum_j rt_{ij}. \tag{V-41}$$

With these restrictions the least squares estimates are

$$\hat{\mu} = \frac{X_{\cdots}}{nvk} = \bar{x}, \tag{V-42}$$

$$t_i = \frac{X_{i\cdot\cdot}}{nk} - \hat{\mu} = \bar{x}_{i\cdot} - \bar{x}, \tag{V-43}$$

$$r_j = \frac{X_{\cdot j\cdot}}{vk} - \hat{\mu} = \bar{x}_{\cdot j} - \bar{x}, \tag{V-44}$$

and

$$rt_{ij} = \frac{X_{ij\cdot}}{k} - t_i - r_j - \hat{\mu} = \bar{x}_{ij} - \bar{x}_{i\cdot} - \bar{x}_{\cdot j} + \bar{x}. \tag{V-45}$$

The variances and covariances of the above least squares estimates may be obtained as before and are left as an exercise for the reader.

If it is assumed that the τ_i, ρ_j, $\tau\rho_{ij}$, and ϵ_{ijh} are random independent variates with zero means and variances σ_τ^2, σ_ρ^2, $\sigma_{\rho\tau}^2$, and σ_ϵ^2, respectively, we may proceed to obtain the expectations of the various mean squares obtained in section V-1.5.

The residual (sampling error) sum of squares obtained after fitting the constants $\hat{\mu}$, t_i, r_j, and rt_{ij} has the expectation:

$$E[\sum\sum\sum X_{ijh}^2 - \hat{\mu}X_{\cdots} - \sum t_i X_{i\cdot\cdot} - \sum r_j X_{\cdot j\cdot} - \sum\sum rt_{ij}X_{ij\cdot}]$$
$$= E\left[\sum_i\sum_j\left(\sum_h X_{ijh}^2 - \frac{X_{ij\cdot}^2}{k}\right)\right]$$
$$= E\sum_i\sum_j\left[\sum_h(\mu+\tau_i+\rho_j+\tau\rho_{ij}+\epsilon_{ijh})^2 - \frac{(k\mu+k\tau_i+k\rho_j+k\rho\tau_{ij}+\epsilon_{ij1}+\cdots+\epsilon_{ijk})^2}{k}\right]$$
$$= nvk(\mu^2 + \sigma_\tau^2 + \sigma_\rho^2 + \sigma_{\tau\rho}^2 + \sigma_\epsilon^2) - nv(k\mu^2 + k\sigma_\tau^2 + k\sigma_\rho^2 + k\sigma_{\tau\rho}^2 + \sigma_\epsilon^2)$$
$$= nv(k-1)\sigma_\epsilon^2. \tag{V-46}$$

In a like manner the expectations of the other sums of squares are

$$E\left[\frac{\sum X_{i\cdot\cdot}^2}{nk} - \frac{X_{\cdots}^2}{nvk}\right] = (v-1)(\sigma_\epsilon^2 + k\sigma_{\rho\tau}^2 + nk\sigma_\tau^2), \tag{V-47}$$

$$E\left[\frac{\sum X_{\cdot j\cdot}^2}{vk} - \frac{X_{\cdots}^2}{nvk}\right] = (n-1)(\sigma_\epsilon^2 + k\sigma_{\rho\tau}^2 + vk\sigma_\rho^2), \tag{V-48}$$

and

$$E\left[\frac{\sum\sum X_{ij}.^2}{k} - \frac{\sum X_{i..}^2}{nk} - \frac{\sum X_{.j.}^2}{vk} + \frac{X_{...}^2}{nvk}\right] = (v-1)(n-1)(\sigma_\epsilon^2 + k\sigma_{\rho\tau}^2). \tag{V-49}$$

If it is assumed that the τ_i, ρ_j, and $\tau\rho_{ij}$ are fixed effects which sum to zero in the experiment, then the sampling error mean square has expectation σ_ϵ^2; the interaction = "experimental error" mean square has expectation $\sigma_\epsilon^2 + k\sum\sum(\rho\tau_{ij})^2/(n-1)(v-1)$; the treatment mean square has expectation $\sigma_\epsilon^2 + nk\sum\tau_i^2/(v-1)$; and the replicate mean square has expectation $\sigma_\epsilon^2 + vk\sum\rho_j^2/(n-1)$. For an experiment in which all effects are assumed to be fixed, the sampling error is the appropriate error mean square for testing the null hypotheses of the effects ρ_j, τ_i, and $\rho\tau_{ij}$. This was explained in example V-2. The nature of the material determines the linear model. The assumptions are made by the experimenter.

The above expectations are difficult to obtain if the number of individuals, k_{ij}, per experimental unit varies. A discussion of the various expectations may be found in the literature [79, 111, 155, 169, 175, 290].

V-3 Development of Formulae for Missing or Deleted Values

If an experimental unit is missing, the yields for an experiment designed as a randomized complete block may be represented as

Treatment	Replicate 1	2	...	n	Totals
1	$\hat{X}_{11}$	X_{12}	...	X_{1n}	$X_{1.} + \hat{X}_{11}$
2	X_{21}	X_{22}	...	X_{2n}	$X_{2.}$
.	.	.	.	.	.
.	.	.	...	.	.
.	.	.	.	.	.
v	X_{v1}	X_{v2}	...	X_{vn}	$X_{v.}$
Totals	$X_{.1} + \hat{X}_{11}$	$X_{.2}$		$X_{.n}$	$X_{..} + \hat{X}_{11}$

where $\hat{X}_{11}$ is the missing value, n = number of replicates, v = number of treatments, X_{ij} = yields, and the various totals are listed in the last row and the last column. There is no loss in generality in placing $\hat{X}_{11}$ in the first row and column. If the analysis of variance on the values in the above table is computed, the error sum of squares is

$$R = \hat{X}_{11}^2 + \sum\sum X_{ij}^2 - \frac{\left[(X_{1.} + \hat{X}_{11})^2 + \sum_{j=2}^{v} X_{i.}^2\right]}{n} - \frac{\left[(X_{.1} + \hat{X}_{11})^2 + \sum_{j=2}^{n} X_{.j}^2\right]}{v} + \frac{(X_{..} + \hat{X}_{11})^2}{nv}. \tag{V-50}$$

$$\frac{\partial R}{\partial \hat{X}_{11}} = 2\hat{X}_{11} - \frac{2(X_{1.} + \hat{X}_{11})}{n} - \frac{2(X_{.1} + \hat{X}_{11})}{v} + \frac{2(X_{..} + \hat{X}_{11})}{nv} = 0. \tag{V-51}$$

Solution of equation (V-51) for $\hat{X}_{11}$ results in the following:

$$\hat{X}_{11} = \frac{vX_{1.} + nX_{.1} - X_{..}}{(n-1)(v-1)}, \tag{V-52}$$

which is equation (V-5).

If more values are to be estimated, say $\hat{X}_{11}$ and $\hat{X}_{21}$, we follow the same procedure and obtain two equations for estimating the missing values,

$$\hat{X}_{11} = \frac{nX_{.1} + (v-1)X_{1.} + X_{2.} - X_{..}}{(v-2)(n-1)} \tag{V-53}$$

and

$$\hat{X}_{21} = \frac{nX_{.1} + (v-1)X_{2.} + X_{1.} - X_{..}}{(v-2)(n-1)}. \tag{V-54}$$

If $\hat{X}_{11}$ and $\hat{X}_{22}$ are missing, the two equations for estimating the missing values are

$$\hat{X}_{11} = \frac{(n-1)(v-1)(nX_{.1} + vX_{1.}) - vX_{2.} - nX_{.2} - (nv - v - n)X_{..}}{(nv - v - n)(nv - v - n + 2)} \tag{V-55}$$

and

$$\hat{X}_{22} = \frac{(n-1)(v-1)(nX_{.2} + vX_{2.}) - vX_{1.} - nX_{.1} - (nv - v - n)X_{..}}{(nv - v - n)(nv - v - n + 2)}. \tag{V-56}$$

The procedure may be continued to obtain the equations for various combinations of missing values. The general formula for missing values becomes too complex for easy manipulation [111]. An experimenter may either use the iterative method suggested by Yates [316], or he could develop formulae for his particular needs by the method outlined above.

CHAPTER VI

The Latin Square Design

VI-1 Applications of the Latin Square Design

VI-1.1 INTRODUCTION

In randomized complete block designs the restriction is imposed that all treatments or varieties must appear together in a block an equal or proportional number of times rather than being allotted at random over the whole experimental area as in the completely randomized design. For the latin square design, two restrictions are imposed; namely, that for an experimental area divided into rows and columns, each treatment must appear once in a row and once in a column. Thus for latin squares, the treatments are grouped into replicates in two ways, once in rows and once in columns. Through the elimination of row and column effects from the within treatment variation the residual or error variance may be considerably reduced. The effect of the removal of the row and column variances on the residual variance is illustrated after the discussion on the construction and design of latin squares.

Latin square designs have a wide variety of applications in experimental work. They are used in industrial, laboratory, field, greenhouse, educational, medical, marketing, and sociological experimentation; from comparing a group of varieties or fertilizer treatments to testing biological assays, from comparing worker differences in the laboratory to comparing weaving processes, and from tasting tea to comparing patients in a hospital. Reference to literature citations of numerical examples of the latin square design indicates the wide variety of experiments for which the latin square design has been used. Many more uses may be found in other literature citations in which the original data are not reproduced.

One has but to consult researchers to determine the popularity of the latin square design. Despite its popularity the latin square design is practical only for five to twelve treatments; if two or more squares are used, it is suitable for fewer treatments. For the 2×2, 3×3, and 4×4 latin squares, there are zero, 2, and 6 degrees of freedom associated with the residual sum of squares, and with such few degrees of freedom in the error term, it is recommended that the latin square be repeated or that another design be used. Since the latin square design requires as many replicates as treatments. the

design is seldom used for more than ten to twelve treatments. With regard to the use of the latin square design and with regard to the high precision (standard error less than 2 per cent of the mean) frequently obtained, Fisher [126, sec. 33] says, "If experimentation were only concerned with the comparison of four to eight treatments or varieties, it (the latin square design) would therefore be not merely the principal but almost the universal design employed."

VI-1.2 ADVANTAGES AND DISADVANTAGES

The advantages of the latin square design over other designs are

(i) With a two-way stratification or grouping the latin square controls more of the variation than the completely randomized design or the randomized complete block design. The two-way elimination of variation often results in a small error mean square.
(ii) The analysis is simple; it is only slightly more complicated than that for the randomized complete block design.
(iii) The analysis remains relatively simple even with missing data [1, 14, 33, 85, 216, 316]. Analytical procedures are available for omitting one or more treatments, rows, or columns [321, 333].

The disadvantages of the latin square design are

(i) The number of treatments is limited to the number of rows and columns, except as noted above [321, 333]. For more than ten treatments the latin square is seldom used.
(ii) For fewer than five treatments the allocation of degrees of freedom for controlling heterogeneity is disproportionately large. Even with repetition of squares a disproportionate number of degrees of freedom is associated with rows and columns for two, three, and four treatments. When corrections are made for degrees of freedom (formula (I-1)) the latin square may not be as efficient as the randomized complete block or completely randomized designs for two, three, and four treatments.

Cochran [45, 47] reported that the efficiency of the latin square design relative to the completely randomized design is 222 per cent, and, relative to the randomized complete block design, is 137 per cent for the experiments grown at Rothamsted and associated centers during the years 1927 to 1934. This means that ten replicates of a completely randomized design or six replicates of a randomized complete block design are roughly equivalent to four or five replicates of a latin square. Similar results were obtained at the University of Saskatchewan by Ma and Harrington [201], who found that the randomized complete block was only 79 per cent as efficient as the latin square; i.e., four replicates of a latin square are approximately equivalent to five replicates of a randomized complete block design.

VI-1.3 CONSTRUCTION AND ARRANGEMENTS

For the discussion on the construction of latin squares, it is advantageous to define or explain some of the terminology used in connection with these designs. Fisher and Yates [129] give the following definitions:

(i) *standard square.* A square is said to be standard if the first row and first column are ordered alphabetically or numerically. There are as many standard squares for a $k \times k$ latin square as there are types which cannot be converted into another square by a reshuffling of rows and columns.
(ii) *conjugate square.* Two standard squares are conjugate if the rows of one are the columns of the other.
(iii) *self-conjugate square.* A square is self-conjugate if its arrangement in rows and columns is the same.
(iv) *adjugate set.* By permuting with each other the three categories, rows, columns, and letters, six sets (not necessarily all different) are formed. The resulting sets are said to be adjugate.
(v) *self-adjugate set.* A set is self-adjugate if a permutation of the three categories, columns, rows, and letters results in the same set.

For the 2 × 2 latin square, there is only the one standard square,

A	B
B	A

The two conjugate squares for the above standard square result in the same arrangement as given above. This means that the 2 × 2 latin square is also self-conjugate, since the letters in a row are in the same order as those in the corresponding column. By interchanging rows with columns, columns with letters, and letters with rows, three latin square arrangements are obtained. The conjugate of each of the above three sets may be obtained resulting in the six adjugate sets. These sets give the single square for the 2 × 2 latin square; hence, the 2 × 2 latin square is self-adjugate.

Likewise, for the 3 × 3 latin square, there is only one standard square,

A	B	C
B	C	A
C	A	B

The square is self-conjugate, since the arrangement of the letters in rows and columns is the same. To illustrate the construction of the adjugate set, interchange the column order with letter order in the square,

11A	12B	13C
21B	22C	23A
31C	32A	33B

111	122	133
212	223	231
313	321	332

to obtain the square,

A	B	C
C	A	B
B	C	A

Likewise, interchange the row order and letter order to obtain the square,

A	C	B
B	A	C
C	B	A

Upon interchanging rows and columns of the above three squares, we obtain a total of six squares, or the adjugate set; these squares are the same as the standard square when reordered by interchanging rows and by interchanging columns. However, the six squares of the adjugate set are not all the same.

There are twelve possible arrangements for the 3 × 3 latin square:

A	B	C
B	C	A
C	A	B

A	C	B
B	A	C
C	B	A

B	C	A
C	A	B
A	B	C

B	A	C
C	B	A
A	C	B

C	B	A
A	C	B
B	A	C

C	A	B
A	B	C
B	C	A

A	B	C
C	A	B
B	C	A

A	C	B
C	B	A
B	A	C

B	C	A
A	B	C
C	A	B

B	A	C
A	C	B
C	B	A

C	B	A
B	A	C
A	C	B

C	A	B
B	C	A
A	B	C

There are $3!\,(3-1)! = 12$ arrangements for the 3 × 3 latin square, of which 11 are nonstandard squares.

Four standard squares are possible for the 4 × 4 latin square:

A	B	C	D
B	A	D	C
C	D	B	A
D	C	A	B

A	B	C	D
B	C	D	A
C	D	A	B
D	A	B	C

A	B	C	D
B	D	A	C
C	A	D	B
D	C	B	A

A	B	C	D
B	A	D	C
C	D	A	B
D	C	B	A

All four standard 4×4 latin squares are self-conjugate. For each standard square, there are $4!\,(4-1)! = 144$ possible arrangements, resulting in a total of 576 possible arrangements; these have been tabulated by Kitagawa and Mitome [187]. Of the 576 arrangements, 572 are nonstandard squares, and the remaining are the four standard squares.

For the 5×5 latin square, there are twenty-five standard squares and their conjugates, plus six self-conjugate squares, resulting in fifty-six standard squares. Also, there are $56(5!)(4!) = 56(2880) = 161{,}280$ possible arrangements [129].

The number of possible arrangements increases rapidly as the size of the latin square increases. It is obvious, then, why the possible arrangements for all $k \times k$ latin squares have not been tabulated. Fisher and Yates [129] give the standard squares for the 4×4 and 5×5 latin squares; they [128, 129] give the five conjugate pairs of transformation sets and the twelve sets containing conjugates for the 6×6 latin squares. Norton [233] has tabulated the 562 sets from which it is possible to generate the 16,927,968 standard squares for the 7×7 latin square.[1] To date, all the standard squares for higher ordered latin squares have not been tabulated.

Further discussion on the formation of latin squares appears in the literature citations [126, 128, 129, 175, 207, 308, 317] at the end of the book. A number of literature citations relating to the construction of latin squares appears in references [113] and [129]. Some of the topics related to orthogonal latin squares [129] and graeco-latin squares [126, 129] are discussed in later chapters.

Fisher and Yates [129] list sample squares for the 7×7, 8×8, 9×9, 10×10, 11×11, and 12×12 latin squares. The experimenter may also make up his own sample squares for the larger squares.

VI-1.4 RANDOMIZATION

In designing an experiment as a latin square, one of the possible arrange-

[1]Sade (*Ann. Math. Stat.* **22**:306, 1951) has found the correct number of 7×7 latin squares to be 16,942,080.

ments is selected at random. The procedure is quite simple for the 2×2 latin square; of the two arrangements,

A	B
B	A

B	A
A	B

one is chosen by the toss of a coin or from a table of random numbers. The letters A and B represent two treatments under consideration. Likewise, for a 3×3 latin square with the treatments A, B, and C, one of the twelve arrangements listed above is chosen at random.

All possible arrangements of the 2×2 and 3×3 latin squares may be found in several references. The remainder have not been enumerated to date. Therefore, another method for selecting a random arrangement must be provided. In accordance with the rule for obtaining latin square arrangements as set forward by Fisher and Yates [129] the following procedure may be utilized for obtaining latin square designs for experiments:

(i) *2 × 2 latin square.* Randomize the arrangement of the columns of the standard square, or, alternatively, select one of the two arrangements at random.

(ii) *3 × 3 latin square.* Randomize the arrangement of the three columns and of the last two rows of the standard square or, alternatively, select one of the twelve arrangements at random.

(iii) *4 × 4 latin square.* Select one of the four standard squares at random and then randomize the arrangement of the columns and the last three rows. Also, the procedure of selecting one of the 576 arrangements at random may be used [187].

(iv) *5 × 5 latin square.* Select one of the fifty-six standard squares at random and then randomize the arrangement of the five columns and the last four rows; this results in the selection of one of the 161,280 arrangements.

(v) *6 × 6 latin square.* Select one of the 9408 standard squares at random and randomize the arrangement of the columns and the last five rows; alternatively, select at random one of the sets enumerated by Fisher and Yates [129] in proportion to the number of standard squares possible in the set and then randomize the allotment of the letters to the treatments, the arrangement of the columns, and the arrangement of the rows.

(vi) *7 × 7 latin square.* Select one of the 16,942,080 standard squares at random and then arrange all columns and the last six rows at random; alternatively, follow the second plan for the 6×6 latin square.

(vii) *8 × 8 and higher latin squares.* Select one of the tabulated squares or construct one and then arrange the columns and the rows at random and assign the letters to the treatments at random or follow the alternative procedure outlined below.

The procedure given in (vii) may be used to construct the 5×5 and larger latin square arrangements. However, it must be remembered that this method

does not result in all possible arrangements, since certain configurations are excluded. Little harm from this procedure is likely to result unless the latin squares are used extensively or unless the results of several experiments are summarized. Yates [317] has discussed the theoretical basis for randomization of latin squares. The student is referred to the above reference for a further discussion of this topic.

An alternative procedure for obtaining a random arrangement may be used for the larger latin squares. It is observed (i) that the method is biased in that all arrangements do not have an equal probability of being drawn and (ii) that this method gives all possible arrangements. An outline of the procedure follows:[1]

(i) Assign letters to first row at random; this results in $k!$ permutations of the letters in the first row.
(ii) Assign the remaining $(k - 1)$ letters at random in the first column resulting in $(k - 1)!$ permutations of these letters.
(iii) Continue the process until all rows and columns are filled, excluding letters which have already appeared in a row or in a column.

As a further suggestion, one might take the resulting square from the above procedure and randomize the rows, columns, and letters. It is not known what effect the second randomization has on the bias. The bias may be illustrated fairly simply with the 4×4 latin square. If the above procedure is followed, it will be observed that the probability of selecting two of the four standard squares is $\frac{1}{3}$ each, and the probability of selecting the other two standard squares is $\frac{1}{6}$ each.

VI-1.5 EXPERIMENTAL LAYOUT

The general concept is that the latin square design should occupy a square or nearly square experimental area. In practice, this is generally true for field experiments, but it is not necessary; usually the rows are perpendicular to the columns; thus:

	Column number		
Row number	1	2	3
1	A	C	B
2	C	B	A
3	B	A	C

[1]Suggested in a discussion with F. Yates and P. J. McCarthy.

Now, the purpose of the latin square design in field and laboratory experiments is to control variation in two directions, such as down the field and across the field, across the greenhouse bench and along the bench, or from two sources. In some instances, it may be desirable to keep the treatments in a row in a compact block in such a way that the blocks are the rows and the order within the blocks represents the columns. Such an experimental design might be illustrated by the following:

Row 1 or Block 1			Row 2 or Block 2			Row 3 or Block 3		
A	C	B	C	B	A	B	A	C

or

	A
Row 1	C
	B
	C
Row 2	B
	A
	B
Row 3	A
	C

The above design might be used on a single row or set of rows in a grape vineyard, where treatment A represents no spray or check, treatment B represents spray 1, and treatment C represents spray 2. The sprayer could be equipped with two tanks and the various treatments applied as indicated in the design.

In some instances, more replication is desired. The procedure here is to select, at random, the desired number of arrangements of the latin square. Suppose that nine replicates for the three treatments A, B, and C are desired. The field design could be of the following form for three rows (or three sets of rows) in a grape vineyard:

A	C	A
C	A	B
B	B	C
C	B	B
B	C	C
A	A	A
B	A	C
A	B	A
C	C	B

or for the three locations, farms, or positions:

Square I

A	C	B
C	B	A
B	A	C

Square II

C	A	B
B	C	A
A	B	C

Square III

A	B	C
B	C	A
C	A	B

Modifications of latin square designs result in other configurations, depending upon the nature of the experiment and the experimental material. The first of the two designs listed above may be useful in baking or cooking experiments which are conducted over a period of days. If it is possible to bake three cakes per day and if the worker tires as the day progresses, it may be desirable to have each kind of cake baked in all three orders of baking. The 3×3 latin square design satisfies these requirements. The experiment may be repeated on a second set of three days, using a different randomization.

VI-1.6 STATISTICAL ANALYSIS FOR ONE OBSERVATION PER EXPERIMENTAL UNIT

The breakdown of the total degrees of freedom in the analysis of a $k \times k$ latin square design is

Source of variation	Degrees of freedom	Mean square
Row	$k - 1$	R
Column	$k - 1$	C
Treatment	$k - 1$	T
Error or residual	$(k - 1)(k - 2)$	E
Total	$k^2 - 1$	

The row sum of squares is obtained by summing the squares of the row totals, $X_{i..}$, dividing by k, and then subtracting the correction term (equal to the grand total, $X_{...}$, squared and divided by k^2);

$$\frac{X_{1..}^2 + X_{2..}^2 + \cdots + X_{k..}^2}{k} - \frac{X_{...}^2}{k^2} = \sum_i \frac{X_{i..}^2}{k} - \frac{X_{...}^2}{k^2}$$

$$= \bar{x}_{1..}X_{1..} + \bar{x}_{2..}X_{2..} + \cdots + \bar{x}_{k..}X_{k..} - \bar{x}X_{...} = k\sum(\bar{x}_{i..} - \bar{x})^2, \quad \text{(VI-1)}$$

where $\bar{x}_{i..}$ = row mean and $\bar{x}$ = experiment mean. In a similar manner the treatment and column sums of squares are obtained as

$$\frac{X_{..1}^2 + \cdots + X_{..k}^2}{k} - \frac{X_{...}^2}{k^2} \quad \text{(VI-2)}$$

and

$$\frac{X_{.1.}^2 + \cdots + X_{.k.}^2}{k} - \frac{X_{...}^2}{k^2}, \quad \text{(VI-3)}$$

respectively, where $X_{..h}$ represents the treatment total and $X_{.j.}$ represents the column total. The total sum of squares with $k^2 - 1$ degrees of freedom is obtained by squaring the k^2 determinations, X_{ijh}, and subtracting the correction term,

$$\sum_{i,j,h} X_{ijh}^2 - \frac{X_{...}^2}{k^2}. \tag{VI-4}$$

$i = 1, 2, \cdots, k$ and the subscript $j = 1, 2, \cdots, k$ to give the k^2 observations. Within each row and column the treatments are arranged to appear once in each row and once in each column. Thus, for $h = 1, 2, \cdots, k$, there are two restrictions imposed. The error or residual sum of squares is obtained by subtracting the row, column, and treatment sums of squares from the total,

$$\sum X_{ijh}^2 - \frac{\sum X_{i..}^2}{k} - \frac{\sum X_{.j.}^2}{k} - \frac{\sum X_{..h}^2}{k} + \frac{2X_{...}^2}{k^2}. \tag{VI-5}$$

The estimated standard error of a difference between two means is obtained from the formula,

$$\sqrt{\frac{2(\text{error mean square})}{k}}. \tag{VI-6}$$

In the event that one of the treatments yields more variable results than the other $k - 1$ treatments, Yates [316, 321] gives a method for determining the error of the more variable treatment and the error of the remaining $k - 1$ treatments which have approximately the same amount of variation. In addition, he describes a procedure for calculating missing values and for making comparisons among the means.

Example VI-1. One of the studies of the Regional Cooperative Project S-5 reported by the Southern Cooperative Group is concerned with the variation in moisture content from plant to plant and from leaf to leaf of turnip greens. Knowledge of the magnitude of these sources of variation is of importance in sampling studies for chemical determinations. In order to study the sources of variation in sampling turnip greens, Peterson *et al.* [247] set up a 5×5 latin square with five different plants as the rows and five different leaf sizes, ranked from smallest to largest and designated as A, B, C, D, and E as the columns. In addition, it was desired to measure the relative variation due to time of sampling. The treatments in this experiment are the five times of sampling. The data on moisture content of deribbed turnip leaves are presented in table VI-1. The moisture contents given in the table are in percentages and are coded values obtained by subtracting 80 from each of the original moisture contents. The sums of squares are obtained from the data and totals in table VI-1. Application of formula (VI-1) results in the following sum of squares for rows or plants:

$$\frac{40.68^2 + \cdots + 32.76^2}{5} - \frac{179.64^2}{25} = 1320.2443 - 1290.8212 = 29.4231.$$

Application of equations (VI-2), (VI-3), and (VI-4) results in the sums of squares for

times, leaf sizes, and the total corrected for the mean. The error or residual sum of squares, equation (VI-5), is

$$1353.5604 - 1320.2443 - 1313.8162 - 1291.3635 + 2(1290.8212)$$
$$= 62.7392 - 29.4231 - 22.9950 - 0.5423 = 9.7788.$$

The above sums of squares and the corresponding mean squares are presented at the bottom of table VI-1.

TABLE VI-1. Moisture content of turnip greens (minus 80)

Plants	Leaf size (A to E, smallest to largest) A	B	C	D	E	Total	Uncoded mean
1	v. 6.67	iv. 7.15	i. 8.29	iii. 8.95	ii. 9.62	40.68	88.14
2	ii. 5.40	v. 4.77	iv. 5.40	i. 7.54	iii. 6.93	30.04	86.01
3	iii. 7.32	ii. 8.53	v. 8.50	iv. 9.99	i. 9.68	44.02	88.80
4	i. 4.92	iii. 5.00	ii. 7.29	v. 7.85	iv. 7.08	32.14	86.43
5	iv. 4.88	i. 6.16	iii. 7.83	ii. 5.38	v. 8.51	32.76	86.55
Total	29.19	31.61	37.31	39.71	41.82	179.64	-
Uncoded mean	85.84	86.32	87.46	87.94	88.36	-	87.19

	Time totals and means (uncoded) i	ii	iii	iv	v
Total	36.59	36.22	36.03	34.50	36.30
Uncoded mean	87.32	87.24	87.21	86.90	87.26

Analysis of variance

Source of variation	df	ss	ms
Plants (row)	4	29.4231	7.3558
Leaf sizes (column)	4	22.9950	5.7488
Times (treatment)	4	0.5423	0.1356
Error	12	9.7788	0.8149
Total	24	62.7392	-
Correction for mean	1	1290.8212	-
Total uncorrected	25	1353.5604	-

An F test indicates that differences exist among plants and among leaf sizes, but that time differences are negligible, thus:

$$F = \frac{7.3558}{.8149} = 9.03 > F_{01}(4 \text{ and } 12df) = 5.41,$$

$$F = \frac{5.7488}{.8149} = 7.05 > F_{01}(4 \text{ and } 12df) = 5.41,$$

and

$$F = \frac{0.1356}{.8149} = 0.17,$$

where the F_α values are obtained from table II-8. It may be of interest to determine if the time means are more alike than expected relative to the error variance; thus $F = .8149/.1356 = 6.0$, which is greater than the tabulated value for F_{05} (12 and 4 *df*) $= 5.91$; this is the 10 instead of the 5 per cent level, since the F test was made after observing which mean square was the smaller. Hence, the mean square for time of sampling is small but probably not unusually small. If it were significantly smaller than the error mean square, then competition, in the general sense, would be suspected, and we should determine the element in our experimental technique which gives either an error mean square that is too large or a times mean square that is too small.

As a further subdivision of the sum of squares, it may be of interest to obtain the linear, quadratic, cubic, and residual components for leaf size. Without additional knowledge on leaf size, we could assign the values -2, -1, 0, 1, and 2, or 1, 2, 3, 4, and 5, and obtain the components by the method described by Snedecor [273, Ch. 14 and 15]. In this particular experiment, such comparisons are unrealistic, since the actual weights are available and the authors [247] used a covariance analysis on moisture contents for leaf size.

The standard error of a time, plant, or leaf-size mean is

$$s_{\bar{x}} = \sqrt{\frac{.8149}{5}} = .404.$$

The standard error of a mean difference is

$$s_{\bar{d}} = \sqrt{\frac{2(.8149)}{5}} = .571.$$

The coefficient of variation is

$$\frac{s}{\bar{x}} = \frac{\sqrt{.8149}}{87.19} = 1 \text{ per cent.}$$

Since the heterogeneity was controlled by the two groupings, plants and leaf sizes, the experimental design was satisfactory. If either plant variation or leaf-size variation is ignored, the residual variance increases considerably. This means that the latin square design is more efficient that either a randomized complete block or a completely randomized design for comparing treatment (time) means. If the plants were the replicates, the efficiency of this latin square relative to a randomized complete block design is

$$\left(\frac{(k-1)(k-2)+1}{(k-1)(k-2)+3}\right)\left(\frac{(k-1)^2+3}{(k-1)^2+1}\right)\frac{C+(k-1)E}{kE}, \tag{VI-7}$$

where the symbols are defined in the analysis of variance table at the beginning of this section; for this example, the efficiency is equal to

$$\frac{(12+1)\,(16+3)}{(12+3)\,(16+1)}\left(\frac{5.7488+4(.8149)}{5(.8149)}\right) = 214 \text{ per cent.}$$

Approximately eleven replicates of a randomized complete block design with plants as replicates would have yielded a standard error of a mean equal to that obtained with five in the latin square. It is probably unrealistic to consider leaf sizes as replicates and to ignore plant differences, but if the columns were the replicates, the efficiency of the latin square relative to the randomized complete block is obtained from the formula,

$$\frac{[(k-1)(k-2)+1][(k-1)^2+3][R+(k-1)E]}{[(k-1)(k-2)+3][(k-1)^2+1][kE]}, \tag{VI-8}$$

which for this latin square is

$$\left(\frac{13}{15}\right)\left(\frac{19}{17}\right)\frac{7.3558+4(.8149)}{5(.8149)} = 252 \text{ per cent.}$$

Approximately thirteen replicates of a randomized block design of this type would have been equal to five replicates of the latin square.

Similarly, the efficiency of the two-way grouping in the latin square relative to no stratification, as in the completely randomized design, is

$$\left(\frac{df_1+1}{df_1+3}\right)\left(\frac{df_2+3}{df_2+1}\right)\frac{(k-1)(R+C)+[k-1+(k-1)(k-2)]E}{(k-1)E}$$
$$=\left(\frac{(k-1)(k-2)+1}{(k-1)(k-2)+3}\right)\left(\frac{k(k-1)+3}{k(k-1)+1}\right)\frac{R+C+(k-1)E}{(k+1)E}. \tag{VI-9}$$

The efficiency of the latin square relative to the completely randomized design is

$$\left(\frac{13}{15}\right)\left(\frac{23}{21}\right)\frac{7.3558+5.7488+4(.8149)}{6(.8149)} = 318 \text{ per cent,}$$

or, approximately sixteen replicates of a completely randomized design would have been equivalent to the latin square with five.

VI-1.7 STATISTICAL ANALYSIS FOR A GROUP OF LATIN SQUARES WITH A SINGLE DETERMINATION PER PLOT

In some cases, it is desirable to have more than a single latin square at a single location or to have a single latin square at several locations. For the 2×2, 3×3, and sometimes the 4×4 latin squares, it is often desirable to have two or more squares at a location in order to have sufficient degrees of freedom in the error sum of squares. The procedure of designing an experiment in more than one latin square has already been discussed. The breakdown of the total degrees of freedom in the analysis of variance follows:

Source of variation	Degrees of freedom	Mean square
Squares (or locations)	$s-1$	S
Rows within squares	$s(k-1)$	R
Columns within squares	$s(k-1)$	C
Treatments	$k-1$	T
Treatments × squares	$(s-1)(k-1)$	TS
Residual within squares	$s(k-1)(k-2)$	E
Total	sk^2-1	

The "treatment × squares" sum of squares may be pooled with "residual within-squares" sum of squares if there is no treatment-square interaction. If the squares are at different locations, then, for some hypotheses, it is appropriate to use the "treatment × square mean square" to test the treatment differences. Fisher [126, sec. 65] discusses the analysis for a group of latin squares and the appropriate error mean square for testing the differences among treatment means for various hypotheses. The analysis for two or more latin squares has been discussed by several authors.

The formulae for obtaining the sums of squares in the above analysis of variance table are given below.

For squares,

$$\sum_{i=1}^{s}\frac{X_{i\cdots}^2}{k^2} - \frac{X_{\cdots\cdot}^2}{sk^2}; \tag{VI-10}$$

for rows within squares,

$$\sum_{i=1}^{s}\left\{\sum_{h=1}^{k}\frac{X_{i\cdot h\cdot}^2}{k} - \frac{X_{i\cdots}^2}{k^2}\right\}; \tag{VI-11}$$

for columns within squares,

$$\sum_{i=1}^{s}\left\{\sum_{g=1}^{k}\frac{X_{i\cdot\cdot g}^2}{k} - \frac{X_{i\cdots}^2}{k^2}\right\}; \tag{VI-12}$$

for treatments,

$$\sum_{j=1}^{k}\frac{X_{\cdot j\cdot\cdot}^2}{sk} - \frac{X_{\cdots\cdot}^2}{sk^2}; \tag{VI-13}$$

for treatments × squares,

$$\sum_{i=1}^{s}\sum_{j=1}^{k}\frac{X_{ij\cdot\cdot}^2}{k} - \sum_{i=1}^{s}\frac{X_{i\cdots}^2}{k^2} - \sum_{j=1}^{k}\frac{X_{\cdot j\cdot\cdot}^2}{sk} + \frac{X_{\cdots\cdot}^2}{sk^2}; \tag{VI-14}$$

for error or residual within squares,

$$\sum_{i=1}^{s}\left\{\sum_{j,g,h}X_{ijgh}^2 - \sum_{j}\frac{X_{ij\cdot\cdot}^2}{k} - \sum_{h}\frac{X_{i\cdot h\cdot}^2}{k} - \sum_{g}\frac{X_{i\cdot\cdot g}^2}{k} + 2\frac{X_{i\cdots}^2}{k^2}\right\}; \tag{VI-15}$$

and for the total sum of squares,

$$\sum_{i}\sum_{j,h,g}X_{ijhg}^2 - \frac{X_{\cdots\cdot}^2}{sk^2}. \tag{VI-16}$$

The subscripts j, h, and g correspond to subscripts h, i, and j for the single latin square, as described previously, and $i = 1, 2, \cdots, s$. The treatments × squares sum of squares in formula (VI-14) may also be obtained by subtracting the treatment sum of squares from the treatment-within-squares sum of squares. An application of the above formulae is illustrated below.

The efficiency of the latin square design relative to what it would have been had a completely randomized design been used is[1]

$$\frac{(s-1)S + s(k-1)(R+C) + s(k-1)^2E'}{(sk^2-1)E'}, \tag{VI-17}$$

[1]The correction for the difference in degrees of freedom associated with the two mean squares is not included.

where E' is the mean square from the pooled sums of squares for error within squares and treatments $\times$ squares. The other symbols are defined above. The efficiency of the latin square design relative to what it would have been had the rows been used as replicates within each square is (for E', as defined in formula (VI-17)[1]

$$\frac{s(k-1)C + s(k-1)^2E'}{sk(k-1)E'}. \tag{VI-18}$$

If C is replaced by R in formula (VI-18), the formula for efficiency is obtained for using the columns as replicates. Also, the efficiency of the latin square relative to what it would have been had a completely randomized design been used in each square is[1]

$$\frac{s(k-1)(R+C) + s(k-1)^2E'}{s(k^2-1)E'}. \tag{VI-19}$$

Example VI-2. Dominick [92] has used sets of latin squares in marketing research. The data, pounds of apples sold per hundred customers, presented in table VI-2, represent a part of one such set. Data for the remainder of the set are given in problem VI-3. Four treatments on McIntosh apples were compared in four stores; treatment A = regular apples, B = apples of 2.25-inch diameter at a lower price, C = carefully handled uniform apples of 2.5-inch diameter, and D = highly colored uniform apples of 2.5-inch diameter. It was suspected that the days of the week as well as the parts of the week might contribute to the variability. The first part of the week is Monday, Tuesday, Wednesday, and Thursday, and the second part of the week is composed of Friday morning, Friday afternoon, Saturday morning, and Saturday afternoon. From previous work, it was found that the two parts of the week contained about the same number of customers.

The randomization scheme followed was a random allocation of the four standard squares to the two parts of each of the two weeks. Then, the randomization scheme of section VI-1.4 was followed for each square.

The analysis of variance is obtained for each square separately, and the results are combined by the method of the preceding section. The separate analyses present no additional work, unless the individual mean squares are obtained, and may indicate the source of large variations. As a general rule, it is wise to study the individual analyses in connection with the combined analysis. The separate analyses and the combined analysis of variance are presented at the bottom of table VI-2.

The sum of squares for weeks (squares), formula (VI-10), is obtained by adding the correction terms from the individual weeks and subtracting the new correction term, thus: $10050.06 + 11130.25 - 823^2/32 = 13.78 = (401 - 422)^2/32$, with $1 + 1 - 1 = 1$ degree of freedom.

The stores-within-weeks, days-within-weeks, and treatments-within-weeks sums of squares are obtained by adding the sums of squares from each latin square, thus: stores-within-weeks $= 848.19 + 408.75 = 1256.94$, days-within-weeks $= 237.69 + 1080.75 = 1318.44$, and treatments-within-weeks $= 707.19 + 1146.75 = 1853.94$,

[1]The correction for the difference in degrees of freedom associated with the two mean squares is not included.

TABLE VI-2. Pounds of McIntosh apples per 100 customers purchased in four stores for four treatments in the first parts of two weeks

	Week 1					Week 2				
	Store number					Store number				
Day of week	1	2	3	4	Total	1	2	3	4	Total
Monday	A 14	B 8	C 40	D 48	110	B 24	D 30	C 24	A 12	90
Tuesday	B 20	A 22	D 48	C 25	115	D 42	A 4	B 12	C 32	90
Wednesday	D 24	C 12	B 12	A 27	75	A 8	C 8	D 36	B 28	80
Thursday	C 31	D 16	A 32	B 22	101	C 28	B 32	A 48	D 54	162
Total	89	58	132	122	401	102	74	120	126	422
Store total	191	132	252	248						
Day totals	Mon.	Tues.	Wed.	Thurs.						
	200	205	155	263						

	Treatments				
	A	B	C	D	
Week 1	95	62	108	136	401
Week 2	72	96	92	162	422
Totals	167	158	200	298	823
Means	20.9	19.8	25.0	37.2	25.7

Analyses of variance for each week

		Week 1		Week 2	
Source of variation	df	ss	ms	ss	ms
Stores (column)	3	848.19	282.73	408.75	136.25
Days (row)	3	237.69	79.23	1080.75	360.25
Treatment	3	707.19	235.73	1146.75	382.25
Error	6	451.87	75.31	613.50	102.25
Total	15	2244.94	-	3249.75	-
Correction for mean	1	10050.06	-	11130.25	-
Total uncorrected	16	12295.00	-	14380.00	-

Combined analysis of variance

Source of variation	df	ss	ms	
Squares (weeks)	1	13.78	13.78	
Days within weeks	6	1318.44	219.74	
Stores within weeks	6	1256.94	209.49	
Treatments within weeks	6	1853.94	-	
Treatment	3	1540.59	513.53	
Treatment x week	3	313.35	104.45	91.91
Error within weeks	12	1065.37	88.78	
Total	31	5508.47	-	
Correction for mean	1	21166.53	-	

each with $3 + 3 = 6$ degrees of freedom. The latter sum of squares is partitioned into the two components, treatment sum of squares, formula (VI-13),

$$\frac{167^2 + 158^2 + 200^2 + 298^2}{2(4)} - \frac{823^2}{2(4)(4)} = 1540.59,$$

with 3 degrees of freedom, and the treatment $\times$ week (square) sum of squares, $1853.94 - 1540.59 = 313.35$, with $6 - 3 = 3$ degrees of freedom. The other two sums of squares may be partitioned in the same manner as treatments-within-weeks for this particular example. If the two latin squares had been conducted during the two parts of one week, it would not be correct to partition the days-within-weeks sum of squares into the components days and days $\times$ weeks. Also, the above statement holds if eight stores instead of four had been used for the two 4×4 latin squares.

The error-within-squares sum of squares is obtained by summing the individual error sums of squares, $451.87 + 613.50 = 1065.37$, with $6 + 6 = 12$ degrees of freedom. The total sum of squares for the two squares may be obtained by formula (VI-16) or by adding the uncorrected total sums of squares from the individual squares and subtracting the overall correction term, $12295 + 14380 - 823^2/32 = 5508.47$, with $16 + 16 - 1 = 31$ degrees of freedom.

The treatment mean square is significantly larger than ordinarily expected in sampling from a homogeneous population; $F = 513.53/88.78 = 5.78$ is larger than $F_{025}(3, 12df) = 4.47$ and almost equal to $F_{01}(3, 12df) = 5.95$ (see table II-8). Therefore, we reject the null hypothesis of no difference among the four treatments on the purchase of apples. If it is desired to compare the other mean squares with the error mean square, the F test may be used. However, for this particular experimental setup the stores-within-weeks and days-within-weeks sums of squares should be partitioned into the component parts prior to making any tests.

The standard error of a treatment mean is $s_{\bar{x}} = \sqrt{E/sk} = \sqrt{88.78/8} = 3.33$. The standard error of a difference of two treatment means is $s_{\bar{d}} = \sqrt{E\left(\frac{1}{sk} + \frac{1}{sk}\right)} = \sqrt{88.78/4} = 4.71$. The coefficient of variation is $\sqrt{E}/\bar{x} = 32\sqrt{88.78}/823 = 37$ per cent. Application of Duncan's multiple comparisons test indicates that treatment D is different from treatments A, B, and C and that A, B, and C do not differ among themselves.

The efficiency of the two latin squares used relative to what would have been obtained from a completely randomized design is equal to (formula (VI-17)),

$$\frac{13.78 + 6(219.74 + 209.49) + 18(91.91)}{31(91.91)} \times 100 = 149 \text{ per cent},$$

where $91.91 = (313.35 + 1065.37)/15$. The use of $E' = 91.91$ assumes a zero treatment $\times$ week interaction. If this is not true, then formulae (VI-17) to (VI-19) should be modified accordingly. Approximately twelve replicates of a completely randomized design would have been required to attain the same precision as the present design with eight replicates. Likewise, if the stores and weeks were the replicates of a randomized complete block design, the efficiency of the latin square is (formula (VI-18)),

$$\frac{6(219.74) + 18(91.91)}{24(91.91)} \times 100 = 135 \text{ per cent.}$$

VI-1.8 ANALYSIS FOR MORE THAN ONE OBSERVATION PER EXPERIMENTAL UNIT

Several variations of latin squares are possible, and some of these will be discussed in later chapters. The example of the present section is a latin square with more than one unit per plot. The particular breakdown of the degrees of freedom in the analysis of variance depends upon the nature of the material and the design of the experiment.

Example VI-3. A 4×4 latin square design [288] was set up to compare the effects of four light intensities (D = dark or zero, $L = 500$, $M = 900$, and $H = 1200$ foot-candles of light) on the difference in bioelectric potential (in millivolts) between a point on the stem of the bean plant and the point at which the nutrient solution made contact with the stem. Since the difference in bioelectric potential required a period of 1 to 1½ hours to become stabilized after a change in light intensities, it was necessary to wait two hours after changing light intensities in order to obtain a reliable measure of the difference in potential between the two points measured on the stem of the bean plant. The first treatment was applied at 10:00 A.M., and the reading was recorded at noon. The second treatment was started at noon, and the corresponding reading was taken at 2:00 P.M. Likewise, the third and fourth treatment readings were recorded at 4:00 and 6:00 P.M., respectively. It was believed that time of day might have an effect on differences in bioelectric potential. Therefore, it was necessary to have the treatments (light intensities) applied once at each of the four times. A period of four days was required to run the experiment. The time of day was considered to be the row effect and the day of the week the column effect. The light intensities were the treatments.

The original data with three plants per day exposed to each of four light intensities and two readings on each plant under each of the light intensities are recorded in table VI-3. A different set of three plants was used on each of the four days. The order of applying light-intensity treatments was at random with the restriction that each of the treatments must occur in each of the orders over a four day period. The arrangement of the treatments (light intensities) and the various totals used in obtaining the analysis of variance in table VI-4 are given in table VI-3.

The sums of squares for the analysis of variance in table VI-4 are obtained in much the same manner as for previous examples. The total sum of squares is obtained by squaring the 96 determinations and subtracting the correction term,

$$64^2 + 65^2 + \cdots + 53^2 + 30^2 - \frac{4440^2}{96} = 15{,}654.00.$$

The row, column, and treatment sums of squares are, respectively,

$$\frac{1112^2 + 1296^2 + 1068^2 + 964^2}{3 \times 2 \times 4 = 24} - \frac{4440^2}{96} = 2403.33,$$

$$\frac{1226^2 + 1059^2 + 1135^2 + 1020^2}{24} - \frac{4440^2}{96} = 1032.58,$$

and

$$\frac{1174^2 + 1113^2 + 1113^2 + 1040^2}{24} - \frac{4440^2}{96} = 375.58.$$

TABLE VI-3. Differences in bioelectric potential (millivolts) between stem and nutrient solution for bean plants under four light intensity treatments (D = 0, L = 500, M = 900, and H = 1200 foot-candles of light) arranged in a 4 × 4 latin square

		Wednesday Plant Number 1	2	3	Thursday Plant Number 1	2	3	Friday Plant Number 1	2	3	Saturday Plant Number 1	2	3	Row Total
		Treatment H			Treatment L			Treatment M			Treatment D			
Noon	1st det.	64	65	35	56	34	64	38	37	44	32	46	47	
	2nd det.	60	64	32	56	34	64	38	34	43	32	46	47	
	Total	124	129	67	112	68	128	76	71	87	64	92	94	
Cell total				320			308			234			250	1112
		Treatment M			Treatment D			Treatment L			Treatment H			
2:00 PM	1st det.	59	63	62	67	56	57	59	54	38	36	51	49	
	2nd det.	58	65	62	65	58	56	59	52	37	34	50	49	
	Total	117	128	124	132	114	113	118	106	75	70	101	98	
Cell total				369			359			299			269	1296
		Treatment D			Treatment* M			Treatment H			Treatment L			
4:00 PM	1st det.	15	62	56	53	42	31	62	49	45	34	49	37	
	2nd det.	20	60	58	52	41	30	60	48	44	35	49	36	
	Total	35	122	114	105	83	61	122	97	89	69	98	73	
Cell total				271			249			308			240	1068
		Treatment L			Treatment H			Treatment D			Treatment M			
6:00 PM	1st det.	26	54	54	24	23	22	52	44	52	48	54	30	
	2nd det.	27	52	53	28	24	22	52	42	52	46	53	30	
	Total	53	106	107	52	47	44	104	86	104	94	107	60	
Cell total				266			143			294			261	964
Totals		329	485	412	401	312	346	420	360	355	297	398	325	4440
Column total				1226			1059			1135			1020	4440

TABLE VI-4. Analysis of variance of the data in table VI-3

Source of variation	Degrees of freedom		Sum of squares		Mean square	
Time of day (row)	3		2403.33		801.11	
Day of week (column)	3		1032.58		344.19	
Light intensities (treatment)	3		375.58		125.19	
Experimental error	6		3337.18		556.20	
Among plants within cells	32		8437.33		263.67	
Among plants in columns		8		3034.17		379.27
Remainder		24		5403.16		225.13
Between readings on the same plant	48		68.00		1.42	
Total	95		15654.00			

The experimental error sum of squares is obtained by subtracting the row, column, and treatment sums of squares from the sum of squares of the $k^2 = 16$ cell totals squared; i.e.,

$$\frac{320^2 + 308^2 + \cdots + 294^2 + 261^2}{6} - \frac{4440^2}{96} - (2403.33 + 1032.58 + 375.58)$$

$$= 212{,}498.67 - 205{,}350.00 - 3811.49$$

$$= 7148.67 - 3811.49 = 3337.18,$$

with 6 degrees of freedom.

The sum of squares associated with the variation among plant totals in each of the 16 cells is

$$\frac{124^2 + 129^2 + 67^2}{2} - \frac{320^2}{6} + \cdots + \frac{94^2 + 107^2 + 60^2}{2} - \frac{261^2}{6}$$

$$= 220{,}936.00 - 212{,}498.67 = 8437.33,$$

32 degrees of freedom. The sum of squares attributable to the variation among s within days is

$$\frac{329^2 + 485^2 + 412^2}{8} - \frac{1226^2}{24} + \cdots + \frac{297^2 + 398^2 + 325^2}{8} - \frac{1020^2}{24}$$

$$= 209{,}416.75 - 206{,}382.58 = 3034.17,$$

with 8 degrees of freedom. Subtracting the above sum of squares from that for variation among plant totals in each of the sixteen cells results in the remainder sum of squares which is a composite sum of squares of plants $\times$ treatments (ignoring rows) within columns, thus:

$$8437.33 - 3034.17 = 5403.16,$$

with 24 degrees of freedom.

The sum of squares of the differences among readings on the same plant is obtained as follows:

$$64^2 + 60^2 - \frac{124^2}{2} + 65^2 + 64^2 - \frac{129^2}{2} + \cdots + 30^2 + 30^2 - \frac{60^2}{2}$$
$$= 221{,}004.00 - 220{,}936.00 = 68.00 = \frac{(64 - 60)^2}{2} + \cdots + \frac{(30 - 30)^2}{2},$$

with 48 degrees of freedom.

In an experiment designed in this manner, it is possible to test several hypotheses. The experimental error is used to test the variation among treatment means. In this particular case the treatment mean square is less than the experimental error mean square which indicates somewhat more uniformity among the treatment means than might be expected in a population *with error variances of the magnitude* found in this experiment.

The F ratio of the experimental-error mean square and the mean square associated with the differences among plants within the cells of the 4×4 latin square is $F = 556.20/263.67 = 2.11$, which is slightly lower than the tabulated F value, 2.40, at the 5 per cent level of probability for 6 and 32 degrees of freedom.

One could test the hypothesis of no differences among the plant means within days by the F test,

$$F = \frac{379.27}{225.13} = 1.68.$$

The corresponding F value at the 5 per cent point for 8 and 24 degrees of freedom is equal to 2.36. The variation among plant means for each day could be tested in a like manner to determine if any group of three plants may be considered as unusually variable.

The variance attributable to the differences between readings on the same plant, 1.42, is extremely small in comparison with the remaining mean squares. The conclusion is that one reading per plant would be sufficient and that more homogeneous groups of plants are required. If this is impossible, then more plants per cell and more replicates of the treatments are required to obtain standard errors of a mean that are relatively small.

The treatment mean square is smaller, but not significantly so, than any of the others (table VI-4). Neither of the mean squares for time of day or day of the week exhibit any unusual variability. If the treatment mean square were larger than the error mean square, the next step in examining the experimental results might be to obtain the linear regression of light intensity and bioelectric potential.

The standard error of a light-intensity mean is $s_{\bar{x}} = \sqrt{556.20/2 \times 3 \times 4} = 4.81$, and the standard error of a difference of two treatment means is $s_{\bar{d}} = \sqrt{2(556.20)/2 \times 3 \times 4} = 6.81$. The efficiency of a latin square of this type relative to a randomized complete block design or to a completely randomized design may be computed from formulae (VI-7), (VI-8), or (VI-9). The coefficient of variation is equal to

$$cv = \frac{\sqrt{556.20/6}}{46.25} = 21 \text{ per cent},$$

which appears to be rather high for experimental work. There might be differences among the treatment, row, or column means, but the experimental material or methods were too variable to allow differences of this size to be detected. Following the results

from the initial analysis of variance, Taylor [288] studied the experimental material and the procedure. He found that plants with high potential readings tended to remain high and *vice versa*. With this information, the plants were divided into homogeneous groups with regard to magnitude of initial bioelectric potential readings, and the differences among the groups were confounded with day to day differences by applying the treatments to a different group each day. By refining his techniques further, it was possible to reduce the magnitude of the experimental error mean square and to detect differences among the treatments.

Another arrangement of the above experiment would be to use three different plants in each of sixteen cells of the 4 × 4 latin square, resulting in a total of forty-eight plants rather than the twelve used. This procedure was considered impractical owing to the amount of time required for setting up the apparatus to obtain readings on differences in bioelectric potentials between the stem of a bean plant and the nutrient solution in which its roots were submerged, but if it had been used, the breakdown of the total degrees of freedom would be

Source of variation	Degrees of freedom
Row (time of day)	3
Column (day of week)	3
Treatment (light intensities)	3
Error (experimental)	6
Among plants within cells	$2 \times 16 = 32$
Between readings on same plants	$1 \times 48 = 48$
Total	95

VI-1.9 MISSING DATA

VI-1.9.1 Missing experimental units. Allan and Wishart [1] and Yates [316] present the following formula for estimating a missing yield for the ith row, jth column, and hth treatment in a $k \times k$ latin square:

$$\hat{X}_{ijh} = \frac{k(X_{i..} + X_{.j.} + X_{..h}) - 2X_{...}}{(k-1)(k-2)}, \tag{VI-20}$$

where the totals are as defined previously. Yates [316] has also given an iterative method for estimating the yields for several missing values in a $k \times k$ latin square. For each missing datum computed, 1 degree of freedom is subtracted from the error degrees of freedom.

Bartlett [14] suggests the procedure of inserting a one for the missing value and zeros otherwise and performing a covariance analysis with the zeros and the one as the independent variate (see Chapter XVI). If more than one experimental unit is missing, the same procedure is followed except that a multiple covariance is performed. Nair [216] used Bartlett's [14] method for analyzing the results from a $k \times k$ latin square design with several missing values. A paper by DeLury [85] in 1946 summarizes most of the results for handling missing experimental units in latin squares or sets of latin squares.

The row, column, and treatment mean squares have expectations which are

slightly too large. The correct mean squares may be obtained with little additional work [316].

VI-1.9.2 Disproportionate numbers in the experimental unit. If disproportionate numbers of observations per experimental unit are available, one of the approximate methods of analysis described in V-1.6.2 may suffice. If a more precise analysis is desired, an extension of the methods described in Chapter 11 of Snedecor's *Statistical Methods* [273] may be used. It will be necessary to estimate treatment regression coefficients as well as those for rows and columns.

VI-1.9.3 Other situations. In some cases the latin square may be designed with $k - 1$ treatments in the k rows and columns or one of the treatments may have failed in the experiment. Yates [321] presents the analysis for this situation. Also, if one row or column has been lost or is omitted in the latin square, Yates [321] gives the method of analysis and illustrates it with a numerical example. Yates and Hale [333] give a method of analysis for two or more missing rows, columns, or treatments in a latin square. Their results are illustrated with an example. It should be pointed out that one or more rows, columns, or treatments may be missing either by design or by accident. DeLury [85] generalizes their results to several latin squares, and Smith [269] presents the analysis for missing observations in an incomplete latin square.

If two experimental unit yields are obtained as a single total and not individually, the method described by Bose and Mahalanobis [33] and by Nair [216] may be used to estimate the missing yields.

VI-2 Least Squares Estimates and Expectation of Mean Squares

In the following discussion, it is assumed that treatments, columns, rows, and squares represent random samples from their respective populations. If any or all of these items are considered to be from a finite population, adjustments in the expectations may be made in the manner described in Chapters IV and V.

VI-2.1 ONE UNIT PER EXPERIMENTAL UNIT

If a single observation is made on each plot of a latin square design, the yield of the ijhth observation may be expressed as

$$X_{ijh} = \mu + \rho_i + \lambda_j + \tau_h + \epsilon_{ijh}, \tag{VI-21}$$

where $i, j = 1, 2, \cdots, k$; $h = 1, 2, \cdots, k$ appears once in a row and once in a column; μ represents the population mean, ρ_i an effect common to the ith row, λ_j an effect common to the jth column, τ_h an effect common to the hth treatment, and ϵ_{ijh} an effect common to the ijhth observation; it is assumed

that the yield of any observation is expressible as the sum of several independent linear effects.

VI-2.1.1 Least squares estimates. The least squares estimates of the $3k + 1$ parameters, $\mu, \rho_1, \cdots, \rho_k, \lambda_1, \cdots, \lambda_k, \tau_1, \cdots, \tau_k$, are obtained by partial differentiation of the residual sum of squares with respect to the $3k + 1$ parameters, by setting the resulting equations equal to zero, and by solving for the set of estimates. The residual sum of squares after fitting the $3k + 1$ constants is

$$\sum_{i,j,h} (X_{ijh} - \hat{\mu} - r_i - c_j - t_h)^2, \tag{VI-22}$$

and the normal equations after differentiation are

$$X_{\cdots} = k\sum r_i + k\sum c_j + k\sum t_h + k^2\hat{\mu}, \tag{VI-23}$$

$$X_{i\cdot\cdot} = kr_i + \sum c_j + \sum t_h + k\hat{\mu}, \tag{VI-24}$$

$$X_{\cdot j\cdot} = \sum r_i + kc_j + \sum t_h + k\hat{\mu}, \tag{VI-25}$$

and

$$X_{\cdot\cdot h} = \sum r_i + \sum c_j + kt_h + k\hat{\mu}. \tag{VI-26}$$

The r_i, c_j, t_h, and $\hat{\mu}$ are the estimates which make the residual sum of squares a minimum. Now in order to obtain a unique solution the following restrictions are necessary:

$$\sum_i r_i = \sum_j c_j = \sum_h t_h = 0. \tag{VI-27}$$

With the above conditions, then,

$$\hat{\mu} = \bar{x} = X_{\cdots}/k^2, \tag{VI-28}$$

$$r_i = X_{i\cdot\cdot}/k - \hat{\mu} = \bar{x}_{i\cdot\cdot} - \bar{x}, \tag{VI-29}$$

$$c_j = X_{\cdot j\cdot}/k - \hat{\mu} = \bar{x}_{\cdot j\cdot} - \bar{x}, \tag{VI-30}$$

and

$$t_h = X_{\cdot\cdot h}/k - \hat{\mu} = \bar{x}_{\cdot\cdot h} - \bar{x}. \tag{VI-31}$$

The variances of the least squares estimates may be obtained as before (Chapters IV and V).

VI-2.1.2 Expectation of mean squares (Model II). The sum of squares due to $\hat{\mu}$ is the correction term and has the expectation:

$$E[\hat{\mu}X_{\cdots}] = E\left[\frac{X_{\cdots}^2}{k^2}\right] = E\frac{\left[\sum_{ijh}(\mu + \rho_i + \lambda_j + \tau_h + \epsilon_{ijh})\right]^2}{k^2}$$

$$= k^2\mu^2 + k\sigma_\rho^2 + k\sigma_\lambda^2 + k\sigma_\tau^2 + \sigma_\epsilon^2. \tag{VI-32}$$

The total sum of squares (uncorrected for the mean) has the expectation:

$$E\Big[\sum_{ijh} X_{ijh}^2\Big] = \sum_{ijh} E[\mu + \rho_i + \lambda_j + \tau_h + \epsilon_{ijh}]^2$$
$$= k^2(\mu^2 + \sigma_\rho^2 + \sigma_\lambda^2 + \sigma_\tau^2 + \sigma_\epsilon^2). \tag{VI-33}$$

The residual sum of squares after fitting r_i, c_j, t_h, and $\hat{\mu}$ has the expectation:

$$E[\sum X_{ijh}^2 - \hat{\mu}X_{...} - \sum r_i X_{i..} - \sum c_j X_{.j.} - \sum t_h X_{..h}]$$
$$= k^2(\mu^2 + \sigma_\rho^2 + \sigma_\lambda^2 + \sigma_\tau^2 + \sigma_\epsilon^2) - (k^2\mu^2 + k^2\sigma_\rho^2 + k\sigma_\lambda^2 + k\sigma_\tau^2 + k\sigma_\epsilon^2)$$
$$- (k^2\mu^2 + k\sigma_\rho^2 + k^2\sigma_\lambda^2 + k\sigma_\tau^2 + k\sigma_\epsilon^2) - (k^2\mu^2 + k\sigma_\rho^2 + k\sigma_\lambda^2 + k^2\sigma_\tau^2 + k\sigma_\epsilon^2)$$
$$+ 2(k^2\mu^2 + k\sigma_\rho^2 + k\sigma_\lambda^2 + k\sigma_\tau^2 + \sigma_\epsilon^2) = (k-1)(k-2)\sigma_\epsilon^2, \tag{VI-34}$$

which is the residual error variance times the degrees of freedom.

The sum of squares due to the r_i only is the sum of squares due to $\hat{\mu}$, r_i, c_j, and t_h minus the sum of squares due to $\hat{\mu}'$, c_j', and t_h', assuming r_i equal to zero; for the orthogonal case, the expectation is

$$E[\sum r_i X_{i..}] = E\left[\frac{\sum X_{i..}^2}{k} - \frac{X_{...}^2}{k^2}\right] = (k-1)(\sigma_\epsilon^2 + k\sigma_\rho^2), \tag{VI-35}$$

with $k - 1$ degrees of freedom.

In a like manner the sum of squares due to the c_j and the t_h have the respective expectations:

$$(k-1)(\sigma_\epsilon^2 + k\sigma_\lambda^2) \tag{VI-36}$$

and

$$(k-1)(\sigma_\epsilon^2 + k\sigma_\tau^2). \tag{VI-37}$$

The total sum of squares corrected for the mean has the expectation:

$$E\left[\sum_{ijh} X_{ijh}^2 - \frac{X_{...}^2}{k^2}\right]$$
$$= (k^2-1)\sigma_\epsilon^2 + (k-1)k\sigma_\tau^2 + (k-1)k\sigma_\rho^2 + (k-1)k\sigma_\lambda^2, \tag{VI-38}$$

which is the sum of the expectations for the treatment, row, column, and residual sum of squares.

VI-2.2 s SQUARES OF $k \times k$ LATIN SQUARES

The linear model for s groups or squares of $k \times k$ latin squares is

$$X_{ijhg} = \mu + \delta_i + \tau_j + \delta\tau_{ij} + \rho_{ih} + \lambda_{ig} + \epsilon_{ijhg}, \tag{VI-39}$$

where μ represents the population parameter for the mean, δ_i = an effect common to ith square, τ_j = effect common to jth treatment, $\delta\tau_{ij}$ = an effect common to the jth treatment in the ith square, ρ_{ih} = effect common to hth row in the ith square, λ_{ig} = effect common to the gth column in the ith

square, ϵ_{ijhg} = effect common to $ijhg$th observation, $i = 1, 2, \cdots, s$; $j = 1, 2, \cdots, k$ and appears once in a row and once in a column in each square; $h = 1, 2, \cdots, k$; and $g = 1, 2, \cdots, k$.

VI-2.2.1 Least squares estimates. The least squares estimates are obtained from the following sets of normal equations:

$$X_{....} = k^2\sum d_i + sk\sum t_j + k\sum\sum dt_{ij} + k\sum\sum r_{ih} + k\sum\sum c_{ig} + sk^2\hat{\mu}. \quad \text{(VI-40)}$$

$$X_{i...} = k^2 d_i + k\sum t_j + k\sum dt_{ij} + k\sum r_{ih} + k\sum c_{ig} + k^2\hat{\mu}. \quad \text{(VI-41)}$$

$$X_{.j..} = k\sum d_i + skt_j + k\sum dt_{ij} + \sum\sum r_{ih} + \sum\sum c_{ig} + sk\hat{\mu}. \quad \text{(VI-42)}$$

$$X_{ij..} = kd_i + kt_j + kdt_{ij} + \sum r_{ih} + \sum c_{ig} + k\hat{\mu}. \quad \text{(VI-43)}$$

$$X_{i.h.} = kd_i + \sum t_j + \sum dt_{ij} + kr_{ih} + \sum c_{ig} + k\hat{\mu}. \quad \text{(VI-44)}$$

$$X_{i..g} = kd_i + \sum t_j + \sum dt_{ij} + \sum r_{ih} + kc_{ig} + k\hat{\mu}. \quad \text{(VI-45)}$$

After imposing the restrictions,

$$\sum d_i = \sum t_j = \sum_h r_{ih} = \sum_g c_{ig} = \sum_i dt_{ij} = \sum_j dt_{ij} = 0, \quad \text{(VI-46)}$$

the estimates are found to be

$$\hat{\mu} = \bar{x} = X_{....}/sk^2, \quad \text{(VI-47)}$$

$$d_i = (X_{i...}/k^2) - \bar{x} = \bar{x}_{i...} - \bar{x}, \quad \text{(VI-48)}$$

$$t_j = (X_{.j..}/sk) - \bar{x} = \bar{x}_{.j..} - \bar{x}, \quad \text{(VI-49)}$$

$$r_{ih} = (X_{i.h.}/k) - \bar{x}_{i...}, \quad \text{(VI-50)}$$

$$c_{ig} = (X_{i..g}/k) - \bar{x}_{i...}, \quad \text{(VI-51)}$$

and

$$dt_{ij} = (X_{ij..}/k) - (X_{i...}/k^2) - (X_{.j..}/sk) + \bar{x}. \quad \text{(VI-52)}$$

VI-2.2.2 Expectation of mean squares (Model II). The total sum of squares corrected for the mean, or the sum of squares after fitting $\hat{\mu}$, has the expectation:

$$E\left[\sum X_{ijhg}^2 - \frac{X_{....}^2}{sk^2}\right] = \sum_{ijhg} E[\mu + \delta_i + \tau_j + \delta\tau_{ij} + \rho_{ih} + \lambda_{ig} + \epsilon_{ijhg}]^2$$
$$- \frac{1}{sk^2}E[sk^2\mu + k^2(\delta_1 + \cdots + \delta_s) + sk(\tau_1 + \cdots + \tau_k) + k(\delta\tau_{11} + \cdots + \delta\tau_{sk})$$
$$+ k(\rho_{11} + \cdots + \rho_{sk}) + k(\lambda_{11} + \cdots + \lambda_{sk}) + \sum\sum\sum\sum\epsilon_{ijhg}]^2$$
$$= sk^2(\mu^2 + \sigma_\delta^2 + \sigma_\tau^2 + \sigma_{\delta\tau}^2 + \sigma_\rho^2 + \sigma_\lambda^2 + \sigma_\epsilon^2) - (sk^2\mu^2 + k^2\sigma_\delta^2 + sk\sigma_\tau^2 + k\sigma_{\delta\tau}^2$$
$$+ k\sigma_\rho^2 + k\sigma_\lambda^2 + \sigma_\epsilon^2) = (sk^2 - 1)\sigma_\epsilon^2 + (sk - 1)k\sigma_\rho^2 + (sk - 1)k\sigma_\lambda^2$$
$$+ (sk - 1)k\sigma_{\delta\tau}^2 + sk(k - 1)\sigma_\tau^2 + k^2(s - 1)\sigma_\delta^2. \quad \text{(VI-53)}$$

The sum of squares for treatments has the expectation:

$$E[\sum t_j X_{.j..}] = E\left[\frac{\sum X_{.j..}^2}{sk} - \frac{X_{....}^2}{sk^2}\right] = (k-1)(\sigma_\epsilon^2 + k\sigma_{\delta\tau}^2 + sk\sigma_\tau^2), \quad \text{(VI-54)}$$

with $(k - 1)$ degrees of freedom.

The squares sum of squares has the expectation:

$$E[\sum d_i X_{i...}] = E\left[\sum_i \frac{X_{i...}^2}{k^2} - \frac{X_{....}^2}{sk^2}\right]$$
$$= (s-1)(\sigma_\epsilon^2 + k\sigma_\lambda^2 + k\sigma_\rho^2 + k\sigma_{\delta\tau}^2 + k^2\sigma_\delta^2), \quad \text{(VI-55)}$$

with $(s - 1)$ degrees of freedom.

The treatment $\times$ square sum of squares has the expectation:

$$E\left[\sum_i \sum_j \frac{X_{ij..}^2}{k} - \sum_i \frac{X_{i...}^2}{k^2} - \sum_j \frac{X_{.j..}^2}{sk} + \frac{X_{....}^2}{sk^2}\right]$$
$$= (s-1)(k-1)(\sigma_\epsilon^2 + k\sigma_{\delta\tau}^2). \quad \text{(VI-56)}$$

The row within squares, column within squares, and residual within-squares sums of squares have the respective expectations:

$$s(k-1)(\sigma_\epsilon^2 + k\sigma_\rho^2),\ s(k-1)(\sigma_\epsilon^2 + k\sigma_\lambda^2), \text{ and } s(k-1)(k-2)\sigma_\epsilon^2.$$

VI-2.3 $k \times k$ LATIN SQUARE WITH p ITEMS PER CELL AND d DETERMINATIONS ON EACH ITEM

It is assumed that the treatments, rows, columns, items, and determinations are random samples from their respective populations and that the yield of the $ijhgf$th observation may be expressed as the sum of the several independent effects; that is,

$$X_{ijhgf} = \mu + \rho_i + \lambda_j + \tau_h + \epsilon_{ijh} + \pi_{ijhg} + \delta_{ijhgf}, \quad \text{(VI-57)}$$

where μ = population mean, ρ_i = effect common to ith row, λ_j = effect common to jth column, τ_h = effect common to the hth treatment, ϵ_{ijh} = effect common to ijhth cell, π_{ijhg} = effect common to the gth item in the ijhth cell, δ_{ijhgf} = effect common to the $ijhgf$th determination, $i, j = 1, 2, \cdots, k$; $h = 1, 2, \cdots, k$ appears once in each row and once in each column; $g = 1, 2, \cdots, p$; and $f = 1, 2, \cdots, d$. For this example a new sample of items is used in each cell of the $k \times k$ latin square and determination f for one item has nothing in common with the fth determination for another item.

The least squares estimates may be obtained as before and are left as an exercise for the student. Also, the following expectations may be verified by the student:

Source of variation	Degrees of freedom	Average value of mean square
Row	$k - 1$	$\sigma_\delta^2 + d\sigma_\pi^2 + dp\sigma_\epsilon^2 + kdp\sigma_\rho^2$
Column	$k - 1$	$\sigma_\delta^2 + d\sigma_\pi^2 + dp\sigma_\epsilon^2 + kdp\sigma_\lambda^2$
Treatment	$k - 1$	$\sigma_\delta^2 + d\sigma_\pi^2 + dp\sigma_\epsilon^2 + kdp\sigma_\tau^2$
Residual	$(k-1)(k-2)$	$\sigma_\delta^2 + d\sigma_\pi^2 + dp\sigma_\epsilon^2$
Items within cells	$k^2(p-1)$	$\sigma_\delta^2 + d\sigma_\pi^2$
Determinations on same item	$pk^2(d-1)$	σ_δ^2
Total	dpk^2	

In the event that a sample of items is used for each column (see example VI-3), the linear model is

$$X_{ijhgf} = \mu + \rho_i + \lambda_j + \tau_h + \epsilon_{ijh} + \pi_{jg} + \alpha_{ijhg} + \delta_{ijhgf}, \quad \text{(VI-58)}$$

where the effects and subscripts are the same as before except for π_{jg} and α_{ijhg}. π_{jg} is the effect common to the gth item in the jth column, α_{ijhg} is the effect common to the $ijhg$th observation, and $g = 1, 2, \cdots, p$ in each column.

For this case the expectations for the various mean squares are

Source of variation	Degrees of freedom	Average value of mean square
Row	k − 1	$\sigma_\delta^2 + d\sigma_\alpha^2 + dp\sigma_\epsilon^2 + dpk\sigma_\rho^2$
Column	k − 1	$\sigma_\delta^2 + d\sigma_\alpha^2 + dk\sigma_\pi^2 + dp\sigma_\epsilon^2 + dpk\sigma_\lambda^2$
Treatment	k − 1	$\sigma_\delta^2 + d\sigma_\alpha^2 + dp\sigma_\epsilon^2 + dpk\sigma_\tau^2$
Residual	(k − 1)(k − 2)	$\sigma_\delta^2 + d\sigma_\alpha^2 + dp\sigma_\epsilon^2$
Items within columns	k(p − 1)	$\sigma_\delta^2 + d\sigma_\alpha^2 + dk\sigma_\pi^2$
Remainder	k(p − 1)(k − 1)	$\sigma_\delta^2 + d\sigma_\alpha^2$
Determinations on same item	pk²(d − 1)	σ_δ^2
Total	dpk² − 1	———

The correction term has the expectation:

$$E\left[\frac{X_{\cdots}^2}{dpk^2}\right] = \frac{1}{dpk^2}E[dpk^2\mu + dpk(\sum\rho_i + \sum\lambda_j + \sum\tau_h) + dp\sum\sum\sum\epsilon_{ijh}$$

$$+ dk\sum\sum\pi_{jg} + d\sum\sum\sum\sum\alpha_{ijhg} + \sum\sum\sum\sum\sum\delta_{ijhgf}]^2$$

$$= dpk^2\mu^2 + dpk(\sigma_\rho^2 + \sigma_\lambda^2 + \sigma_\tau^2) + dp\sigma_\epsilon^2 + dk\sigma_\pi^2 + d\sigma_\alpha^2 + \sigma_\delta^2 = CT. \quad \text{(VI-59)}$$

The items-within-columns sum of squares with $k(p - 1)$ degrees of freedom has the expectation:

$$E\left[\frac{\sum\sum X_{\cdot j \cdot g \cdot}^2}{dk} - \sum_j \frac{X_{\cdot j \cdots}^2}{dpk}\right]$$

$$= dpk^2\mu^2 + dpk\sigma_\rho^2 + dpk^2\sigma_\lambda^2 + dpk\sigma_\tau^2 + dpk\sigma_\epsilon^2 + dpk^2\sigma_\pi^2 + dpk\sigma_\alpha^2 + kp\sigma_\delta^2$$

$$- [dpk^2\mu^2 + dpk^2\sigma_\lambda^2 + dpk(\sigma_\rho^2 + \sigma_\tau^2 + \sigma_\epsilon^2) + dk^2\sigma_\pi^2 + dk\sigma_\alpha^2 + k\sigma_\delta^2]$$

$$= k(p - 1)[\sigma_\delta^2 + d\sigma_\alpha^2 + dk\sigma_\pi^2]. \quad \text{(VI-60)}$$

The remaining expectations are obtained similarly and are left as an exercise for the student. For further discussion the reader is referred to Wilks [308, sec. 9.4], Mann [207], and Kempthorne [175, Ch. 10].

VI-3 Development of Formulae for Missing Experimental Units

For estimating the value of a single experimental unit, there is no loss in generality if we assume that the item is missing in the first row, in the first

column, and for treatment one. This is true since any arrangement of a latin square can be rearranged in this way. The residual sum of squares, including the missing value, $\hat{X}_{111}$, is

$$\hat{X}_{111}^2 + \sum_{ijh} X_{ijh}^2 - \frac{1}{k}\left\{(X_{1..} + \hat{X}_{111})^2 + \sum_{2}^{k} X_{i..}^2 + (X_{.1.} + \hat{X}_{111})^2 + \sum_{2}^{k} X_{.j.}^2 + (X_{..1} + \hat{X}_{111})^2 + \sum_{2}^{k} X_{..h}^2\right\} + \frac{2(X_{...} + \hat{X}_{111})^2}{k^2} \quad \text{(VI-61)}$$

The various totals are composed of the yield values in the latin square, assuming $\hat{X}_{111}$ equals zero. The partial derivative of the above sum of squares with respect to $\hat{X}_{111}$ is

$$2\hat{X}_{111} - \frac{2}{k}\left\{X_{1..} + \hat{X}_{111} + X_{.1.} + \hat{X}_{111} + X_{..1} + \hat{X}_{111}\right\} + \frac{4(X_{...} + \hat{X}_{111})}{k^2} \quad \text{(VI-62)}$$

We set the above equation equal to zero and solve for $\hat{X}_{111}$. The result is equation (VI-20).

Suppose now that two values are missing and that the values to be estimated are $\hat{X}_{111}$ and $\hat{X}_{221}$; i.e., two of the yields for treatment one need to be estimated. The error sum of squares is

$$\begin{aligned}&\hat{X}_{111}^2 + \hat{X}_{221}^2 + \sum X_{ijh}^2 - \frac{1}{k}\Big\{(X_{1..} + \hat{X}_{111})^2 + (X_{2..} + \hat{X}_{221})^2 \\ &+ X_{3..}^2 + \cdots + X_{k..}^2 + (X_{.1.} + \hat{X}_{111})^2 + (X_{.2.} + \hat{X}_{221})^2 \\ &+ X_{.3.}^2 + \cdots + X_{.k.}^2 + (X_{..1} + \hat{X}_{111} + \hat{X}_{221})^2 + X_{..2}^2 + \cdots + X_{..k}^2\Big\} \\ &+ \frac{2}{k^2}(X_{...} + \hat{X}_{111} + \hat{X}_{221})^2, \end{aligned} \quad \text{(VI-63)}$$

where the totals are composed of the available yield values.

The partial derivatives of equation (VI-63) with respect to $\hat{X}_{111}$ and $\hat{X}_{221}$ are

$$2\hat{X}_{111} - \frac{2}{k}\left\{X_{1..} + X_{.1.} + X_{..1} + 3\hat{X}_{111} + \hat{X}_{221}\right\} + \frac{4}{k^2}\left\{X_{...} + \hat{X}_{111} + \hat{X}_{221}\right\} \quad \text{(VI-64)}$$

and

$$2\hat{X}_{221} - \frac{2}{k}\left\{X_{2..} + X_{.2.} + X_{..1} + X_{111} + 3\hat{X}_{221}\right\} + \frac{4}{k^2}\left\{X_{...} + \hat{X}_{111} + \hat{X}_{221}\right\}. \quad \text{(VI-65)}$$

If we set the above two formulae equal to zero and solve for $\hat{X}_{111}$ and $\hat{X}_{221}$, we obtain

$$\hat{X}_{111} = \frac{k(X_{1..} + X_{.1.} + X_{..1}) - 2X_{...}}{k(k-2)^2/(k-1)} + \frac{k(X_{2..} + X_{.2.} + X_{..1}) - 2X_{...}}{k(k-2)^2} \quad \text{(VI-66)}$$

and

$$\hat{X}_{221} = \frac{k(X_{2..} + X_{.2.} + X_{..1}) - 2X_{...}}{k(k-2)^2/(k-1)} + \frac{k(X_{1..} + X_{.1.} + X_{..1}) - 2X_{...}}{k(k-2)^2}. \quad \text{(VI-67)}$$

The above formulae may be used to estimate two missing values in the same row or in the same column merely by changing the subscript designations. Also, the same procedure may be used to obtain formulae for other combinations or more missing values. For example, suppose that the two missing values are for different rows, columns, and treatments and that the two values to be estimated are $\hat{X}_{111}$ and $\hat{X}_{222}$. If the above procedure of minimizing the residual sum of squares is followed, it will be found that the values of $\hat{X}_{111}$ and $\hat{X}_{222}$ are given by the formulae,

$$\hat{X}_{111} = \frac{k(X_{1..} + X_{.1.} + X_{..1}) - 2X_{...}}{k(k-3)(k^2-3k+4)/(k-1)(k-2)} - 2\left\{\frac{k(X_{2..} + X_{.2.} + X_{..2}) - 2X_{...}}{k(k-3)(k^2-3k+4)}\right\} \qquad \text{(VI-68)}$$

and

$$\hat{X}_{222} = \frac{k(X_{2..} + X_{.2.} + X_{..2}) - 2X_{...}}{k(k-3)(k^2-3k+4)/(k-1)(k-2)} - 2\left\{\frac{k(X_{1..} + X_{.1.} + X_{..1}) - 2X_{...}}{k(k-3)(k^2-3k+4)}\right\}. \qquad \text{(VI-69)}$$

No loss in generality is suffered by the procedure outlined above, since formulae (VI-68) and (VI-69) may be used to estimate missing values for different items of the three categories, rows, columns, and treatments.

CHAPTER VII

The Choice of Treatments and the Factorial Experiment—p^n Series

VII-1 Selection of Treatments and Treatment Combinations

The foregoing chapters deal with the construction, layout, and statistical analysis of some of the simpler and more widely used experimental or observational designs and with plot or pen techniques. The merits and faults of each of the designs are discussed. Before proceeding to more complicated experimental designs, it is deemed desirable to discuss the concepts related to the design and to the analysis of "factorial" experiments. The material developed in the present chapter is used to a considerable extent in the following chapters.

In many experiments, success or failure may depend more upon the selection of treatments for comparisons to be made than upon the design. The selection of both the design and of the treatments is important, and neither should be slighted in planning the experiment. For example, the design of the experiment might be a latin square and the treatments new methods or new varieties. The design may be quite appropriate, but the selection of the treatments may include only new methods or varieties affording no comparisons (assuming these comparisons are desired) with the standard method or check variety. On the other hand, the *proper choice* of treatments may have been made but a *poor choice* of design, such as a systematic, might invalidate the results.

Other examples of the selection of treatments could be cited, but the only one that merits considerable discussion is the group of treatments involving two or more levels or kinds of two or more substances or factors in combinations; a few such examples are

(i) light, oxygen, and temperature,
(ii) temperature and length of storage,
(iii) spacings and rates of planting,
(iv) levels of two or more fertilizers,
(v) levels of ingredients and methods of mixing,
(vi) levels of ingredients and baking temperatures,
(vii) length and types of mixings,

(viii) levels of proteins and carbohydrates in feeding trials,
(ix) levels of insecticides or fungicides and varieties or species,
(x) methods of teaching and schools,
(xi) teaching methods and age of students,
(xii) stores and price premiums, etc.

A group of treatments which contains two or more levels of two or more factors or substances in all combinations is known as a *factorial* arrangement. As indicated by the various types of *factorials* listed above, the factorial arrangement of treatments covers a wide variety of experiments.

For the successful conclusion of an experiment, several items, of which the choice of the treatments and of the design represent only one phase in the planning of an experiment, are of considerable importance. Any experiment may be planned satisfactorily, but many things can go wrong in conducting the experiment: the experimental site might be located in the path of floods or made variable in other ways; the measurements could be recorded unreliably, thereby vitiating the results; a poor choice of size and shape of experimental unit and replicate and number of replicates may have bearing on the successful conclusion of an experiment, etc. (see Chapter I). However, it will be assumed that the other elements of scientific experimentation have been considered and that a factorial experiment has been decided upon. Some general concepts and definitions related to factorial arrangements and some alternative procedures will be cited prior to the discussion of specific factorial experiments and illustrative examples.

VII-2 The Factorial Experiment

The factorial experiment was called the "complex experiment" prior to 1926 when Fisher [125] designated it as the factorial experiment. Yates [319, 324] followed this notation, and at present an experiment containing all combinations of several levels of several factors is known almost exclusively as a factorial experiment. Yates [319] states that the complex or factorial experiment has been used on wheat experiments at Broadbalk since 1843-44 and on barley experiments at Hoosfield since 1852. Although Fisher [125] is primarily responsible for the development and analysis of factorial experiments, Yates [319, 324] deserves no small amount of the credit for extending the development and for obtaining the analysis for factorial experiments. The classic work on factorial designs in complete block and in incomplete block designs is the pamphlet written by Yates [324] in 1937 and entitled ' The design and analysis of factorial experiments." Except for some notational deviations, the definitions, concepts, and analysis used in the present text follow those of Yates.

VII-2.1 DEFINITIONS OF MAIN EFFECTS AND INTERACTIONS

Before proceeding to the design and analysis of factorial experiments, some definitions are set forth and some facts are noted. First, the factorial

experiment, in itself, is not considered as an experimental design. Any of the experimental designs discussed thus far, or others, may be used for the factorial experiment. The choice of treatments, the "treatment design," determines whether or not the experiment is a factorial. However, the construction and analysis of factorial experiments are extremely useful in the design and analysis of certain incomplete block designs. Second, the factors are designated by lower case letters a, b, c, etc. The particular level of a factor is denoted by the lower case letter with a subscript a_i, b_j, c_h, etc. The particular combination of the various factors is denoted by either symbol $a_ib_jc_h\cdots$ or $ijh\cdots$. The latter designation is the one most commonly used in the remainder of the text. Third, comparisons among the combinations (treatments) in the factorial experiment are denoted by capital letters A, B, $A \times B$, C, etc. These symbols denote the *effects*. The effects in a factorial experiment are composed of *main effects* and *interactions*. A main effect, say A, represents a set of $p - 1$ orthogonal single-degree-of-freedom contrasts among the p totals, say $(A)_i$, of a given factor, say a. The levels of an effect are designated by the symbols $(A)_0$, $(A)_1$, $\cdots$, $(A)_{p-1}$. The parentheses around the letter denote that the level of the effect, say $(A)_i$, is a total of all experimental units contributing to the ith level of the effect; the symbol A_i denotes that the level of the effect is on an experimental unit basis. Likewise, the symbol (A) denotes that the effect is on a total basis, whereas the symbol A denotes that the effect is on an experimental unit basis. Yates [324] uses square brackets instead of parentheses to denote that the effect is on a total basis; thus, $(A) = [A]$. Fourth, all the units are used to evaluate main effects and interactions. Fifth, the lowest or zero level of a factor need not be zero or none but is the lowest level of the factor considered pertinent to the experiment; e.g., 50 pounds equals zero or the lowest level and 150 pounds equals one or the highest level of a factor. Last, the interaction of factors needs to be defined both by words and with symbols.

The interaction of two factors is the failure of the levels of one factor, say a, to retain the same order and magnitude of performance (within random sampling errors) throughout all levels of the second factor, say b. For two levels, zero and one, of the two factors a and b, the failure of the yields from the zero and one levels of factor a, say a_0 and a_1 on the one level of b, say b_1, to be of the same magnitude as on the zero level of b, say b_0, is a measure of the interaction of the factors a and b. Symbolically, the interaction of the two factors a and b at zero and one levels is $(AB) = (a_1b_1 - a_0b_1) - (a_1b_0 - a_0b_0)$. A graphical picture of an interaction of two factors each at two levels is presented in figure VII-1. Zero interaction would be obtained if the yields for the factor b_0 were as pictured by the broken line.[1]

The interaction of two factors each at three levels is pictured in figure

[1]Other types of interaction are possible (e.g., quantity $\times$ quality interactions [126, sec. 50-53; 256]), but these are not discussed in the present text [also, see 311].

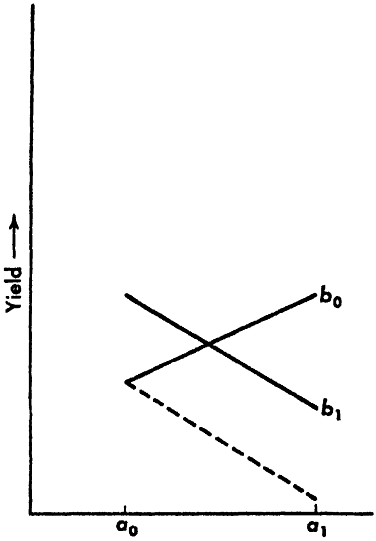


Figure VII-1. Graphical representation of a two-factor interaction for a 2 × 2 factorial.

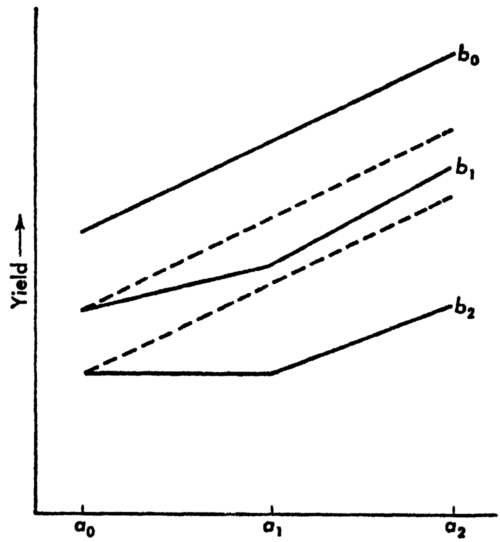


Figure VII-2. Graphical representation of a two-factor interaction for a 3 × 3 factorial.

VII-2. Zero interaction of the two factors is obtained when the graphical array of yields is as pictured by the broken lines. The graph for the broken lines is obtained by plotting the expected values for zero interaction as per the following table:

	b_0	b_1	b_2
a_0	x	$x + y$	$x + z$
a_1	$x + u$	$x + y + u$	$x + z + u$
a_2	$x + v$	$x + y + v$	$x + z + v$

The sum of squares of the deviations of observed values from the above values is the sum of squares due to interaction. The resulting mean square is then compared with the error mean square to determine whether or not there is an interaction of the factors.

The interaction of three factors, say a, b, and c, is illustrated graphically in figure VII-3. If the graph for c_1 followed the broken lines, there would be a zero three-factor interaction. As illustrated, a nonsignificant (or significant) three-factor interaction does not require that the two-factor interactions be nonsignificant (or significant) but that the interaction of two factors be the same for all levels of the third factor (see problem VII-3).

The above definition of an interaction allows interchange of any of the letters in the graph. The interaction of a with b, or $A \times B$, is the same as the interaction of b with a, or $B \times A$. Likewise, the interaction of three

factors represents the failure for a two-factor interaction, say $A \times B$, to remain relatively constant at all levels of the third factor, say c; also, the interaction of three factors is the failure for $A \times C$ to remain relatively the same for all levels of b.

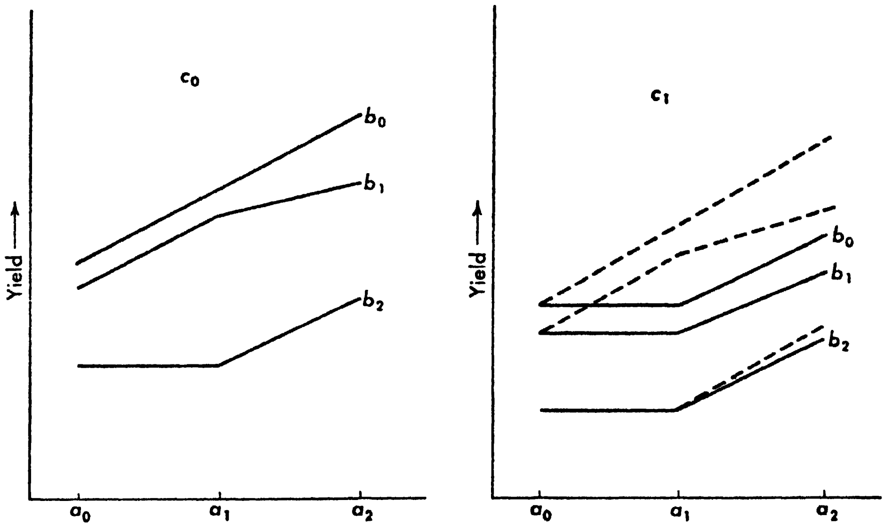


Figure VII-3. Graphical representation of a three-factor interaction for a $3 \times 3 \times 2$ factorial.

The notation $A \times B$ is used to denote the interaction of two factors and $A \times B \times C$ to denote the interaction of three factors. These interactions may be partitioned into component parts. According to a convenient symbolism, which is introduced later, AB represents one part of the $A \times B$ interaction when the factors are present at more than two levels. Likewise, the higher-factor interactions may be partitioned into component parts.

The two-factor interaction sum of squares may be obtained from the formula,

$$\begin{aligned}
&\frac{1}{r}\sum_j\sum_h\left\{X_{.jh} - \frac{X_{.j.}}{q} - \frac{X_{..h}}{p} + \frac{X_{...}}{pq}\right\}^2 \\
&= \frac{1}{r}\left\{\sum_j\sum_h X_{.jh}^2 - \frac{1}{q}\sum X_{.j.}^2 - \frac{1}{p}\sum X_{..h}^2 + \frac{X_{...}^2}{pq}\right\} \\
&= \frac{1}{r}\left\{\left(\sum_j\sum_h X_{.jh}^2 - \frac{X_{...}^2}{pq}\right) - \left(\frac{1}{q}\sum X_{.j.}^2 - \frac{X_{...}^2}{pq}\right)\right. \\
&\quad \left. - \left(\frac{1}{p}\sum X_{..h}^2 - \frac{X_{...}^2}{pq}\right)\right\}, \qquad \text{(VII-1)}
\end{aligned}$$

where $j = 1, 2, \cdots, p$; $h = 1, 2, \cdots, q$; the factor a is at p levels, the factor b is at q levels, and there are r replicates on each combination. The dot indicates summation over the subscript it replaces.

The three-factor interaction is obtained from the formula,

$$\frac{1}{r}\sum_j\sum_h\sum_g\left\{X_{\cdot jhg} + \frac{X_{\cdot j\cdot\cdot}}{kq} + \frac{X_{\cdot\cdot h\cdot}}{kp} + \frac{X_{\cdot\cdot\cdot g}}{pq} - \frac{X_{\cdot jh\cdot}}{k} - \frac{X_{\cdot j\cdot g}}{q} - \frac{X_{\cdot\cdot hg}}{p} - \frac{X_{\cdot\cdot\cdot\cdot}}{kpq}\right\}^2$$

$$= \frac{1}{r}\left\{\sum_j\sum_h\sum_g X_{\cdot jhg}^2 + \frac{1}{kq}\sum X_{\cdot j\cdot\cdot}^2 + \frac{1}{kp}\sum X_{\cdot\cdot h\cdot}^2 + \frac{1}{pq}\sum X_{\cdot\cdot\cdot g}^2 - \frac{1}{k}\sum\sum X_{\cdot jh\cdot}^2\right.$$

$$\left. - \frac{1}{q}\sum\sum X_{\cdot j\cdot g}^2 - \frac{1}{p}\sum\sum X_{\cdot\cdot hg}^2 - \frac{X_{\cdot\cdot\cdot\cdot}^2}{kpq}\right\}, \qquad \text{(VII-2)}$$

where $g = 1, 2, \cdots, k$ levels of factor c and the other quantities are as defined above. Four- and higher-factor interactions may be obtained from an extended form of equations (VII-1) and (VII-2) or by alternative procedures. Further discussions on the formation, calculation, and interpretation of various interactions are given later for specific factorials and examples.

VII-2.2 ADVANTAGES AND DISADVANTAGES OF FACTORIAL ARRANGEMENTS

As stated at the beginning of this section, all combinations are used to evaluate main effects and interactions in a factorial experiment. This is not true for other arrangements of treatments. Therefore, there is some loss in information in nonfactorial arrangements owing to the fact that only a fraction of the total number of experimental units is used to evaluate an effect. The percentage of the total number of experimental units used to evaluate an effect depends upon the composition of treatments in a nonfactorial experiment. In addition, the main effects in a factorial arrangement are evaluated over a wider range of conditions, and information is obtained on the interaction of factors. In fact, if the objective of the experiment is to estimate main effects and interactions, a factorial arrangement of treatments (either a complete factorial or an appropriate subset) is optimum for this purpose [175, p. 425]. A factorial experiment allows description of the response pattern over a prescribed surface. If the objective of the experiment is to describe a particular point on the response surface, a nonfactorial set of treatments may be more appropriate than a factorial set (see section VII-3).

The use of all experimental units in evaluating an effect increases the efficiency of the particular experiment. The concept of "hidden replication" as opposed to "absolute replication" [126, sec. 40] or the repetition of the set of treatments should be noted. Fisher [126, sec. 40] and Yates [324] state that it is often desirable to include additional factors in an experiment in order to observe the effects of interest over a wider range of conditions. As an example of this, an experimenter might include dates of planting or sampling as an additional factor in a variety yield trial to measure, to some

extent, seasonal variations. Also, several technicians instead of a single one may be used, or animals from several strains may be included in the experiment in order to cover a wider range of conditions for the treatments being compared. Too often this fact is overlooked in experimental work, and the different "absolute" replicates are too much alike to make inferences of the desired nature. However, for experiments on the standardization of techniques, it may be desirable to limit the number of subsidiary factors in the preliminary stages of the study.

Factorial experiments result in unbiased conclusions even if trends or gradients are present in the experiment, although their effectiveness is somewhat reduced [102, Ch. 13]. Worker fatigue, temperature, light, and humidity changes, gradients in material, certain types of interactions, etc., account for trends in experimental material.

Briefly then, the advantages of a factorial experiment may be summarized as follows:

(i) all experimental units are utilized in evaluating effects, resulting in the most efficient use of resources;
(ii) the effects are evaluated over a wider range of conditions with the minimum outlay of resources;
(iii) an estimate of the interaction of the factors is obtainable;
(iv) unbiased estimates of effects are obtained whether or not time trends are present;
(v) a factorial set of treatments is optimum for estimating main effects and interactions.

The so-called disadvantages of factorial arrangements of treatment combinations are often more superficial than real [60; 126; 102, Ch. 13; 324]. The factorial arrangement may turn out to be an inefficient procedure to obtain the answer for a specific question. However, information on a number of effects and interactions is usually preferable to information upon a specific combination. The objectives of the experiment will determine whether or not a factorial arrangement of the treatments is desirable.

One real disadvantage of factorial arrangements is the large and prohibitive number of combinations necessary to study several factors at several levels. For example, a factorial arrangement of seven factors each at three levels requires 2187 combinations, and a factorial arrangement of ten factors each at two levels requires 1024 combinations. The scheme known as "fractional replication" (see Chapter IX) greatly reduces the number of combinations required to estimate certain effects.

Associated with the large number of treatment combinations required in some factorial experiments is the decrease in the efficiency of an experiment due to increasing the size of the replicate. This difficulty may be overcome by "confounding" some of the unimportant effects with "incomplete blocks" differences; the incomplete block contains only a portion of the total number

of treatment combinations and, therefore, does not include as much of the experimental material as the complete block. With confounding, the complexity of computations increases; the extent to which this is a disadvantage depends upon the complexity of the analysis and the available computing facilities.

VII-2.3 CHOICE OF LEVELS

The choice of levels of factors to be used in an experiment depends upon the nature of the experimental yields and upon the objectives of the experiment [82, 268, 271]. The form of the response curve and the portion of the curve studied determine the number and location of the levels to be investigated. If the experimenter knows the range in levels of interest and if he desires to investigate the form of the response curve, he should select as many levels as are practical. The selected levels may or may not be equally spaced. To facilitate the computations of curvilinear responses of levels [273] of an effect, it is suggested that equally spaced levels be chosen; i.e., one unit, two units, three units, etc., or k^0 units, k^1 units, k^2 units, etc. The latter levels, k^i, are equidistant on a logarithmic scale. If the range in which the factor is effective is unknown, but the lower (or the upper) limit is known, then it is suggested that the first level be Ck^0, the second level be Ck^1, the third level be Ck^2, etc. where C is the amount used for the first level; with such a scheme a wide range is covered with a limited number of levels.

VII-3 Procedures to Investigate Specific Objectives

A number of procedures have been developed to investigate specific areas or points on the response surface. If the objective of the experiment is to estimate the concentration of a drug yielding a specified percentage of successes or to estimate the combination of two factors giving the maximum (or minimum) yield, specific procedures resulting in a nonfactorial selection of treatments are available. The probit technique method is used extensively to estimate the concentration of a drug necessary to obtain a given percentage of success [23; 24; 126, sec. 69]. Alternative procedures for this problem have been proposed by Dixon and Mood [91] and by Robbins and Munro [257]. Procedures for estimating a maximum have been described by a number of authors [102, Ch. 13; 161; 182]. The procedure developed by Kiefer and Wolfowitz [182] is operationally simple and tends to concentrate the combinations of factors used near the combination giving the maximum yield. A more general approach for studying the entire response surface has been presented by Box *et al.* [35, 36].

VII-4 The Factorial Experiment—2^n Series

VII-4.1 THE 2 × 2 FACTORIAL

Although some experimenters may prefer to think of the effects and interactions in terms of the I, J, W, X, Y, Z, etc. effects outlined by Yates [324], it has been found that the modulo notation [29, 30, 173, 175, 177] is extremely

useful for the more complex factorial experiments and for confounding certain effects with block differences (see following chapters). The method has been found to be useful in the design and analysis of incomplete block designs [177]. The use of modulo notation in the present text is confined to prime numbers (2, 3, 5, 7, 11, 13, 17, etc.) or powers of prime numbers. The system used does not work for nonprime numbers in that the effects so estimated are partially confounded. For modulo p the following relationship among numbers exists:

$$
\begin{aligned}
0 &= p = 2p = \cdots = qp;\\
1 &= p + 1 = 2p + 1 = \cdots = qp + 1;\\
2 &= p + 2 = 2p + 2 = \cdots = qp + 2;\\
&\cdot\\
&\cdot\\
&\cdot\\
p - 1 &= 2p - 1 = 3p - 1 = \cdots = qp + p - 1.
\end{aligned}
$$

The subscripts of the factors a, b, c, etc. are i, j, h, etc., where $i = 0, 1, \cdots, p - 1$; $j = 0, 1, \cdots, p - 1$; $h = 0, 1, \cdots, p - 1$; etc. For a $p^n = 2^2$ factorial the levels of the main effects and interactions are composed of the following treatments:

$$
\left.
\begin{aligned}
(A)_{i=0} &= a_0b_0 + a_0b_1 = 00 + 01,\\
(A)_{i=1} &= a_1b_0 + a_1b_1 = 10 + 11,\\
(B)_{j=0} &= a_0b_0 + a_1b_0 = 00 + 10,\\
(B)_{j=1} &= a_0b_1 + a_1b_1 = 01 + 11,\\
(AB)_{i+j=0} &= a_0b_0 + a_1b_1 = 00 + 11,\\
(AB)_{i+j=1} &= a_0b_1 + a_1b_0 = 01 + 10,
\end{aligned}
\right\} \qquad \text{(VII-3)}
$$

and the effects and interactions are

$$(A) = (A)_1 - (A)_0 = (10+11) - (00+01);\ A = \frac{1}{2r}(10+11-00-01); \qquad \text{(VII-4)}$$

$$(B) = (B)_1 - (B)_0 = (01+11) - (00+10);\ B = \frac{1}{2r}(01+11-00-10); \qquad \text{(VII-5)}$$

$$(AB) = (AB)_0 - (AB)_1 = (00+11) - (01+10);\ AB = \frac{1}{2r}(00+11-01-10), \qquad \text{(VII-6)}$$

where r = the number of replicates and the treatment designations are replaced by treatment totals. The two-factor interaction effect AB is estimated as the difference in the two levels of one factor compared at the two levels of the second factor; thus:

$$
\begin{aligned}
(AB) &= [b_1 - b_0] \text{ in the presence of } a_1 - [b_1 - b_0] \text{ in the presence of } a_0\\
&= a_1b_1 - a_1b_0 - a_0b_1 + a_0b_0\\
&= 11 + 00 - 10 - 01. \qquad \text{(VII-7)}
\end{aligned}
$$

Also, the interaction AB is equal to the interaction BA:
$(BA) = [a_1 - a_0]$ in the presence of $b_1 - [a_1 - a_0]$ in the presence of b_0

$$= 11 + 00 - 01 - 10 = (AB) = (AB)_0 - (AB)_1. \qquad \text{(VII-8)}$$

The above comparisons correspond to the table of plus and minus signs used prevalently in statistical references [60, 273, 324];

	Combination of treatments			
Effect	$a_0b_0 = 1$	$a_1b_0 = a$	$a_0b_1 = b$	$a_1b_1 = ab$
(A)	−	+	−	+
(B)	−	−	+	+
(AB)	+	−	−	+
Total	+	+	+	+

Thus, $(A) = -a_0b_0 + a_1b_0 - a_0b_1 + a_1b_1 = 10 - 00 + 11 - 01$, etc.

Yates [324] and Fisher [126, sec. 43] present a set of algebraic quantities for obtaining the signs in the above table. They define the factor levels as follows: $a_0 = 1$, $a_1 = a$, $b_0 = 1$, $b_1 = b$, and $a_0b_0 = 1$, $a_1b_0 = a$, $a_0b_1 = b$, and $a_1b_1 = ab$. Then, the following algebraic quantities give the signs for the effects in the above table:

$$\left.\begin{aligned} (A) &= (a-1)(b+1) = a - 1 + ab - b \\ (B) &= (a+1)(b-1) = b - 1 + ab - a \\ (AB) &= (a-1)(b-1) = ab + 1 - a - b \end{aligned}\right\}. \qquad \text{(VII-9)}$$

Another (equivalent) procedure for obtaining the coefficients for the interaction AB is to multiply the coefficients for A by the corresponding ones for B.

The sums of squares for the main effects and interaction from r replicates are

$$\frac{[(A)_1 - (A)_0]^2}{4r} = \frac{(X_{\cdot 11} + X_{\cdot 10} - X_{\cdot 00} - X_{\cdot 01})^2}{r[1^2 + 1^2 + (-1)^2 + (-1)^2]}, \qquad \text{(VII-10)}$$

$$\frac{[(B)_1 - (B)_0]^2}{4r} = \frac{(X_{\cdot 11} + X_{\cdot 01} - X_{\cdot 00} - X_{\cdot 10})^2}{r(1 + 1 + 1 + 1)}, \qquad \text{(VII-11)}$$

and

$$\frac{[(AB)_0 - (AB)_1]^2}{4r} = \frac{(X_{\cdot 11} + X_{\cdot 00} - X_{\cdot 10} - X_{\cdot 01})^2}{r(1 + 1 + 1 + 1)}, \qquad \text{(VII-12)}$$

where $X_{\cdot 00}$, $X_{\cdot 01}$, $X_{\cdot 10}$, and $X_{\cdot 11}$ are the totals for treatments 00, 01, 10, and 11, respectively. The replicate and error sum of squares are obtained in the usual manner.

Individual errors may be computed for each comparison; i.e., the interaction of the three effects A, B, and AB with replicates yield three error terms each with $r - 1$ degrees of freedom [273, sec. 15.7]. Usually the pooled

error is used in an experiment, since the individual error variances are considered to be estimates of the same residual error variance σ_ϵ^2.

From the above table of plus and minus signs, it should be noted that the treatment totals may be obtained from the effect totals. This fact is important in the analysis of experiments in which some of the effects are confounded. The various totals may be obtained from the following formulae:

$$X_{\cdot 00} = \frac{-(A) - (B) + (AB) + X_{\cdots}}{4},$$

$$X_{\cdot 10} = \frac{(A) - (B) - (AB) + X_{\cdots}}{4},$$

$$X_{\cdot 01} = \frac{-(A) + (B) - (AB) + X_{\cdots}}{4},$$

and

$$X_{\cdot 11} = \frac{(A) + (B) + (AB) + X_{\cdots}}{4},$$

where $X_{\cdots}$ is the total for the experiment. In general,

$$X_{\cdot ij} = \frac{(-1)^{i-1}(A) + (-1)^{j-1}(B) + (-1)^{i+j}(AB) + X_{\cdots}}{4}$$

$$= \frac{(A)_i + (B)_j + (AB)_{i+j}}{2} - \frac{2X_{\cdots}}{4}, \qquad \text{(VII-13)}$$

since

$$(A) = (A)_1 - (A)_0 = 2[(A)_1 - X_{\cdots}/2]$$

$$= 2[X_{\cdots}/2 - (A)_0], \text{ etc.}$$

Example VII-1. Naik [214] studied the effect of various types of budding on citrus trees. The particular example chosen to illustrate the analysis for a 2×2 factorial represents two amounts of wood on the bud of acid lime scions in combination with two times of cutting the Kichili orange rootstock arranged in a randomized complete block design. The four treatments are

- 00 = bud inserted with a thin slice of wood adhering to it. The rootstock seedling was lopped off about 3 inches above the bud insertion after the bud had grown 2 inches or more.
- 01 = bud inserted without wood with the same treatment on the rootstock seedling as for treatment 00.
- 10 = bud inserted with thin slice of wood adhering to it. The rootstock seedling was cut off immediately after the bud insertion.
- 11 = the same bud treatment as for 01 and the same rootstock treatment as for 10.

The data presented in table VII-1 are the percentages of success of budding acid lime buds on Kichili orange rootstocks. It will be assumed that all percentages are derived from an equal number of trials[1] and that the value for treatment 11 in block

[1]If the percentages are derived from unequal numbers of observations Cochran [51] discusses the procedure to follow in computing the analysis of variance.

I represents an actual percentage and not a missing plot value; at the conclusion of the present analysis, this value will be estimated and the analysis carried through with the estimated value in the analysis. Also, the percentage data were rounded to whole numbers. The arcsine transformation on the percentages was used [273, table

TABLE VII-1. Percentage "bud-take" of acid lime scion on Kichili orange rootstocks; tables of effects and the analysis of variance

Blocks	Treatments 00 X'	X	01 X'	X	10 X'	X	11 X'	X	Total X
I	64	53.13	23	28.66	30	33.21	0	6.42[a]	121.42
II	75	60.00	14	21.97	50	45.00	33	35.06	162.03
III	76	60.67	12	20.27	41	39.82	17	24.35	145.11
IV	73	58.69	33	35.06	25	30.00	10	18.44	142.19
Total	-	232.49	-	105.96	-	148.03	-	84.27	570.75
Mean	-	58.12	-	26.49	-	37.01	-	21.07	

$$X' = \text{percent};\ X = \sin^{-1}\sqrt{X'}$$

[a]X' taken equal to 100/4(20) = 1.25%

Effects

Effect	Treatment totals $X_{.00}$ = 232.49	$X_{.01}$ = 105.96	$X_{.10}$ = 148.03	$X_{.11}$ = 84.27	Sum of +'s	Sum of -'s	Total for effect
B = Wood vs no wood	+	-	+	-	380.52	190.23	190.29
A = Late vs early lopping	+	+	-	-	338.45	232.30	106.15
Interaction	+	-	-	+	316.76	253.99	62.77
Total	+	+	+	+	570.75	-	570.75

Analysis of variance on transformed values

Source of variation	df	ss	ms
Block	3	208.14	-
Treatment	3	3213.64	-
B = Wood vs no wood on bud	1	2263.14	**
A = Late vs early lopping	1	704.24	-
AB = Interaction	1	246.25	-
Error	9	527.73	58.637
Total	15	3949.51	-
Correction for mean	1	20359.72	-
Total uncorrected	16	24309.23	-

16.8; 102, Ch. 16]. Bartlett suggested that the value $100/2n$ [12] or the value $100/4n$ [14] be substituted for zero percentage. The latter value was added to zero before making the transformation.

The totals, effects, and the analysis of variance are presented in table VII-1. The coefficients in the table of effects are those given by equation (VII-9) multiplied by minus one for the A and B effects. The coefficients for the interaction are the same.

The A effect represents the comparison of early and late lopping or cutting of the rootstock seedling above the bud insertion; the total A effect is $(A) = (A)_0 - (A)_1 = (232.49 + 105.96) - (148.03 + 84.27) = 338.45 - 232.30 = 106.15$. The effect of including wood or no wood with the bud is $(B) = (B)_0 - (B)_1 = 380.52 - 190.23 = 190.29$. The interaction is calculated in a similar manner. It should be pointed out that (A) could have been defined as $(A)_1 - (A)_0$, and then the effect would have a negative sign. Likewise, the treatments could have been redesignated, resulting in a positive A effect. Since the designation is arbitrary, no confusion should result if the main effects are the negative of those given by equation (VII-9). The interpretation is made with this in mind.

The replicate, treatment, and error sums of squares are computed in the usual manner for a randomized complete block design. The treatment sum of squares,

$$\frac{232.49^2 + 105.96^2 + 148.03^2 + 84.27^2}{4} - \frac{570.75^2}{16} = 3213.64,$$

may be partitioned into sums of squares with individual degrees of freedom (equations (VII-10) to (VII-12)). For these data the sums of squares due to the various comparisons are

Wood vs no wood on the bud [*equation (VII-11)*]:

$$\frac{(190.29)^2}{4(1 + 1 + 1 + 1)} = 2263.14 \text{ with one degree of freedom.}$$

Late vs early lopping of rootstock seedling [*equation (VII-10)*]:

$$\frac{(106.15)^2}{4(1 + 1 + 1 + 1)} = 704.24 \text{ with one degree of freedom.}$$

Interaction of time of pruning and amount of wood [*equation (VII-12)*]:

$$\frac{(62.77)^2}{4(4)} = 246.25 \text{ with one degree of freedom.}$$

The sum of the three individual sums of squares $2263.14 + 704.24 + 246.25 = 3213.63$, with $1 + 1 + 1 = 3$ degrees of freedom, is equal to the treatment sum of squares within rounding errors.

We use F to test the hypothesis of zero effects and reject at the 5 per cent level; thus:

$$F = \frac{2263.14}{58.637} = 38.60 > F_{01}(1, 9df),$$

$$F = \frac{704.24}{58.637} = 12.01 > F_{01}(1, 9df),$$

and

$$F = \frac{246.25}{58.637} = 4.20 < F_{05}(1\ 9df) = 5.12.$$

Alternatively, we could use the t test to test the same hypotheses. It is simpler to compute a least significant difference for the effect totals given in the central portion of table VII-1 than to compute the various F values. The standard error of these effect totals is

$$s_t = \sqrt{s^2 r \sum c_i^2} = \sqrt{58.637(4)(1+1+1+1)} = 30.63,$$

and the significant difference at the 5 per cent level is $t_{.05}(9df)s_t = 2.262(30.63) = 69.29$; the c_i are the coefficients for the particular comparison. As before, the A and B effects

TABLE VII-2. Effects and analysis of variance for data of table VII-1 when the value for treatment 11 in block I is estimated

Effects

Effect	Treatment totals $X_{.00}$ = 232.49	$X_{.01}$ = 105.96	$X_{.10}$ = 148.03	$X_{.11}$ = 100.86	Sum of +'s	Sum of −'s	Total for effect
A	+	+	−	−	338.45	248.89	89.56
B	+	−	+	−	380.52	206.82	173.70
AB	+	−	−	+	333.35	253.99	79.36
Total	+	+	+	+	587.34	-	587.34

Analysis of variance

Source of variation	df	ss	ms
Block	3	83.33	27.78
Treatment	3	2780.67	926.89
A	1	501.31	
B	1	1885.73	
AB	1	393.63	
Error	8[a]	372.95	46.62
Total	14[a]	3236.95	-
Correction for mean	1	21560.52	-
Total uncorrected	15[a]	24797.47	-

[a]One value estimated from the data.

exceed the *lsd*, while the interaction effect does not. The confidence intervals on the effects are computed from the effect totals and the *lsd*.

At the beginning of the discussion of this example, it was assumed that the value for treatment 11 in block I was zero. Instead of making this assumption, suppose that we assume that the datum for this particular experimental unit was accidentally lost. Since the design is a randomized complete block design, the missing plot technique in Chapter V may be employed to estimate this value:

$$\hat{X}_{111} = \frac{4(121.42 - 6.42) + 4(84.27 - 6.42) - (570.75 - 6.42)}{(4-1)(4-1)} = 23.01.$$

The value 6.42 is subtracted from each category to obtain the totals without the value 6.42. We insert the value 23.01 in the table of yields and compute the effects and analysis of variance as given in table VII-2. The block I total now becomes 138.01, the total for treatment 11 = $X_{\cdot 11}$ becomes 100.86, and the grand total equals 587.34. All effects exceed the F value at the 5 per cent level. It should be remembered that these tests are slightly biased (see Chapter V).

The computation of a significant difference for the effect totals presents some difficulties, since the contrasts for the effects are no longer independent. The variance of a difference between a treatment mean, say $\bar{x}_{\cdot 11}$, with a missing yield, say $\hat{X}_{111}$, and any other treatment mean, say $\bar{x}_{\cdot 00}$ is:

$$V[(X_{\cdot 11} + \hat{X}_{111})/(r - \bar{x}_{\cdot 00})] = \sigma^2\left\{\frac{rv - r - 1}{r(r-1)(v-1)} + \frac{1}{r} - \frac{2(-1)}{r(r-1)(v-1)}\right\}$$

$$= \sigma^2\left\{\frac{2}{r} + \frac{v}{r(r-1)(v-1)}\right\}. \qquad \text{(VII-14)}$$

The variance of a difference between two treatment means with no missing yields, say $\bar{x}_{\cdot 00}$ and $\bar{x}_{\cdot 10}$, is:

$$V(\bar{x}_{\cdot 00} - \bar{x}_{\cdot 10}) = \sigma^2\left\{\frac{1}{r} + \frac{1}{r} - 2(0)\right\} = 2\sigma^2/r. \qquad \text{(VII-15)}$$

The estimated variance for the main effect total for A = 338.45 is computed as:

$$r^2\hat{\sigma}^2\left\{\frac{3}{r} + \frac{rv - r - 1}{r(r-1)(v-1)} - \frac{2(-1-1-1)}{r(r-1)(v-1)} - 2(0 + 0 + 0)\right\}$$

$$= 16(46.62)\left\{\frac{3}{4} + \frac{12}{36} + \frac{6}{36} = \frac{5}{4}\right\} = 20(46.62) = 932.40;$$

the least significant difference at the 5 per cent level is:

$$2.306\sqrt{932.40} = 70.41.$$

The total effects for B and AB have the same least significant difference.

The means from tables VII-1 and VII-2 are presented graphically in figure VII-4. The responses are presented graphically if the interaction mean square is significantly

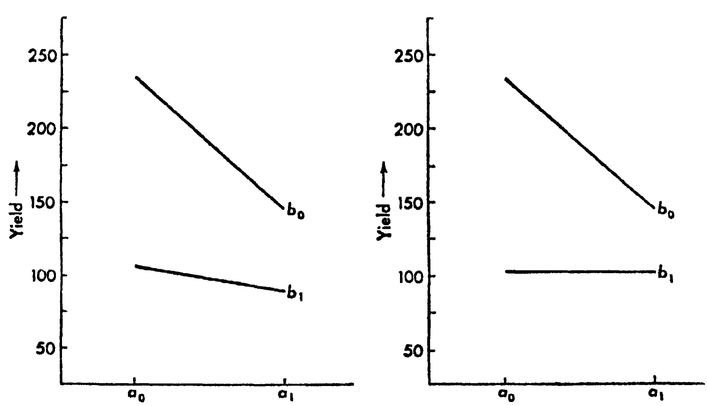


Figure VII-4. Graphical presentation of totals from tables VII-1 and VII-2.

larger than the error mean square or if it is desirable to present a pictorial representation of the response.

VII-4.2 THE 2^3 FACTORIAL

The next factorial arrangement in the 2^n series is the 2^3, which involves three factors, a, b, and c, each at two levels in all combinations. The eight treatments are

$$000,\ 100,\ 010,\ 110,\ 001,\ 101,\ 011,\ 111,$$

and the levels of the main effects and interactions are obtained from the following combination of treatments:

$$\left.\begin{aligned}
(A)_{i=0} &= 000 + 010 + 001 + 011;\\
(A)_{i=1} &= 100 + 110 + 101 + 111;\\
(B)_{j=0} &= 000 + 100 + 001 + 101;\\
(B)_{j=1} &= 010 + 110 + 011 + 111;\\
(AB)_{i+j=0} &= 000 + 110 + 001 + 111;\\
(AB)_{i+j=1} &= 100 + 010 + 101 + 011;\\
(C)_{h=0} &= 000 + 100 + 010 + 110;\\
(C)_{h=1} &= 001 + 101 + 011 + 111;\\
(AC)_{i+h=0} &= 000 + 010 + 101 + 111;\\
(AC)_{i+h=1} &= 100 + 110 + 001 + 011;\\
(BC)_{j+h=0} &= 000 + 100 + 011 + 111;\\
(BC)_{j+h=1} &= 010 + 110 + 001 + 101;\\
(ABC)_{i+j+h=0} &= 000 + 110 + 101 + 011;\\
(ABC)_{i+j+h=1} &= 100 + 010 + 001 + 111.
\end{aligned}\right\} \qquad \text{(VII-16)}$$

The main effects and interactions are the contrasts of two sums; thus:

$$\left.\begin{aligned}
(A) &= (A)_1 - (A)_0;\ A = \frac{1}{4r}\left\{(A)_1 - (A)_0\right\};\\
(B) &= (B)_1 - (B)_0;\ B = \frac{1}{4r}\left\{(B)_1 - (B)_0\right\};\\
(AB) &= (AB)_0 - (AB)_1;\ AB = \frac{1}{4r}\left\{(AB)_0 - (AB)_1\right\};\\
&\ \ \vdots\\
(ABC) &= (ABC)_1 - (ABC)_0;\ ABC = \frac{1}{4r}\left\{(ABC)_1 - (ABC)_0\right\},
\end{aligned}\right\} \qquad \text{(VII-17)}$$

where the levels of the effects are summed over the r replicates.

In a like manner the effects may be obtained from the following table:

Effect	Treatment combinations and totals							
	000	100	010	110	001	101	011	111
	$X_{\cdot 000}$	$X_{\cdot 100}$	$X_{\cdot 010}$	$X_{\cdot 110}$	$X_{\cdot 001}$	$X_{\cdot 101}$	$X_{\cdot 011}$	$X_{\cdot 111}$
Total	+	+	+	+	+	+	+	+
A	−	+	−	+	−	+	−	+
B	−	−	+	+	−	−	+	+
AB	+	−	−	+	+	−	−	+
C	−	−	−	−	+	+	+	+
AC	+	−	+	−	−	+	−	+
BC	+	+	−	−	−	−	+	+
ABC	−	+	+	−	+	−	−	+

where the totals of each column are added or subtracted as indicated. Fcr example,

$$(AC) = X_{\cdot 000} - X_{\cdot 100} + X_{\cdot 010} - X_{\cdot 110} - X_{\cdot 001} + X_{\cdot 101} - X_{\cdot 011} + X_{\cdot 111}$$

$$= (AC)_0 - (AC)_1. \tag{VII-18}$$

The sum of squares for AC is

$$\frac{[(AC)_0 - (AC)_1]^2}{r(1+1+1+1+1+1+1+1)}, \tag{VII-19}$$

where r equals the number of replicates involved in obtaining the totals $X_{\cdot ijk}$. The remaining sums of squares for the effects are obtained similarly.

A third computational method for obtaining the main effects and interactions has been presented by Yates [324]. The steps are presented in the following table:

Treatment	Yield	Col. 1	Col. 2	Col. 3	Effect total
000	$X_{\cdot 000}$	$X_{\cdot 000} + X_{\cdot 100} = s_1$	$s_1 + s_2$	$s_1 + s_2 + s_3 + s_4$	Total $= X_{\cdots\cdot}$
100	$X_{\cdot 100}$	$X_{\cdot 010} + X_{\cdot 110} = s_2$	$s_3 + s_4$	$s_5 + s_6 + s_7 + s_8$	(A)
010	$X_{\cdot 010}$	$X_{\cdot 001} + X_{\cdot 101} = s_3$	$s_5 + s_6$	$s_2 - s_1 + s_4 - s_3$	(B)
110	$X_{\cdot 110}$	$X_{\cdot 011} + X_{\cdot 111} = s_4$	$s_7 + s_8$	$s_6 - s_5 + s_8 - s_7$	(AB)
001	$X_{\cdot 001}$	$X_{\cdot 100} - X_{\cdot 000} = s_5$	$s_2 - s_1$	$s_3 + s_4 - s_1 - s_2$	(C)
101	$X_{\cdot 101}$	$X_{\cdot 110} - X_{\cdot 010} = s_6$	$s_4 - s_3$	$s_7 + s_8 - s_5 - s_6$	(AC)
011	$X_{\cdot 011}$	$X_{\cdot 101} - X_{\cdot 001} = s_7$	$s_6 - s_5$	$s_4 - s_3 - s_2 + s_1$	(BC)
111	$X_{\cdot 111}$	$X_{\cdot 111} - X_{\cdot 011} = s_8$	$s_8 - s_7$	$s_8 - s_7 - s_6 + s_5$	(ABC)

The sum of the items in a row of column 3 yields the effect totals listed in the last column. The same combinations of treatments for the effects are used as in the two preceding computational methods.

The plus and minus signs in the above table may be obtained from the following algebraic quantities [126, sec. 43; 324, p. 12]:

$$\left.\begin{aligned}
(A) &= (a-1)(b+1)(c+1) = -1+a-b+ab-c+ac-bc+abc \\
(B) &= (a+1)(b-1)(c+1) = -1-a+b+ab-c-ac+bc+abc \\
(AB) &= (a-1)(b-1)(c+1) = 1-a-b+ab+c-ac-bc+abc \\
&\cdot \\
&\cdot \\
&\cdot \\
(AC) &= (a-1)(b+1)(c-1) = 1-a+b-ab-c+ac-bc+abc \\
&\cdot \\
&\cdot \\
&\cdot \\
(ABC) &= (a-1)(b-1)(c-1) = -1+a+b-ab+c-ac-bc+abc,
\end{aligned}\right\} \qquad \text{(VII-20)}$$

where $1 = X_{\cdot 000}$, $a = X_{\cdot 100}$, $b = X_{\cdot 010}$, $ab = X_{\cdot 110}$, $c = X_{\cdot 001}$, $ac = X_{\cdot 101}$, $bc = X_{\cdot 011}$, and $abc = X_{\cdot 111}$.

The standard error for any effect total is $\sqrt{rs^2(1+1+1+1+1+1+1+1)} = 2s\sqrt{2r}$, where s^2 equals the error mean square for the particular experimental design used. The standard error for a mean effect is $s/\sqrt{2r}$.

The treatment totals may be obtained from the effects or from the levels of the effects in a manner similar to that for the 2×2 factorial;

$$\begin{aligned}
X_{\cdot ijh} &= \frac{1}{8}\Big\{X_{\cdots\cdot} + (-1)^{i-1}(A) + (-1)^{j-1}(B) + (AB)(-1)^{i+j} + (C)(-1)^{h-1} \\
&\quad + (AC)(-1)^{i+h} + (BC)(-1)^{j+h} + (ABC)(-1)^{i+j+h-1}\Big\} \\
&= \frac{1}{4}\Big\{(A)_i + (B)_j + (AB)_{i+j} + (C)_h + (AC)_{i+h} + (BC)_{j+h} \\
&\quad + (ABC)_{i+j+h} - 3X_{\cdots\cdot}\Big\}
\end{aligned} \qquad \text{(VII-21)}$$

Example VII-2. An experiment was conducted to study the effectiveness, use, and preparation of stereophotographic aids in engineering education [282]. Only a portion of the experiment is used to illustrate the analysis for a 2^3 factorial. The part of the experiment reproduced (table VII-3) is on the portion dealing with non-stereophotographs.

In setting up the experiment, Straub [282] obtained twenty-four senior and twenty-four (actually twenty-eight, but four were excluded at random in order to have equal numbers in the analysis) freshman students. In each class the students were divided into four groups of six students each. The groups were "equated" or "balanced" on the basis of six characteristics. There were no apparent relationships (all values of the correlation coefficient are not significant at the 5 per cent level) between any of the six characters used for equating the groups and the percentage of correct answers. Hence, for our purposes, it is assumed that the six students to receive each treatment were randomly selected and the design is a completely randomized one. The practice of "equating" or "balancing" the subgroups is not a recommended procedure for the reasons set forth in Chapter I.

The three factors are class = c, picture and question set = b, and position of exposure = a. The freshman group is designated as c_0 and the seniors as c_1. The b_0 group comprised a set of pictures on which twenty-seven questions were asked, and the b_1 group was composed of another set of pictures upon which eighteen questions

TABLE VII-3. Original and transformed data for the 2^3 factorial

Treatments															
000		100		010		110		001		101		011		111	
Number of questions[a]															
R	W	R	W	R	W	R	W	R	W	R	W	R	W	R	W
10	17	10	17	2	16	10	8	6	21	4	23	6	12	9	9
10	17	6	21	1	17	0	18	5	22	8	19	11	7	5	13
6	21	10	17	5	13	4	14	7	20	9	18	5	13	11	7
12	15	5	22	10	8	4	14	12	15	11	16	7	11	8	10
8	19	2	25	8	10	6	12	11	16	6	21	2	16	8	10
5	22	7	20	3	15	6	12	8	19	5	22	6	12	9	9

Arcsine transformation of percentage of questions answered correctly

	37.5	37.5	19.5	48.2	28.1	22.6	35.3	45.0	
	37.5	28.1	13.6	6.8[b]	25.5	33.0	51.4	31.8	
	28.1	37.5	31.8	28.1	30.6	35.3	31.8	51.4	
	41.8	25.5	48.2	28.1	41.8	39.7	38.5	41.8	
	33.0	15.8	41.8	35.3	39.7	28.1	19.5	41.8	
	25.5	30.6	24.1	35.3	33.0	25.5	35.3	45.0	
Sum	203.4	175.0	179.0	181.8	198.7	184.2	211.8	256.8	1590.7
Mean	33.90	29.17	29.83	30.30	33.12	30.70	35.30	42.80	33.14

[a]R = number of questions answered correctly; W = number of questions answered incorrectly.

[b]Zero percent correct taken as 100/4(18) = 1.39% [12, 14, 102, Ch. 16].

Total main effects and interactions for transformed data

	$X_{.000}$ 203.4	$X_{.100}$ 175.0	$X_{.010}$ 179.0	$X_{.110}$ 181.8	$X_{.001}$ 198.7	$X_{.101}$ 184.2	$X_{.011}$ 211.8	$X_{.111}$ 256.8	Sum of +'s	Sum of -'s	Total effect
A	-	+	-	+	-	+	-	+	797.8	792.9	4.9
B	-	-	+	+	-	-	+	+	829.4	761.3	68.1
AB	+	-	-	+	+	-	-	+	840.7	750.0	90.7
C	-	-	-	-	+	+	+	+	851.5	739.2	112.3
AC	+	-	+	-	-	+	-	+	823.4	767.3	56.1
BC	+	+	-	-	-	-	+	+	847.0	743.7	103.3
ABC	-	+	+	-	+	-	-	+	809.5	781.2	28.3
Total	+	+	+	+	+	+	+	+	1590.7	0	1590.7

were asked. In order to obtain a valid comparison with stereo pictures, an exposure of the picture was made a short distance to the left = a_0 and also a short distance to the right = a_1 of the stereo picture; this picture was taken with non-stereophotographic equipment.

The $2^3 = 8$ treatments are

000 =	left exposure,	photo	and	question	set	I,	freshman;
100 =	right " ,	"	"	"	"	I,	" ;
010 =	left " ,	"	"	"	"	II,	" ;
110 =	right " ,	"	"	"	"	II,	" ;
001 =	left " ,	"	"	"	"	I,	senior;
101 =	right " ,	"	"	"	"	I,	" ;
011 =	left " ,	"	"	"	"	II,	" ;
111 =	right " ,	"	"	"	"	II,	" .

Since the data are in two categories, numbers of questions answered correctly and incorrectly, it appears that the percentage of correct answers *might* be binomial in nature; this would indicate that an arcsine transformation of the data might be appropriate. The situation is complicated by the fact that the percentages are derived from unequal numbers [51]. The problem of appropriate weights presents some difficulties. In this connection, however, the only comparison derived from an unequal number of questions is the B effect. In order to make a start on this problem, the percentage of correct answers was transformed to angles by the arcsine transformation [273, table 16.8]. The individual variances on each group of students on the eight treatments were computed (table VII-4). The chi-square test for homogeneity of variances [14; Chapter IV] was not unusually large ($P \doteq .3$). However, the average variance, 127.9, for the four groups answering eighteen questions was significantly higher than the average variance, 47.1, for the remaining four groups which answered twenty-seven questions. If binomial variance is assumed, the latter variance is much closer to its theoretical variance [51, 81; 102, Ch. 16], $821/27 = 30.4$, than is the former one to its theoretical variance, $821/18 = 45.6$. Owing to the fact that the variation among the individual subgroup variances was not unusually large, and because the magnitudes of the variances for the same groups were reversed in an experiment on stereo pictures (see problem VII-2), it will be assumed that the pooled within-group mean square may justifiably be used to compare the various contrasts. It is realized that more study of these data is required. If the errors going into the B contrast, say, are considered to be different, a modification of the error degrees of freedom is necessary (Chapter V).

The main effect and interaction totals in table VII-3 are obtained by summing the plus values and subtracting the sum of the quantities with minus signs (equation (VII-17)). For example,

$$(A) = (A)_1 - (A)_0 = 797.8 - 792.9 = 4.9,$$
$$(AB) = (AB)_0 - (AB)_1 = 840.7 - 750.0 = 90.7,$$

and

$$(ABC) = (ABC)_1 - (ABC)_0 = 809.5 - 781.2 = 28.3.$$

The sums of squares in table VII-4 for treatments, total, and within treatments are computed in the manner described for a completely randomized design (Chapter IV). The treatment sum of squares with 7 degrees of freedom is partitioned into seven independent sums of squares, each with a single degree of freedom. From formulae similar to (VII-19) the sum of squares for the A effect is $4.9^2/8(6) = 0.500$, for the B effect is $68.1^2/48 = 96.617$, for the AB effect is $90.7^2/48 = 171.385$, etc.

The various mean squares may be compared with the error mean square, 87.514, to test the hypothesis of zero effects. None of the effects exceeds the 5 per cent level of F. The class difference exceeds F at the 10 per cent level. However, this is about what would be expected in making multiple F tests; 10 per cent, or one of the effects, would be expected to exceed F_{10} on the average. This point should be kept in mind when making F tests of a large number of hypotheses [see 134].

The standard error of an effect total is $\sqrt{87.514(6)(8)} = 64.81$, and the significant difference at the 5 per cent level is $2.021(64.81) = 131.0$, where 2.021 is the tabulated

TABLE VII-4 Analysis of variance for transformed data from table VII-3

Source of variation	df	ss	ms
Treatment	7	835.80	119.40
A = Exposure	1	0.500	
B = Set	1	96.617	
AB = Exposure x set	1	171.385	
C = Class	1	262.735	
AC = Class x exposure	1	65.567	
BC = Class x set	1	222.310	
ABC = Class x set x exposure	1	16.685	
Among boys within treatments	40	3500.55	87.514
Within 000	5	193.34	38.67
" 100	5	334.19	66.84
" 010	5	887.57	177.51
" 110	5	932.34	186.47
" 001	5	208.27	41.65
" 101	5	206.86	41.37
" 011	5	531.34	106.27
" 111	5	206.64	41.33
Total	47	4336.35	-
Correction for mean	1	52715.14	-
Total uncorrected	48	57051.49	-

value of t with 40 degrees of freedom at the 5 per cent level. The 95 per cent confidence intervals are the effect totals plus and minus the value 131.0. The coefficient of variation is $\sqrt{87.514}/33.14 = 28$ per cent. With such a high degree of variation, it might be profitable to study methods of reducing the variation. Perhaps more questions are needed. Also, it may be possible to stratify the students into homogeneous subgroups and use the randomized complete block design. The characteristics used to equate the groups did not appear to be of much value for stratification purposes. Hence, others need to be studied.

If desired the treatment totals may be computed from the effects or the levels of various effects. Applying formula (VII-21), the total for treatment 001 is

$$X_{\cdot 001} = \frac{1}{4}\{792.9 + 761.3 + 840.7 + 851.5 + 767.3 + 743.7 + 809.5 - 3(1590.7)\}$$

$$= 198.7.$$

The other treatment totals may be computed similarly if desired. This method of obtaining treatment totals is used for incomplete block designs (see later chapters) but it is not ordinarily used to obtain treatment totals.

VII-4.3 THE 2^4 FACTORIAL

The 2^4 factorial arrangement of treatments involves four factors, a, b, c, and d, each at two levels. The sixteen treatments are designated as

$$\begin{array}{rrrr}
(1) = 0000 & c = 0010 & d = 0001 & cd = 0011 \\
a = 1000 & ac = 1010 & ad = 1001 & acd = 1011 \\
b = 0100 & bc = 0110 & bd = 0101 & bcd = 0111 \\
ab = 1100 & abc = 1110 & abd = 1101 & abcd = 1111.
\end{array}$$

The letters are the symbols used for the various combinations by a number of authors [60, 273, 324]. The levels of the effects are equal to the following combination of treatments $a_ib_jc_hd_f = ijhf$:

$$\left.\begin{aligned}
(A)_{i=0} &= 0000 + 0100 + 0010 + 0110 + 0001 + 0101 \\
&\quad + 0011 + 0111; \\
(A)_{i=1} &= 1000 + 1100 + 1010 + 1110 + 1001 + 1101 \\
&\quad + 1011 + 1111; \\
&\cdot \\
&\cdot \\
&\cdot \\
(AC)_{i+h=0} &= 0000 + 0100 + 1010 + 1110 + 0001 + 0101 \\
&\quad + 1011 + 1111; \\
(AC)_{i+h=1} &= 1000 + 1100 + 0010 + 0110 + 1001 + 1101 \\
&\quad + 0011 + 0111; \\
&\cdot \\
&\cdot \\
&\cdot \\
(BCD)_{j+h+f=0} &= 0000 + 1000 + 0110 + 1110 + 0101 + 1101 \\
&\quad + 0011 + 1011; \\
(BCD)_{j+h+f=1} &= 0100 + 1100 + 0010 + 1010 + 0001 + 1001 \\
&\quad + 0111 + 1111; \\
(ABCD)_{i+j+h+f=0} &= 0000 + 1100 + 1010 + 0110 + 1001 + 0101 \\
&\quad + 0011 + 1111; \\
(ABCD)_{i+j+h+f=1} &= 1000 + 0100 + 0010 + 1110 + 0001 + 1101 \\
&\quad + 1011 + 0111.
\end{aligned}\right\} \quad \text{(VII-22)}$$

An effect is obtained as the difference between the levels of an effect; thus:

$$\begin{aligned}
(A) &= (A)_1 - (A)_0;\ A = \{(A)_1 - (A)_0\}/8r; \\
&\vdots \\
(AB) &= (AB)_0 - (AB)_1;\ AB = \{(AB)_0 - (AB)_1\}/8r; \\
&\vdots \\
(ACD) &= (ACD)_1 - (ACD)_0;\ ACD = (ACD)/8r; \\
&\vdots \\
(ABCD) &= (ABCD)_0 - (ABCD)_1;\ ABCD = (ABCD)/8r.
\end{aligned} \tag{VII-23}$$

Alternatively, the effects may be obtained from a table of plus and minus signs (table VII-5).

The fifteen treatment degrees of freedom may be partitioned into fifteen independent comparisons with single degrees of freedom; thus, for r replicates, this part of the analysis of variance is

Source of variation	Degrees of freedom	Sum of squares
A	1	$[(A)_1 - (A)_0]^2/16r$
B	1	$[(B)_1 - (B)_0]^2/16r$
AB	1	$[(AB)_0 - (AB)_1]^2/16r$
C	1	$[(C)_1 - (C)_0]^2/16r$
AC	1	$[(AC)_0 - (AC)_1]^2/16r$
BC	1	$[(BC)_0 - (BC)_1]^2/16r$
ABC	1	$[(ABC)_1 - (ABC)_0]^2/16r$
D	1	$[(D)_1 - (D)_0]^2/16r$
AD	1	$[(AD)_0 - (AD)_1]^2/16r$
BD	1	$[(BD)_0 - (BD)_1]^2/16r$
ABD	1	$[(ABD)_1 - (ABD)_0]^2/16r$
CD	1	$[(CD)_0 - (CD)_1]^2/16r$
ACD	1	$[(ACD)_1 - (ACD)_0]^2/16r$
BCD	1	$[(BCD)_1 - (BCD)_0]^2/16r$
ABCD	1	$[(ABCD)_0 - (ABCD)_1]^2/16r$

The standard error for the effect totals in the right-hand column of table VII-5 is $\sqrt{rs^2(16)}$, where s^2 is the experimental error variance. The total for a particular treatment may be obtained from the effect totals or from the levels of the various main effects and interactions. The formula is similar to formula (VII-21).

Factorials of the 2^n series for n larger than four may be obtained in a manner similar to that described for the 2^2, 2^3, and 2^4 factorials.

TABLE VII-5. Coefficients for totals $X_{.ijhf}$ for total effects of a 2^4 factorial

Effect	(1) $X_{.0000}$	a $X_{.1000}$	b $X_{.0100}$	ab $X_{.1100}$	c $X_{.0010}$	ac $X_{.1010}$	bc $X_{.0110}$	abc $X_{.1110}$	d $X_{.0001}$	ad $X_{.1001}$	bd $X_{.0101}$	abd $X_{.1101}$	cd $X_{.0011}$	acd $X_{.1011}$	bcd $X_{.0111}$	abcd $X_{.1111}$	Total for effect
A	-	+	-	+	-	+	-	+	-	+	-	+	-	+	-	+	(A)
B	-	-	+	+	-	-	+	+	-	-	+	+	-	-	+	+	(B)
AB	+	-	-	+	+	-	-	+	+	-	-	+	+	-	-	+	(AB)
C	-	-	-	-	+	+	+	+	-	-	-	-	+	+	+	+	(C)
AC	+	-	+	-	-	+	-	+	+	-	+	-	-	+	-	+	(AC)
BC	+	+	-	-	-	-	+	+	+	+	-	-	-	-	+	+	(BC)
ABC	-	+	+	-	+	-	-	+	-	+	+	-	+	-	-	+	(ABC)
D	-	-	-	-	-	-	-	-	+	+	+	+	+	+	+	+	(D)
AD	+	-	+	-	+	-	+	-	-	+	-	+	-	+	-	+	(AD)
BD	+	+	-	-	+	+	-	-	-	-	+	+	-	-	+	+	(BD)
ABD	-	+	+	-	-	+	+	-	+	-	-	+	+	-	-	+	(ABD)
CD	+	+	+	+	-	-	-	-	-	-	-	-	+	+	+	+	(CD)
ACD	-	+	-	+	+	-	+	-	+	-	+	-	-	+	-	+	(ACD)
BCD	-	-	+	+	+	+	-	-	+	+	-	-	-	-	+	+	(BCD)
ABCD	+	-	-	+	-	+	+	-	-	+	+	-	+	-	-	+	(ABCD)
Total	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	Total

VII-4.4 GENERALIZED INTERACTION IN THE 2^n SERIES

The concept of a generalized interaction has been discussed in detail by Yates [324] and by Fisher [126, sec. 45.1]. As illustrated earlier the interaction of A and B is AB. Likewise, the interaction of A and BC is ABC, and of A and AB is B. The latter example $A \times AB = A^2B = B$, reduced modulo two, illustrates the fact that 0 and 1 are the only elements of this number system and that $A^2 = A^0 = 1$, since any number to the zero power is unity. Some further examples will illustrate the method and principles involved:

$$\begin{aligned}
&ABC \times BCD = AD;\\
&ABC \times DEF = ABCDEF;\\
&ADE \times AFG = DEFG;\\
&BDF \times BCFG = CDG;\\
&BDF \times ABC = ACDF;\\
&AB \times BC = AC;\\
&C \times BCDF = BDF;\\
&ABCD \times ABC = D.
\end{aligned}$$

The concept of generalized interactions is useful for clarifying the construction of various incomplete block designs.

VII-5 The Factorial Experiment—3^n Series

The 3^n series is constructed in much the same way as the 2^n. There are n factors each at three levels in a 3^n factorial arrangement. The number system here is $0 = 3 = 6 = \cdots, 1 = 4 = 7 = \cdots, 2 = 5 = 8 = \cdots$.

VII-5.1 THE 3^2 FACTORIAL

For the 3^2 factorial the factors a and b are at the 0, 1, and 2 levels and are in all possible combinations; this results in the nine treatments: 00, 01, 02, 10, 11, 12, 20, 21, 22. The 0, 1, and 2 levels of effects A and B are

$$\left.\begin{aligned}
(A)_0 &= 00 + 01 + 02;\\
(A)_1 &= 10 + 11 + 12;\\
(A)_2 &= 20 + 21 + 22;\\
(B)_0 &= 00 + 10 + 20;\\
(B)_1 &= 01 + 11 + 21;\\
(B)_2 &= 02 + 12 + 22.
\end{aligned}\right\} \qquad \text{(VII-24)}$$

The comparison among the three levels of the factors A or B from r replicates yields a sum of squares with 2 degrees of freedom,

$$\frac{(A)_0^2 + (A)_1^2 + (A)_2^2}{3r} - \frac{[(A)_0 + (A)_1 + (A)_2]^2}{9r} \qquad \text{(VII-25)}$$

or

$$\frac{(B)_0^2 + (B)_1^2 + (B)_2^2}{3r} - \frac{[(B)_0 + (B)_1 + (B)_2]^2}{9r}. \quad \text{(VII-26)}$$

Among the nine treatments, there are 8 degrees of freedom; thus, there are 4 degrees of freedom for the interaction of the factors a and b (denoted as $A \times B$). It is possible to partition these 4 degrees of freedom into two separate portions, which, in conformity with the code given below, are denoted as $AB(= J$ effect in Yates' notation) and $AB^2(= I$ effect in Yates' notation). This partitioning may have little or no meaning experimentally but is important in constructing incomplete block experiments. The three levels of AB and AB^2 are [324, p. 40]

$$\begin{aligned}
(AB)_{i+j=0} &= 00 + 12 + 21 = [J_1];\\
(AB)_{i+j=1} &= 01 + 10 + 22 = [J_2];\\
(AB)_{i+j=2} &= 02 + 11 + 20 = [J_3];\\
(AB^2)_{i+2j=0} &= 00 + 11 + 22 = [I_1];\\
(AB^2)_{i+2j=1} &= 02 + 10 + 21 = [I_2];\\
(AB^2)_{i+2j=2} &= 01 + 12 + 20 = [I_3];
\end{aligned} \quad \text{(VII-27)}$$

since the notation is reduced modulo three, and $0 = 3$, $1 = 4$, and $2 = 5$.

The first letter in the interaction has the coefficient of unity and the second letter has either unity or $2 = 3 - 1$. This is done to obtain $p + 1$ independent and unique effects. For example, the three levels of the effect A^2B are composed of the same treatments as are the levels of $AB^2 = (AB^2)^2 = A^2B$, reduced modulo three. The notation is merely an extension of that used for the 2^n series where the partitioning of the treatment sum of squares is in units of $(p - 1) = 2 - 1 =$ single degrees of freedom. For the 3^n series the partitioning of the treatment sum of squares is in groups of $3 - 1 = 2$ degrees of freedom. This system may be extended for $p = 5, 7, 11, 13$, etc. [173, 175, 177].

As in the 2^n series the total for treatment ij from a 3×3 factorial may be obtained from the formula,

$$\bullet \quad X_{\cdot ij} = \frac{(A)_i + (B)_j + (AB)_{i+j} + (AB^2)_{i+2j}}{3} - \frac{3X_{\cdots}}{9}, \quad \text{(VII-28)}$$

which involves levels of the main effects and interactions.

Example VII-3. Mahalanobis [203] presents a numerical example of three rice varieties in combination with no inorganic fertilizer and two inorganic fertilizers, ammophos at 40.5 pounds per acre and ammonium sulphate at 30.4 pounds per acre. A randomized complete block design with six replicates of the nine 1/93 acre = 12′

$\times$ 39′ plots was used. The yields of the plots are given in chattaks (Indian unit of measure) per plot (table VII-6).

TABLE VII-6. Yield of grain from paddy grown in 39′ $\times$ 12′ plots (1/93 acre) for 9 treatments

Treatment		Block						Total
Variety	Fertilizer	I	II	III	IV	V	VI	
Red Aus	Ammophos	112	128	118	128	92	152	730
	Ammonium sulphate	108	116	144	100	100	80	708
	Control	106	84	68	156	156	128	698
Kashiphul	Ammophos	112	81	108	96	53	48	498
	Ammonium sulphate	61	98	58	86	65	98	466
	Control	97	86	92	80	99	66	520
Dudkalma	Ammophos	134	112	115	114	101	128	705
	Ammonium sulphate	125	106	110	102	56	110	609
	Control	62	60	99	90	58	87	456
Total		977	871	913	952	780	897	5390

	b_0 Red Aus	b_1 Kashiphul	b_2 Dudkalma	Total	Mean
Ammophos = a_2	730	498	705	1933	107.4
Ammonium sulphate = a_1	708	466	609	1783	99.1
Control (no fertilizer) = a_0	698	520	456	1674	93.0
Total	2136	1484	1770	5390	-
Mean	118.7	82.4	98.3	-	99.3

Analysis of variance

Source of variation	df	ss	ms
Replicate	5	2670.5	534.1
Treatment	8	17459.8	2182.5
A = fertilizers	2	1878.9	939.4
B = varieties	2	11867.7	5933.8
A x B	4	3713.2	928.3
Error	40	22713.8	567.84
Total	53	42844.1	-
Correction for mean	1	538001.9	-
Total uncorrected	54	580846	-

Before computing the analysis of variance table, it is necessary to obtain the replicate, treatment, and grand totals, and to construct a table of variety and fertilizer totals. The sums of squares for treatments, replicates, and total are computed in the

manner set forth for the randomized complete block design (see Chapter V). The fertilizer (the a factor) sum of squares is obtained from formula (VII-25) as

$$\frac{1933^2 + 1783^2 + 1674^2}{3(6)} - \frac{5390^2}{54} = 1878.9 \text{ with } 2df.$$

The variety sum of squares (formula (VII-26)) is

$$\frac{2136^2 + 1484^2 + 1770^2}{3(6)} - \frac{5390^2}{54} = 11867.7 \text{ with } 2df.$$

The variety × fertilizer interaction, $A \times B$, is usually obtained by subtracting the above two sums of squares from the treatment sum of squares, 17459.8 − 1878.9 − 11867.7 = 3713.2 with 4 degrees of freedom. This sum of squares may be computed directly from the sum of squares for the two components AB and AB^2; thus:

$$\frac{1}{3r}\left\{\sum_{u=0}^{2}(AB)_u^2 + \sum_{u=0}^{2}(AB^2)_u^2\right\} - \frac{2X_{\cdots}^2}{9r} \tag{VII-29}$$

$$= \frac{1}{18}\Big\{(498 + 609 + 698)^2 + (705 + 708 + 520)^2$$

$$+ (730 + 466 + 456)^2 + (698 + 466 + 705)^2 + (498 + 708 + 456)^2$$

$$+ (730 + 520 + 609)^2\Big\} - \frac{2(5390)^2}{54}$$

$$= 540201.0 + 539515.9 - 2(538001.9)$$

$$= 3713.1 \text{ with } 2 + 2 = 4 \text{ degrees of freedom.}$$

Since the two sums of squares are considered to be estimates of the same quantity, the results are pooled in the analysis of variance table.

Making use of the F test it is seen that the treatment mean square is significantly larger than the error mean square, $F = 2182.5/567.84 = 3.84 > F_{.05}(8,40df)$. Partitioning of the treatment sum of squares into its component parts indicates that the variation among varieties accounts for the major portion of the treatment sum of squares.

The standard error of a variety or fertilizer mean is $\sqrt{567.84/6(3)} = 5.62$. The standard error of the corresponding total is $\sqrt{567.84(6)(3)} = 101.10$. The corresponding standard errors of a difference are the above standard errors multiplied by $\sqrt{2}$. The coefficient of variation equals $\sqrt{567.84}/99.8 = 24$ per cent. The confidence intervals are computed as described in section II-1.1.4.

The analysis of the above factorial has not been completed. We might wish to know the reaction of the two kinds of fertilizers with varieties, the comparison between the two kinds of fertilizers, a_1 and a_2, the contrast of the two nitrogen fertilizers with the check, and the interaction of this contrast with varieties. The sums and totals necessary to obtain the sums of squares for the above contrasts are presented in

table VII-7. The first table of totals consists of computing the quantity $a_2 - a_1$ for each replicate and for each variety. The first figure is $-56 = 112 - 168$, the second figure is $12 = 128 - 116$, etc. The totals are summed algebraically to obtain the variety and replicate totals. The second table of totals is computed as $a_2 + a_1 - 2a_0$. The first value is $112 + 168 - 2(106) = 68$, etc. The variety and replicate totals are obtained. The differences between fertilizer totals are equal to the totals of these

TABLE VII-7. Comparisons among fertilizers

Variety	Blocks I	II	III	IV	V	VI	Total
			$(a_2 - a_1)$				
Red Aus	-56	12	-26	28	- 8	72	22
Kashiphul	51	-17	50	10	-12	-50	32
Dudkalma	9	6	6	12	45	18	96
Total	4	1	30	50	25	40	150
			$(a_2 + a_1 - 2a_0)$				
Red Aus	68	76	126	-84	-120	-24	42
Kashiphul	-21	7	-18	22	- 80	14	-76
Dudkalma	135	98	28	36	41	64	402
Total	182	181	136	-26	-159	54	368

Analysis of variance

Source of variation	df	ss	ms
Fertilizers	2	1878.9	-
$a_2 - a_1$	1	625.0	-
$a_2 + a_1 - 2a_0$	1	1253.9	-
A x B	4	3713.2	928.3
$(a_2 - a_1)$ x var.	2	268.7	134.35
$(a_2 + a_1 - 2a_0)$ x var.	2	3444.5	1722.25
Error	40	22713.8	567.84
Var. x rep.	10	2607.2	-
Fert. x rep.	10	5353.4	-
$(a_2 - a_1)$ x var. x rep.	10	9175.0	-
$(a_2 + a_1 - 2a_0)$ x var. x rep.	10	5578.3	-

quantities in the two tables; thus, $1933 - 1783 = 150$, and $1933 + 1783 - 2(1674) = 368$. The sum of the sums of squares of these two quantities equals the sum of squares for fertilizers; thus, $150^2/(1 + 1)(6)(3) + 368^2/6(3)(1 + 1 + 4) = 625.0$

+ 1253.9 = 1878.9. Likewise, the totals for varieties may be used to compute the interaction of the two contrasts with varieties,

$$\frac{22^2 + 32^2 + 96^2}{6(1 + 1)} - \frac{150^2}{36} + \frac{42^2 + (-76)^2 + 402^2}{6(1 + 1 + 4)} - \frac{368^2}{108}$$

$$= 268.7 + 3444.5 = 3713.2.$$

As a further investigation, it was decided to compare the interaction of the various contrasts with replicates. The four mean squares are not different, although there is a slight indication that the main effects by replicates mean squares are somewhat less variable than the interaction by replicate mean square, $A \times B \times$ replicates. However, this may be a fortuitous event.

The nitrogen fertilizers yield higher than no fertilizer, although not significantly higher, and the interaction of this contrast with varieties is close to significance at the 5 per cent level; thus:

$$F = 1722.25/576.84 = 3.03 < F_{05}(2, 40df) = 3.23.$$

The interaction of fertilizer versus no fertilizer with variety is presented graphically in figure VII-5.

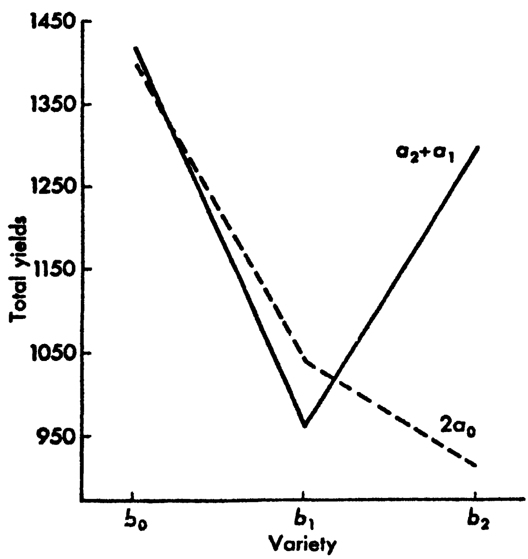


Figure VII-5. Interaction of the contrast $a_2 + a_1 - 2a_0$ with variety.

In some experiments it may be desirable to partition the 2 degrees of freedom for A and (or) for B into linear and quadratic effects (A_L, B_L, A_Q, and B_Q) as described

by Snedecor [273, Ch. 15]. The following table of plus and minus coefficients for single degree of freedom comparisons in a 3 × 3 factorial has been prepared for this purpose:

Effect	Treatment totals									Divisors
	$X_{\cdot 00}$	$X_{\cdot 01}$	$X_{\cdot 02}$	$X_{\cdot 10}$	$X_{\cdot 11}$	$X_{\cdot 12}$	$X_{\cdot 20}$	$X_{\cdot 21}$	$X_{\cdot 22}$	
A_L	−	−	−	0	0	0	+	+	+	6r
A_Q	+	+	+	−2	−2	−2	+	+	+	18r
B_L	−	0	+	−	0	+	−	0	+	6r
B_Q	+	−2	+	+	−2	+	+	−2	+	18r
$A_L \times B_L$	+	0	−	0	0	0	−	0	+	4r
$A_L \times B_Q$	−	+2	−	0	0	0	+	−2	+	12r
$A_Q \times B_L$	−	0	+	+2	0	−2	−	0	+	12r
$A_Q \times B_Q$	+	−2	+	−2	+4	−2	+	−2	+	36r

VII-5.2 THE 3^3 FACTORIAL

It may be desired to test three factors a, b, and c in all combinations of three levels per factor. The resulting twenty-seven treatments may be included in a randomized complete block or some other design. The treatment combinations are

000	001	002	010	011	012	020	021	022
100	101	102	110	111	112	120	121	122
200	201	202	210	211	212	220	221	222

The levels of the effects in terms of the treatment combinations are

$$(A)_0 = 000 + 001 + 002 + 010 + 011 + 012 + 020 + 021 + 022;$$
$$(A)_1 = 100 + 101 + 102 + 110 + 111 + 112 + 120 + 121 + 122;$$
$$(A)_2 = 200 + 201 + 202 + 210 + 211 + 212 + 220 + 221 + 222;$$
$$(B)_0 = 000 + 001 + 002 + 100 + 101 + 102 + 200 + 201 + 202;$$
$$(B)_1 = 010 + 011 + 012 + 110 + 111 + 112 + 210 + 211 + 212;$$
$$(B)_2 = 020 + 021 + 022 + 120 + 121 + 122 + 220 + 221 + 222;$$
$$(AB)_0 = 000 + 001 + 002 + 120 + 121 + 122 + 210 + 211 + 212;$$
$$(AB)_1 = 010 + 011 + 012 + 100 + 101 + 102 + 220 + 221 + 222;$$
$$(AB)_2 = 020 + 021 + 022 + 110 + 111 + 112 + 200 + 201 + 202;$$
$$(AB^2)_0 = 000 + 001 + 002 + 110 + 111 + 112 + 220 + 221 + 222;$$
$$(AB^2)_1 = 020 + 021 + 022 + 100 + 101 + 102 + 210 + 211 + 212;$$
$$(AB^2)_2 = 010 + 011 + 012 + 120 + 121 + 122 + 200 + 201 + 202;$$
$$(C)_0 = 000 + 010 + 020 + 100 + 110 + 120 + 200 + 210 + 220;$$
$$(C)_1 = 001 + 011 + 021 + 101 + 111 + 121 + 201 + 211 + 221;$$
$$(C)_2 = 002 + 012 + 022 + 102 + 112 + 122 + 202 + 212 + 222;$$
$$(AC)_0 = 000 + 010 + 020 + 102 + 112 + 122 + 201 + 211 + 221;$$
$$(AC)_1 = 001 + 011 + 021 + 100 + 110 + 120 + 202 + 212 + 222;$$
$$(AC)_2 = 002 + 012 + 022 + 101 + 111 + 121 + 200 + 210 + 220;$$
$$(AC^2)_0 = 000 + 010 + 020 + 101 + 111 + 121 + 202 + 212 + 222;$$
$$(AC^2)_1 = 002 + 012 + 022 + 100 + 110 + 120 + 201 + 211 + 221;$$

$$
\begin{aligned}
(AC^2)_2 &= 001 + 011 + 021 + 102 + 112 + 122 + 200 + 210 + 220;\\
(BC)_0 &= 000 + 012 + 021 + 100 + 112 + 121 + 200 + 212 + 221;\\
(BC)_1 &= 001 + 010 + 022 + 101 + 110 + 122 + 201 + 210 + 222;\\
(BC)_2 &= 002 + 011 + 020 + 102 + 111 + 120 + 202 + 211 + 220;\\
(BC^2)_0 &= 000 + 011 + 022 + 100 + 111 + 122 + 200 + 211 + 222;\\
(BC^2)_1 &= 002 + 010 + 021 + 102 + 110 + 121 + 202 + 210 + 221;\\
(BC^2)_2 &= 001 + 012 + 020 + 101 + 112 + 120 + 201 + 212 + 220;\\
(ABC)_0 &= 000 + 012 + 021 + 102 + 111 + 120 + 201 + 210 + 222;\\
(ABC)_1 &= 001 + 010 + 022 + 100 + 112 + 121 + 202 + 211 + 220;\\
(ABC)_2 &= 002 + 011 + 020 + 101 + 110 + 122 + 200 + 212 + 221;\\
(ABC^2)_0 &= 000 + 011 + 022 + 101 + 112 + 120 + 202 + 210 + 221;\\
(ABC^2)_1 &= 002 + 010 + 021 + 100 + 111 + 122 + 201 + 212 + 220;\\
(ABC^2)_2 &= 001 + 012 + 020 + 102 + 110 + 121 + 200 + 211 + 222;\\
(AB^2C)_0 &= 000 + 011 + 022 + 102 + 110 + 121 + 201 + 212 + 220;\\
(AB^2C)_1 &= 001 + 012 + 020 + 100 + 111 + 122 + 202 + 210 + 221;\\
(AB^2C)_2 &= 002 + 010 + 021 + 101 + 112 + 120 + 200 + 211 + 222;\\
(AB^2C^2)_0 &= 000 + 012 + 021 + 101 + 110 + 122 + 202 + 211 + 220;\\
(AB^2C^2)_1 &= 002 + 011 + 020 + 100 + 112 + 121 + 201 + 210 + 222;\\
(AB^2C^2)_2 &= 001 + 010 + 022 + 102 + 111 + 120 + 200 + 212 + 221.
\end{aligned}
$$

The comparisons among three levels of each of the $p^2 + p + 1 =$ thirteen effects yield thirteen sums of squares, each with 2 degrees of freedom. The following breakdown of the 26 treatment degrees of freedom is possible:

Source of variation	Degrees of freedom		
Treatments	26		
Main effects			
A		2	
B		2	
C		2	
2-factor interactions			
A × B: AB		2	4
A × B: AB²		2	
A × C: AC		2	4
A × C: AC²		2	
B × C: BC		2	4
B × C: BC²		2	
3-factor interaction			
A × B × C: ABC = JJ		2	8
A × B × C: ABC² = II		2	
A × B × C: AB²C = JI		2	
A × B × C: AB²C² = IJ		2	

where the JJ, etc. notation is that used by Yates [324] and others [e.g., 60].

In a factorial experiment with no confounding of effects the two-factor and three-factor interactions usually are not partitioned into separate parts. The partitioning is useful in the construction and for the analysis of incomplete block experiments. The sums of squares are obtained as before. For example, the sum of squares for the AB^2C effect from r replicates is

$$\frac{(AB^2C)_0^2 + (AB^2C)_1^2 + (AB^2C)_2^2}{9r} - \frac{X....^2}{27r}; \tag{VII-30}$$

$9r$ yields make up each total for the level of an effect and $27r$ yields make up the grand total, X.....

VII-5.3 GENERALIZED INTERACTION IN THE 3^n SERIES

The concept of a generalized interaction carries over to the 3^n series or to the p^n series in general. Several examples of interactions, reduced modulo three, with the first factor at the first power are presented below:

$$\begin{cases} AB \times AB^2 = A^2B^3 = A^4B^6 = AB^0 = A; \\ AB \times A^2B^4 = A^3B^5 = B. \end{cases}$$

$$\begin{cases} A \times AB = A^2B = A^4B^2 = AB^2; \\ A \times A^2B^2 = A^3B^2 = B^2 = B. \end{cases}$$

$$\begin{cases} A \times B = AB; \\ A \times B^2 = AB^2. \end{cases}$$

$$\begin{cases} AB \times CD = ABCD; \\ AB \times C^2D^2 = ABC^2D^2. \end{cases}$$

$$\begin{cases} AB^2 \times CD^2 = AB^2CD^2; \\ AB^2 \times C^2D^4 = AB^2C^2D^4 = AB^2C^2D. \end{cases}$$

$$\begin{cases} ABC^2 \times CD^2 = ABC^3D^2 = ABD^2; \\ ABC^2 \times C^2D^4 = ABC^4D^4 = ABCD. \end{cases}$$

$$\begin{cases} ABCD \times B^2C^2D^2 = AB^3C^3D^3 = A; \\ ABCD \times B^4C^4D^4 = AB^2C^2D^2. \end{cases}$$

$$\begin{cases} ABCD \times CEFG = ABC^2DEFG; \\ ABCD \times C^2E^2F^2G^2 = ABDE^2F^2G^2. \end{cases}$$

VII-6 Other Factorials of the p^n Series

Application of the ideas presented thus far enables the experimenter to set up 2^5, 2^6, 2^7, etc. and 3^4, 3^5, etc. factorial combinations. In lieu of using large numbers of treatments, the optimum level for some factors may be determined and then a p^2 or p^3 factorial used for the remaining factors. Also, some form of fractional replication (Chapter IX) may suffice.

VII-6.1 THE 4×4 FACTORIAL

The 4^2 factorial may be considered either as a 2^4 or as a 4×4. The former consideration would be useful in the construction and analysis of an incom-

plete block design. The latter consideration would be necessary if two factors, each at four levels, were tested in all combinations. For example, four methods of mixing cakes and four types of baking powder might be tried in all combinations. The breakdown of the 15 treatment degrees of freedom from r replicates follows:

Source of variation	Degrees of freedom	
Replicates	r − 1	
Treatments	15	
Methods of mixing		3
Types of baking powder		3
Methods × types		9
Error	15(r − 1)	
Total	16r − 1	

The interaction sum of squares is obtained from the 4 × 4 table of treatment totals.

In another type of 4^n factorial experiment [273, Ch. 15] the experimenter may be interested in partitioning the main effect degrees of freedom into linear, quadratic, and cubic effects. In a like manner it is possible to obtain the linear × linear, linear × quadratic, quadratic × linear, linear × cubic, etc. comparisons by partitioning the interaction sum of squares into individual degrees of freedom.

VII-6.2 THE 5 × 5 FACTORIAL

Since five is a prime number, the 5^n series may be handled in the same manner as the 2^n and 3^n series. The breakdown of the degrees of freedom for a 5^2 factorial experiment with r replicates of a randomized complete block design is

Source of variation		Degrees of freedom		
Replicate		r − 1		
Treatment		24		
A			4	
B			4	
AB	A × B		4	16
AB^2			4	
AB^3			4	
AB^4			4	
Error		24(r − 1)		
Total		25r − 1		

For each level of an effect, $5r$ yields are available. Therefore, the sum of squares for the A effect is

$$\frac{(A)_0^2 + (A)_1^2 + (A)_2^2 + (A)_3^2 + (A)_4^2}{5r} - \frac{X_{\ldots}^2}{25r} \qquad \text{(VII-31)}$$

VII-6.3 GENERAL CASE

In general the $A \times B$ interaction in a $p \times p$ factorial may be partitioned into $p - 1$ effects, each with $p - 1$ degrees of freedom; thus,

$$A \times B = AB + AB^2 + \cdots + AB^{p-1} = \sum_{u=1}^{p-1} AB^u. \qquad \text{(VII-32)}$$

The $A \times B \times C$ interaction in a $p \times p \times p$ factorial may be partitioned into $(p - 1)^2$ effects, each with $(p - 1)$ degrees of freedom; thus:

$$A \times B \times C = \sum_{u=1}^{p-1} \sum_{v=1}^{p-1} AB^uC^v. \qquad \text{(VII-33)}$$

The treatments making up a level of an effect are determined from the formula $i + uj$ for a p^2 factorial and from the formula $i + uj + vh$ for the p^3 factorial. The above formula may be extended for p equal to any prime number.

The $p^2 - 1$ degrees of freedom for a p^2 factorial may be partitioned into $p + 1$ effects, each with $p - 1$ degrees of freedom. Likewise, the $p^3 - 1$ degrees of freedom for a p^3 factorial may be partitioned into $p^2 + p + 1$ effects, each with $p - 1$ degrees of freedom. In general, the $p^n - 1$ degrees of freedom for a p^n factorial may be partitioned into $p^{n-1} + p^{n-2} + \cdots + p + 1$ effects each with $p - 1$ degrees of freedom.

VII-7 Experimental Designs For Factorial Experiments

VII-7.1 EXPERIMENTS WITH REPETITION OF THE p^n TREATMENTS

The three experimental designs allowing for a repetition of a set of treatments discussed thus far are the completely randomized design, the randomized complete block design, and the latin square design. The first two designs may be used for any number of treatments, while the latin square design is practical only for the 2^2, 2^3, and 3^2 factorials. Larger factorials in a latin square design require an excessive amount of repetition of the set of treatments.

Ordinarily the 5^2, 3^3, 2^5, 2^6, 4^3, 3^4, 5^3, etc., and sometimes the 2^4 and 4^2 factorial experiments, are not designed as randomized complete block experiments, since it is difficult, and often impossible, to obtain enough homogeneous material. Therefore, it is necessary to stratify a complete replicate into homogeneous subgroups and to use confounding and incomplete block designs to control variation within a complete replicate (see later chapters).

VII-7.2 EXPERIMENTS WITHOUT REPETITION OF THE p^n TREATMENTS

The three- and four-factor (or second- and third-order) interactions may be of negligible size in some factorial experiments. If this is true, these interactions may be pooled with the experimental error. Knowledge of this may be useful in setting up an experiment. For example, suppose it is desired to ob-

serve the effects of the fertilizer treatments—two levels of the four factors, nitrogen (n), potash (k), phosphorus (p), and lime (l), in all combinations—on the composition of the vegetation growing in peat and bog lands. The sixteen fertilizer treatments could be applied in a specified year and the effect on kind and amount of vegetation observed over a period of years. Since it is difficult to transport the fertilizer to the experimental area, it may be decided to use one replicate of the sixteen treatments. Several checks could be included, and the variation among the checks and the interactions for three and four factors could be used as the experimental error. The breakdown of the degrees of freedom in the analysis of variance for the sixteen treatments and four additional checks placed at random over the experimental area is

Source of variation	Degrees of freedom	Source of variation	Degrees of freedom
N	1	L	1
K	1	NL	1
NK	1	KL	1
P	1	PL	1
NP	1	Among 5 checks	4 } 9
KP	1	3- and 4-factor interactions	5 } 9
		Total	19

For such a design, information is available on all main effects and two-factor (or first-order) interactions; 9 degrees of freedom are available for the error sum of squares. The standard error of a main effect is $\sqrt{E_e/4}$. As a further precaution, check plots could be placed between each plot to control (by covariance) some of the variation over the experimental area. With a covariance analysis, 8 degrees of freedom are available for the error sum of squares.

Fisher [126, sec. 41] suggests this type of design for a 2^6 factorial experiment with the three-, four-, five-, and six-factor interactions to be used as error. In a design of this type the three-factor interactions could be excluded from the error, since there are sufficient degrees of freedom for the error term from the four-, five-, and six-factor interactions. If the interactions used as error are not actually zero in the population, a positive bias results in the estimation of the error variance [175, sec. 14.6]. If the true interaction effects are small relative to the error variance, little harm results from the above procedure.

CHAPTER VIII

Other Factorial Experiments

VIII-1 The $p \times q \times k \cdots$ Series of Factorials

In the preceding chapter the discussion is limited to factorials of the p^n type where the n factors are each at p levels. It may be undesirable to have all factors at the same level and, indeed, it is not necessary. The p^n series of factorials are discussed separately from other factorials in order to introduce the modulo notation, which is suitable for the present usage of prime numbers or powers of prime numbers but is not suitable for nonprime numbers. The notational system of the preceding chapter is used wherever applicable in the remaining chapters.

The main effects in a $p \times q$ and in a $p \times q \times k$ are estimated in the same manner as described previously [324]. The interaction sums of squares are obtained from formulae (VII-1) and (VII-2), respectively; the method here is the same as described by Snedecor [273] for two- and three-way classifications. The breakdown of the degrees of freedom is illustrated below for a $p \times q$ factorial and a $p \times q \times k$ factorial. If there are p levels of one factor, say a, and q of another, say b, the breakdown of the degrees of freedom for an experiment in which the pq treatments are arranged in r replicates of a randomized complete block design is

Source of variation	Degrees of freedom	
Replicate	r − 1	
Treatment	pq − 1	
A		p − 1
B		q − 1
A × B		(p − 1)(q − 1)
Error	(r − 1)(pq − 1)	
Replicate × A		(r − 1)(p − 1)
Replicate × B		(r − 1)(q − 1)
Replicate × A × B		(r − 1)(p − 1)(q − 1)
Total	rpq − 1	

The error sum of squares for all two-factor factorials may be partitioned in a similar manner to that described in the above experiment.

As a further illustration of factorial experiments, suppose that three factors, a, b, and c, are at p, q, and k levels, respectively, and are to be tested in all

combinations. Suppose that the design is r replicates of a randomized complete block. Then, the breakdown of the total degrees of freedom in the analysis of variance is

Source of variation	Degrees of freedom	
Replicate = R	r − 1	
Treatment = T	pqk − 1	
A		p − 1
B		q − 1
A × B		(p − 1)(q − 1)
C		k − 1
A × C		(p − 1)(k − 1)
B × C		(q − 1)(k − 1)
A × B × C		(p − 1)(q − 1)(k − 1)
R × T	(r − 1)(pqk − 1)	
Total	rpqk − 1	

The $p - 1$ degrees of freedom may be partitioned into $p - 1$ individual degrees of freedom if the comparisons are meaningful. If the factor a is applied in p different amounts, it may be desirable to estimate the linear, quadratic, and perhaps other responses. Also, the b factor may be partitioned into contrasts with single degrees of freedom if the contrasts represent meaningful, and not fortuitous, comparisons. The interaction of the various contrasts may also be obtained (example VII-3).

If s^2 = the error mean square, the standard error for a mean level of the factor a from a $p \times q \times k$ factorial experiment composed of r replicates is $\sqrt{s^2/rqk}$. Likewise, the corresponding standard errors for the mean level of the factors b and c are $\sqrt{s^2/rpk}$ and $\sqrt{s^2/rpq}$, respectively. The standard error of a mean for a particular treatment, say $\bar{x}_{.ijh}$, is $\sqrt{s^2/r}$.

VIII-1.1 THE $2^n \times 3^s$ SERIES

A useful type of factorial arrangement is to have s factors each at three levels and n factors each at two levels. Thus, for $n = 1 = s$, we have a 2×3 factorial composed of two levels of factor a and three levels of factor b in all combinations. For $n = 2$, $s = 1$, we have a $2 \times 2 \times 3$ factorial composed of twelve treatments. For $n = 1$, $s = 2$, we have a $2 \times 3 \times 3$ factorial composed of eighteen treatments. These are some of the more common factorials of the $2^n \times 3^s$ type. The randomized complete block design is suitable for these factorials in a large number of cases. Occasionally, larger combinations are used, in which case it may be desirable to use one of the incomplete block designs discussed in the following chapters in order to exclude some of the heterogeneity within the complete block. The type of material and heterogeneity of the experimental area should be considered in selecting a design.

Example VIII-1. Sauvage [261] presents the data for three green manure crops, v_0 = Sunnhemp, v_1 = Woolly Pyrol, and v_2 = Sword Bean, grown with $(= n_1)$ and

without ($= n_0$) nitrogen in six replicates of a randomized complete block design. The yields (table VIII-1) in pounds per experimental unit (1/36 acre harvested from each plot after border was removed) are for green weights of the crops.[1]

TABLE VIII-1. Yield data in pounds of green matter per 1/36 acre plot

Treatments	Blocks I	II	III	IV	V	VI	Total
00	229	278	246	331	323	288	1695
01	306	287	306	282	412	305	1898
10	57	91	119	186	170	107	730
11	126	156	83	184	157	75	781
20	145	194	173	154	195	285	1146
21	209	194	226	305	184	292	1410
Total	1072	1200	1153	1442	1441	1352	7660

Varieties $= v_i$

Nitrogen	Sunnhemp	Woolly Pyrol	Sword Bean	Total
n_0 = none	1695	730	1146	3571
n_1 = 150 pounds of $(NH_4)_2SO_4$/A	1898	781	1410	4089
Total	3593	1511	2556	7660

Analysis of variance

Source of variation	df	ss	ms
Block	5	20512	4102
Treatment	5	190073	-
V = variety	2	180614	90307
N = nitrogen	1	7453	7453
V x N	2	2006	1003
Error	25	46507	1860.28
Total	35	257092	-
Correction for mean	1	1629878	-
Total uncorrected	36	1886970	-

The treatment, replicate, total, and error sums of squares in table VIII-1 are obtained in the same manner as for a randomized complete block design. The remaining sums of squares are computed as follows:

Variety sum of squares (2df):

$$\frac{3593^2 + 1511^2 + 2556^2}{2 \times 6 = 12} - \frac{7660^2}{36} = 180614.$$

[1]A more realistic measure of the value of the crops with and without nitrogen might be obtained by using dry weights instead of green weights of the crop. The heavier yields might be due to a higher moisture content; if so, the estimated additional nutrients and organic matter added to the soil are too large.

Nitrogen sum of squares (1df):

$$\frac{3571^2 + 4089^2}{3 \times 6 = 18} - \frac{7660^2}{36} = \frac{(3571 - 4089)^2}{3 \times 6(1 + (-1)^2)} = 7453.$$

Variety × nitrogen sum of squares (5 − 2 − 1 = 2df):

$$190073 - 180614 - 7453 = 2006.$$

In this experiment, Woolly Pyrol yielded 42 per cent and Sword Bean yielded 71 per cent as much green weight per plot as obtained from Sunnhemp. The *hsd* for a difference between variety totals is $\sqrt{1860.28(12)}(3.52) = 525.9$ (section II-1.1.4). The varieties differ from each other with respect to total yield of green weight.

There is an indication that the effect of adding ammonium sulphate at the rate of 150 pounds per acre is to increase green weight. If it is assumed that in no case should the addition of nitrogen decrease yields, then we make a one-tailed t test of the difference; thus:

$$t = \frac{4089 - 3571}{\sqrt{1860.28(18)(2)}} = 2.00 > t_{10}(25df) = 1.708.$$

However, one should know something about the relative moisture content, dry weight, and cost of applying the fertilizer prior to recommending the use of ammonium sulphate over no fertilizer. There is no evidence of a variety by nitrogen interaction; the results are presented graphically in figure VIII-1.

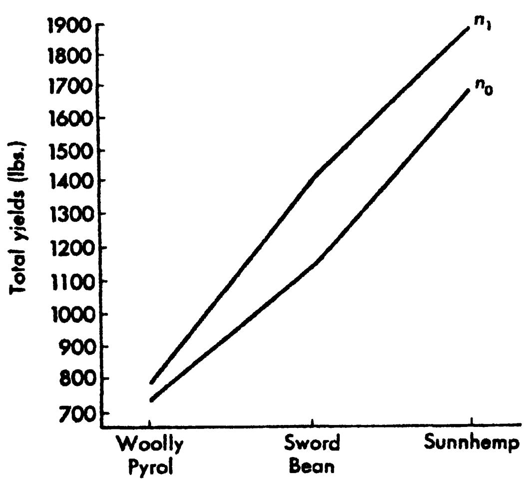


Figure VIII-1. Graphical presentation of treatment yields from table VIII-1

The standard errors are

Treatment totals:

$$\sqrt{s^2 r} = \sqrt{1860.28(6)} = 105.6.$$

TABLE VIII-2. Sums, sums of squares and products, regressions, and analysis of variance of regressions for $n_1(Y)$ with $n_0(X)$ by variety

$(n_0)^2$	$(n_0)(n_1)$	$(n_1)^2$
Sunnhemp (v_0)		
$\Sigma X_u^2 = 487075$	$\Sigma X_u Y_u = 539394$	$\Sigma Y_u^2 = 611934$
$\Sigma X_u = 1695$		$\Sigma Y_u = 1898$
$(\Sigma X)^2/6 = 478837.5$	$\Sigma X \Sigma Y/6 = 536185.0$	$(\Sigma Y)^2/6 = 600400.7$
$\Sigma x^2 = 8237.5$	$\Sigma xy = 3209.0$	$\Sigma y^2 = 11533.3$
$b = 3209.0/8237.5 = 0.3896$		
Woolly Pyrol (v_1)		
$\Sigma X^2 = 100636$	$\Sigma XY = 100194$	$\Sigma Y^2 = 111231$
$\Sigma X = 730$		$\Sigma Y = 781$
$(\Sigma X)^2/6 = 88816.7$	$\Sigma X \Sigma Y/6 = 95021.7$	$(\Sigma Y)^2/6 = 101660.2$
$\Sigma x^2 = 11819.3$	$\Sigma xy = 5172.3$	$\Sigma y^2 = 9570.8$
$b = 5172.3/11819.3 = 0.4376$		
Sword Bean (v_2)		
$\Sigma X^2 = 231556$	$\Sigma XY = 273109$	$\Sigma Y^2 = 344538$
$\Sigma X = 1146$		$\Sigma Y = 1410$
$(\Sigma X)^2/6 = 218886.0$	$\Sigma X \Sigma Y/6 = 269310.0$	$(\Sigma Y)^2/6 = 331350.0$
$\Sigma x^2 = 12670.0$	$\Sigma xy = 3799.0$	$\Sigma y^2 = 13188.0$
$b = 3799.0/12670.0 = 0.2998$		

$$\text{average } b = \frac{3209.0 + 5172.3 + 3799.0}{8237.5 + 11819.3 + 12670.0} = 0.37218$$

Analysis of variance

Source of variation	df	ss	ms
Linear regressions within varieties	3	4652.67	-
Average regression	1	4533.28	-
Among regressions	2	119.39	-
Deviations from individual regressions	12	29639.43	2469.95
Total for n_1 within varieties	15	34292.1	-

Variety totals:

$$\sqrt{s^2rq} = \sqrt{1860.28(6)(2)} = 149.4.$$

Nitrogen totals:

$$\sqrt{s^2rp} = \sqrt{1860.28(6)(3)} = 183.0.$$

The coefficient of variation is equal to $s/\bar{x} = 36\sqrt{1860.28}/7660 = 20$ per cent.

The above analysis has been carried through assuming homogeneity of error variances for the various comparisons and additivity of the data. For use in future research of this type the validity of these assumptions may be tested. First of all the validity of the assumption of additivity for nitrogen yields might be tested. Suppose that there is additivity between yields of n_0 and n_1 within varieties and within replicates. Then, the value of the regression of n_1 yields, $X_{gi1} = Y_u$, on n_0 yields, $X_{gi0} = X_u$, should not differ from unity. If the observed regression differs significantly from $\beta = 1$, there is reason to suspect the assumption of additivity. To test the hypothesis that $\beta = 1$, the sums of squares and products are first computed (table

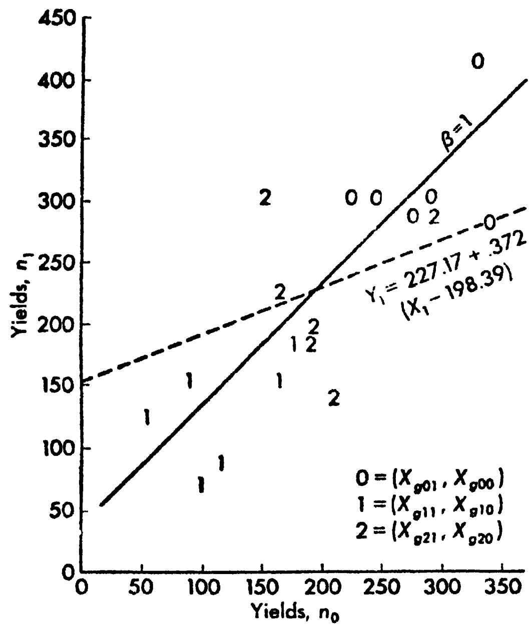


Figure VIII-2. Yields of n_1 plotted against yields of n_0 within variety and replicate

VIII-2). For example, the sum of cross products for n_1 and n_0 for the different replicates for Sunnhemp is $229(306) + 278(287) + 246(306) + 331(282) + 323(412) + 288(305) - 1695(1898)/6 = 3209.0$ [273, sec. 12.5]. The test of the hypothesis that $\beta = 1$ is $t = (.37218 - 1)/\sqrt{2469.95/32{,}726.8} = 2.29 > t_{.05,12df} = 2.18$. Thus, we would reject the hypothesis of unit slope for the average within-variety regression of n_1 yields on n_0 yields (see figure VIII-2). For a given variety the difference between

n_1 and n_0 yields decreases as the yield level of the replicate increases. Thus, there is a replicate × nitrogen interaction, and the assumption of additivity is suspected.

The procedure described in the first part of section II-4 provides a test of another sort of nonadditivity. The necessary totals for applying the procedure are given in table VIII-3. The difference $X_{gi1} - X_{gi0}$ represents the linear response of nitrogen. Hence, the computation of the linear × linear interaction (formulae (II-11) and (V-3)) of these differences yields the linear × linear × linear interaction. The sums

TABLE VIII-3. Totals and deviations for computing sums of squares for non-additivity

	Replicate I	II	III	IV	V	VI	Total	Deviation
				$(n_0 + n_1)$				
Variety - v_0	535	565	552	613	735	593	3593	86.7
v_1	183	247	202	370	327	182	1511	-86.9
v_2	354	388	399	459	379	577	2556	0.2
Total	1072	1200	1153	1442	1441	1352	7660	-
Deviation	-34.1	-12.8	-20.6	27.6	27.4	12.5	-	0
				$(v_0 + v_1 + v_2)$				
Nitrogen - n_0	431	563	538	671	688	680	3571	-14.4
n_1	641	637	615	771	753	672	4089	14.4
Total	1072	1200	1153	1442	1441	1352	7660	-
Deviation	-34.1	-12.8	-20.6	27.6	27.4	12.5	-	0
				$(n_1 - n_0)$				
Variety - v_0	77	9	60	-49	89	17	203	2.5
v_1	69	65	-36	- 2	-13	-32	51	-10.1
v_2	64	0	53	151	-11	7	264	7.6
Total	210	74	77	100	65	- 8	518	-
Deviation	20.6	- 2.1	- 1.6	2.3	- 3.5	-15.7	-	0

of squares for the various components are given in table VIII-4. The various residual variances do not appear to be homogeneous ($\chi^2 = 11.9 > \chi^2_{01}(2df) = 9.2$), and there is nonadditivity of nitrogen with replicates. The nonadditivity is different from that obtained by the previous method in that the difference between n_1 and n_0 is obtained from the totals over varieties (i.e., $X_{g\cdot1} - X_{g\cdot0}$) rather than within varieties. Also, the residual N × replicate mean square, 36.0, appears unusually small. The fact that the hypothesis of the linear × linear form of additivity for variety × replicate and for variety × nitrogen × replicate is not rejected here does not mean that other types of nonadditivity are not present. Since the varieties differ widely in yield, one would suspect that the estimated means are subject to different errors. Also, since the replicates and varieties differ widely in yields, an interaction is probable. More experi-

mental data of this type are needed to investigate these possibilities. Investigations of the above nature on a number of experiments will yield empirical evidence of the type and extent of the nonadditivity. Then, from these results the experimenter will be able to determine whether the analysis should be of the form given in table VIII-4

TABLE VIII-4. Partitioning of replicate × treatment sum of squares

Source of variation	df	ss	ms
Replicate x treatment	25	46507	-
Variety x replicate	10	25178	2517.8
Non-additivity	1	10.4	-
Residual	9	25167.6	2796.4
Nitrogen x replicate	5	4179	835.8
Non-additivity	1	4035.1	-
Residual	4	143.9	36.0
Variety x nitrogen x replicate	10	17150	1715.0
Non-additivity	1	274.0	-
Residual	9	16876.0	1875.1

or whether some of the components in the analysis of variance may be pooled to obtain the estimated error variance for a given contrast.

VIII-1.2 OTHER FACTORIALS

In addition to factorials of the $2^n \times 3^s$ type, we may use $2^n \times 4^s$, $2^n \times 5^s$, $2^n \times 6^s$, $2^n \times 7^s$, etc., $3^n \times 4^s$, $3^n \times 5^s$, $3^n \times 6^s$, etc., and many other factorials. The particular number of levels for the various factors depends upon the nature of the material being tested and the objectives of the experiment. However, with larger factorials, it may be necessary and (or) desirable to use confounding and incomplete block designs to control some of the variability over the experimental area. Methods and designs for doing so are discussed in the following chapters.

VIII-2 Additional Treatments in Factorial Experiments

VIII-2.1 FACTORIAL ARRANGEMENT OF TREATMENTS PLUS OTHER TREATMENTS

It is not necessary that all treatments in a factorial experiment be those constituting a particular factorial arrangement. Other treatments may be added if they are of interest to the experimenter. For example, one might include a zero level of all factors plus a p^n factorial for which the lowest level was not zero (problem VII-1). Or, one might include two factorial arrangements in one experiment; for example, a 2^2 and a 2^3 factorial could be arranged in a randomized complete block or latin square design consisting of

$4 + 8 = 12$ treatments. The breakdown of the total degrees of freedom for such an arrangement in a latin square design follows:[1]

Source of variation	df		Source of variation	df	
Total	143		2^3 set: C		1
Column	11		D		1
Row	11		CD		1
Treatment	11		E		1
2^2 set: A		1	CE		1
B		1	DE		1
AB		1	CDE		1
2^2 set vs 2^3 set		1	Error	110	

If two sets of treatments are included in the experiment, one set may be a factorial arrangement but the other set need not be. For example, suppose that we wish to study the oxygen intake of dairy workers carrying milk pails of two different weights, w, over three different distances, d, down a straight path, and pouring the milk into containers at three different heights, h. In addition, it is desired to determine the effect of walking down a straight aisle, an aisle with a 90-degree turn, and an aisle with steps in it for the combination $d_1w_0h_1$ and for a longer distance than used for the 2×3^2 factorial treatments. Denoting the type of aisle as the A effect, the breakdown of the total degrees of freedom for the $3 \times 2 \times 3 + 3 = 21$ treatments arranged in two replicates of a randomized complete block design is

Source of variation	df		Source of variation	df	
Replicate	1		H		2
Treatment	20		D × H		4
A		2	W × H		2
A vs factorial set		1	D × W × H		4
D		2	Error	20	
W		1	Total	41	
D × W		2	Correction for mean	1	

In some experiments, it may be necessary to use sets of factorial arrangements in order to keep the treatment combinations within the limit of practicability. For other types of experiments, it may be inadvisable to include some combinations in factorial experiments; for example, we might wish to try three levels of baking powder, b, with four levels of sugar, s, in a cake-

[1]Hunter *et al.* [163] present the analysis for a 3×3 factorial of methods of mixing and amounts of sugar in an experiment with a 3^3 factorial composed of 3 kinds of fat $\times$ 3 temperatures $\times$ 3 methods of mixing.

baking experiment, but we know that certain combinations will give failures. Therefore, we make use of the following combinations (marked X) in our experiment, since we are reasonably certain that total failures will not be produced with these treatments:

	s_0	s_1	s_2	s_3
b_0	—	—	X	X
b_1	—	X	X	—
b_2	X	X	X	—

If the above seven treatments are compared in a 7 × 7 latin square design, one possible breakdown of the total degrees of freedom is (note that treatments 11, 12, 21, and 22 form a 2 × 2 factorial)

Source of variation	df	Source of variation	df
Row	6	2^2 set: S	1
Column	6	2^2 set: B	1
Treatment	6	2^2 set: S × B	1
Among 20, 30, 02	2	Error	30
2^2 set vs others	1	Total	48

Of course the nature of the particular combinations chosen determines the treatment comparisons made. With the above set, some information is obtained on the interaction of levels of sugar by levels of baking powder. If it is desired to estimate the maximum cake volume for combinations of the two factors, holding all other factors affecting cake volume constant, one of the methods mentioned in section VII-3 or the one suggested by Box and Hunter [35, 36] may be used. In selecting specific combinations, the experimenter should make certain that the number of replicates (including hidden replicates) is sufficient for his purposes.

VIII-2.2 ADDITIONAL COMBINATIONS INCLUDED IN THE FACTORIAL EXPERIMENT

In some situations the experimenter desires to have additional replication on the check treatment, the lowest level of all factors, or on one of the other combinations of a factorial arrangement. The decision to include extra replicates of a treatment may be made because (i) it is known or suspected that this treatment is more (or less) variable than the other treatments or (ii) more information is desired on this treatment than on the others. Several

problems are immediately encountered in the analysis of a factorial experiment of the above type. Some of these are

(i) the utilization of all experimental units in obtaining estimates of main effects and interactions,
(ii) the orthogonality of contrasts,
(iii) alternative estimates of effects,
(iv) the treatments to be repeated and the method of estimation resulting in orthogonal contrasts,
(v) the computation of variances for various estimates of the effects, and
(vi) the unbiasedness of estimated effects.

The above points should be considered prior to conducting an experiment including extra checks or treatments in a factorial experiment.

VIII-2.3 PROPORTIONAL FACTORIALS

If additional treatments are desired in a p^n factorial, it is possible to select the combinations in such a way that the resulting contrasts form an orthogonal set of comparisons. This condition, in itself, may have little merit [273, sec. 15.4] unless the estimates have other desirable properties, such as minimal variance and unbiasedness. One method of selecting treatments in factorial experiments is to duplicate (or triplicate) the levels of one factor at one or more levels of the second factor. For example, the treatments 00, 00, 10, 10, 01, and 11 would form a set of treatments composed of duplicates of a_0 and a_1 at the zero level of factor b. A set of treatments selected in this manner is designated as a *proportional factorial*, since the numbers of replicates are proportional in the two-way (or higher) classification.

In the 3^2 factorial the three levels of factor a could be duplicated (or triplicated) for one of the levels of a second factor, say b_0, resulting in the twelve treatments: 00, 00, 10, 10, 20, 20, 01, 11, 21, 02, 12, and 22. Also, the level of factor a could be duplicated at two levels of factor b, say b_0 and b_1, resulting in fifteen treatment combinations. This method of constructing proportional factorials may be extended to other factorial combinations.

The problems enumerated in section VIII-2.2 also apply to proportional factorials. Although it is possible to select a set of orthogonal contrasts, some of the contrasts result in biased estimates in certain situations. Prior to conducting an experiment as a proportional factorial, it is desirable to obtain satisfactory answers to the problems enumerated above.

VIII-2.4 COMMENTS ON THE INCLUSION OF ADDITIONAL TREATMENTS

Unless the experimenter is fairly certain that the additional checks or other treatments have a different variance, which must be estimated separately, duplicates (or triplicates) of certain combinations of a factorial arrangement should not be included. In order to obtain more information on certain combinations, it is better to use additional replicates of all treatments. For example, a 2^2

factorial in five replicates occupies twenty experimental units, and a 2^2 factorial and one additional check in four replicates require the same number of plots. The additional replication on the remaining three treatments is well worth considering. Likewise, a 2^3 factorial set of treatments and two additional checks in three replicates require thirty plots, while 2^3 treatments in four replicates require only thirty-two plots. Also, the replicate size is larger when additional treatments are included. Such considerations as these make it imperative to "think twice" before duplicating (or triplicating) certain treatments in factorial experiments at the expense of reducing the replication on other treatments.

The main use of proportional factorials is for situations in which more information is desired for some factors than for others. In the proportional factorial scheme, additional replication is obtained on some combinations, the experimental error variance for the repeated treatments may be separately estimated, and all available material may be utilized in estimating main effects and interactions. In the absence of interaction, unbiased estimates of the main effects are possible. The particular form of the estimate used is the one giving minimal variance [122]. If the error mean square is used for testing the main effect mean squares, no trouble results. However, if the interaction mean square is used as the error, equality of replication of treatments is recommended, since no appropriate experimental error is possible in a proportional factorial [270].

VIII-3 Designs for $p \times q \times k \cdots$ Factorials

Latin square designs may be used for 2 × 3, 2 × 4, 2 × 5, 2 × 6, 2 × 2 × 3, and 3 × 4 factorials, although the replication may be excessive for some of the larger of these factorials. The above factorials plus the 2 × 7, 2 × 2 × 4, 2 × 3 × 3, 2 × 8, 2 × 9, 3 × 5, 3 × 6, 3 × 7, 4 × 5, and occasionally larger factorials may be designed in a randomized complete block design or as a completely randomized design as conditions warrant. For larger factorials, one of the schemes of confounding suggested in the following chapters should suffice [324].

For the larger factorials, replication of the entire set of treatments may be dispensed with and the interactions of some three-factor and the four- and higher-factor interactions used as the estimate of the experimental error variance. For example, it was desired to compare three levels of choline, c, two levels of methionine, m, two levels of folic acid, f, and two levels of B_{12}, b, in a poultry nutrition experiment. Since only twenty-four batteries (pens) were available, a second replicate of the treatments would have to be performed at a later date. This was considered impractical. If, from previous experience, the three-factor interactions and the four-factor interaction could be assumed negligible or nonexistent, they may be used as an estimate of the experimental

error variance and only one replicate is required. For this case the breakdown of the degrees of freedom for the 3×2^3 factorial is

Source of variation		df	Source of variation		df
C	(choline)	2	3 and 4-factor interactions		9
M	(methionine)	1	Error	C×M×F	2
F	(folic acid)	1		C×M×B	2
B	(B_{12})	1		C×F×B	2
C×M		2		M×F×B	1
C×F		2		C×M×F×B	2
C×B		2	Total		23
M×F		1	Correction for mean		1
M×B		1	Total uncorrected		24
F×B		1			

If it is not realistic to assume that the three- and four-factor interactions constitute an estimate of the error variance, an alternative procedure is to estimate the linear, C_L, and quadratic, C_Q, effects of choline. The mean square obtained from pooled sums of squares of the interactions of other factors with C_Q may then be used as an estimate of the experimental error. This error mean square has 7 degrees of freedom associated with it. Whether or not this is a useful procedure depends upon the nature of the treatments in the experiment and upon the nature of the experimental material used.

VIII-4 Missing Data

The estimation of missing plot values in any of the above factorials will depend upon the particular experimental design used. If the completely randomized design has been used, the method of Chapter IV is recommended. In a randomized complete block design a missing experimental unit is estimated as described in Chapter V. Likewise, for a latin square design, missing data are estimated by the method described in Chapter VI.

VIII-5 Least Squares Estimates of Effects and Expectation of Mean Squares

The expectation of mean squares and the estimation of effects for a $p \times q$ factorial in a completely randomized design is equivalent to that presented in Chapter V with k individuals per experimental unit. However, if the $p \times q$ or the $p \times q \times k$ factorial is arranged in a randomized complete block design, the procedure for obtaining the expectations becomes slightly more complex.

In factorial experiments the assumption that the p levels of factor a and the q levels of factor b represent a sample from an infinite population usually is not warranted. For completeness the analyses are given for both the finite and the infinite models.

In Chapters IV to VI the estimates of the parameters, Greek letters, are denoted by Latin letters. Since the number of symbols is greatly increased in the more complex experiments, this system is discontinued in the following chapters. Instead, the estimate of α_i is denoted as $\hat{\alpha}_i$, i.e., the estimate is denoted by a hat over the quantity to be estimated. This scheme is used to denote the estimate of μ in Chapters IV to VI.

VIII-5.1 THE $p \times q$ FACTORIAL IN A RANDOMIZED COMPLETE BLOCK DESIGN

For the first situation, consider that the $p \times q$ factorial is arranged in a randomized complete block design [313] and that the linear model,

$$X_{gij} = \mu + \rho_g + \alpha_i + \beta_j + \alpha\beta_{ij} + \epsilon_{gij}, \tag{VIII-1}$$

represents the yield of the jth level of the factor b and the ith level of the factor a in the gth replicate. It is assumed that μ is the population mean, the effects are independent, ρ_g is the replicate effect, α_i = effect of ith level of a, β_j = effect of jth level of factor b, $\alpha\beta_{ij}$ = interaction effect of the ith level of a on the jth level of b, and ϵ_{gij} = a random effect. No assumption of normality is involved in the estimation of effects or of their estimated variances.

Let $g = 1, 2, \cdots, r$; $i = 1, 2, \cdots, p$; and $j = 1, 2, \cdots, q$; let the grand total be designated as $X_{...}$, the gth replicate total as $X_{g..}$, the ith level total of the factor a as $X_{.i.}$, the jth level total of the factor b as $X_{..j}$, and the ijth total as $X_{.ij}$. Given the residual sum of squares,

$$\sum_{g=1}^{r}\sum_{i=1}^{p}\sum_{j=1}^{q}(X_{gij} - \hat{\mu} - \hat{\rho}_g - \hat{\alpha}_i - \hat{\beta}_j - \widehat{\alpha\beta}_{ij})^2 = \theta, \tag{VIII-2}$$

the least squares estimates of the various effects are obtained by differentiating θ with respect to each effect, setting each resulting equation equal to zero, and solving for the effects in terms of the observations.

$$\frac{\partial\theta}{\partial\hat{\mu}} = -2\sum\sum\sum(X_{gij} - \hat{\mu} - \hat{\rho}_g - \hat{\alpha}_i - \hat{\beta}_j - \widehat{\alpha\beta}_{ij}) = 0. \tag{VIII-3}$$

$$\frac{\partial\theta}{\partial\hat{\rho}_g} = -2\sum_i\sum_j(X_{gij} - \hat{\mu} - \hat{\rho}_g - \hat{\alpha}_i - \hat{\beta}_j - \widehat{\alpha\beta}_{ij}) = 0. \tag{VIII-4}$$

$$\frac{\partial\theta}{\partial\hat{\alpha}_i} = -2\sum_g\sum_j(X_{gij} - \hat{\mu} - \hat{\rho}_g - \hat{\alpha}_i - \hat{\beta}_j - \widehat{\alpha\beta}_{ij}) = 0. \tag{VIII-5}$$

$$\frac{\partial\theta}{\partial\hat{\beta}_j} = -2\sum_g\sum_i(X_{gij} - \hat{\mu} - \hat{\rho}_g - \hat{\alpha}_i - \hat{\beta}_j - \widehat{\alpha\beta}_{ij}) = 0. \tag{VIII-6}$$

$$\frac{\partial\theta}{\partial\widehat{\alpha\beta}_{ij}} = -2\sum_g(X_{gij} - \hat{\mu} - \hat{\rho}_g - \hat{\alpha}_i - \hat{\beta}_j - \widehat{\alpha\beta}_{ij}) = 0. \tag{VIII-7}$$

If we set $\sum\hat{\rho}_g = \sum\hat{\alpha}_i = \sum\hat{\beta}_j = \sum_i\widehat{\alpha\beta}_{ij} = \sum_j\widehat{\alpha\beta}_{ij} = 0$ and solve the above equations, we find that

$$\hat{\mu} = X_{...}/pqr = \bar{x}, \tag{VIII-8}$$

$$\hat{\rho}_g = \frac{X_{g..}}{pq} - \hat{\mu} = \bar{x}_{g..} - \bar{x}, \tag{VIII-9}$$

$$\hat{\alpha}_i = \frac{X_{.i.}}{rq} - \hat{\mu} = \bar{x}_{.i.} - \bar{x}, \tag{VIII-10}$$

$$\hat{\beta}_j = \frac{X_{..j}}{rp} - \hat{\mu} = \bar{x}_{..j} - \bar{x}, \tag{VIII-11}$$

and

$$\widehat{\alpha\beta}_{ij} = \frac{X_{.ij}}{r} - \hat{\alpha}_i - \hat{\beta}_j - \hat{\mu} = \bar{x}_{.ij} - \bar{x}_{.i.} - \bar{x}_{..j} + \bar{x}. \tag{VIII-12}$$

The variance of $\hat{\mu}$ is

$$\begin{aligned} &E[\hat{\mu} - \mu]^2 \\ &= E[\hat{\mu}]^2 - \mu^2 \\ &= E\Big[\mu + \frac{1}{r}\sum\rho_g + \frac{1}{p}\sum\alpha_i + \frac{1}{q}\sum\beta_j + \frac{1}{pq}\sum\sum\alpha\beta_{ij} \\ &+ \frac{1}{rpq}\sum\sum\sum\epsilon_{gij}\Big]^2 - \mu^2 = \frac{\sigma_\epsilon^2}{rpq}, \end{aligned} \tag{VIII-13}$$

if it is assumed that $\sum\rho_g = \sum\alpha_i = \sum\beta_j = \sum_i\alpha\beta_{ij} = \sum_j\alpha\beta_{ij} = 0$. Likewise, the variance of $\hat{\rho}_g$ is

$$\begin{aligned} E[\hat{\rho}_g - \rho_g]^2 &= E\Big[\frac{1}{pq}\sum_i\sum_j\epsilon_{gij} - \frac{1}{pqr}\sum\sum\sum\epsilon_{gij}\Big]^2 \\ &= \frac{r-1}{rpq}\sigma_\epsilon^2 \text{ for } \sum\rho_g = 0. \end{aligned} \tag{VIII-14}$$

The covariance of any two $\hat{\rho}$'s, say $\hat{\rho}_1$ and $\hat{\rho}_2$, is equal to

$$\begin{aligned} E[(\hat{\rho}_1 - \rho_1)(\hat{\rho}_2 - \rho_2)] &= E\Big[\Big(\frac{X_{1..}}{pq} - \bar{x} - \rho_1\Big)\Big(\frac{X_{2..}}{pq} - \bar{x} - \rho_2\Big)\Big] \\ = E\Big[\Big(\frac{1}{pq}\sum\sum\epsilon_{1ij} - \frac{1}{rpq}\sum\sum\sum\epsilon_{gij}\Big)&\Big(\frac{1}{pq}\sum\sum\epsilon_{2ij} - \frac{1}{rpq}\sum\sum\sum\epsilon_{gij}\Big)\Big] \\ = -\sigma_\epsilon^2/rpq. \end{aligned} \tag{VIII-15}$$

Likewise, the variance of the difference between any two $\hat{\rho}$'s, or of any two replicate means, is $2\sigma_\epsilon^2/pq$; the variance of any $\hat{\alpha}_i$ is $(p-1)\sigma_\epsilon^2/rpq$; the covariance of any two $\hat{\alpha}$'s is $-\sigma_\epsilon^2/rpq$; the variance of any $\hat{\beta}_j$ is $(q-1)\sigma_\epsilon^2/rpq$; the covariance of two $\hat{\beta}$'s is $-\sigma_\epsilon^2/rpq$; the variance of $\widehat{\alpha\beta}_{ij}$ is $(pq - p - q + 1)\sigma_\epsilon^2/rpq$; and the covariance of two $\widehat{\alpha\beta}$'s depends upon the two items

concerned. For one i subscript in common the covariance between two $\widehat{\alpha\beta}_{ij}$'s is $-\sigma_\epsilon^2(p-1)/rpq$; for one j subscript in common the covariance is $-\sigma_\epsilon^2(q-1)/rpq$; and for no subscripts in common the covariance is $+\sigma_\epsilon^2/rpq$. Therefore, the variance of a difference between two $\widehat{\alpha\beta}$'s with one i subscript in common is $2\sigma_\epsilon^2(p-1)/rp$; with one j subscript in common the variance is $2\sigma_\epsilon^2(q-1)/rq$; and with no subscripts in common the variance is $2\sigma_\epsilon^2(pq - p - q)/rpq$.

In obtaining the following expectations of the various sums of squares, it is assumed that the ρ_g, α_i, β_j, $\alpha\beta_{ij}$, and ϵ_{gij} are random variates from an infinite population with mean zero and variances σ_ρ^2, σ_α^2, σ_β^2, $\sigma_{\alpha\beta}^2$, and σ_ϵ^2, respectively. Likewise, it is assumed that all effects are additive and independent and that the linear model given by equation (VIII-1) is appropriate. As with previous expectations, no assumptions involving normality are required. However, the assumption of normality usually comes in when one starts testing hypotheses and utilizing the variance components in other ways, such as estimating average genetic advance and heritability.

The expectation of the total sum of squares corrected for the mean is equal to

$$E\left[\sum_{g=1}^{r}\sum_{i=1}^{p}\sum_{j=1}^{q} X_{gij}^2 - \frac{X_{...}^2}{rpq}\right] = E[\sum\sum\sum(\mu + \rho_g + \alpha_i + \beta_j + \alpha\beta_{ij} + \epsilon_{gij})^2$$

$$- \frac{1}{rpq}(rpq\mu + pq\sum\rho_g + qr\sum\alpha_i + pr\sum\beta_j + r\sum\sum\alpha\beta_{ij} + \sum\sum\sum\epsilon_{gij})^2]$$

$$= pqr(\mu^2 + \sigma_\rho^2 + \sigma_\alpha^2 + \sigma_\beta^2 + \sigma_{\alpha\beta}^2 + \sigma_\epsilon^2) - (pqr\mu^2 + pq\sigma_\rho^2 + qr\sigma_\alpha^2$$

$$+ pr\sigma_\beta^2 + r\sigma_{\alpha\beta}^2 + \sigma_\epsilon^2) = pq(r-1)\sigma_\rho^2 + qr(p-1)\sigma_\alpha^2 + pr(q-1)\sigma_\beta^2$$

$$+ r(pq-1)\sigma_{\alpha\beta}^2 + (pqr-1)\sigma_\epsilon^2, \tag{VIII-16}$$

with $rpq - 1$ degrees of freedom.

The expectation of the replicate sum of squares is

$$E\left[\sum\frac{X_{g..}^2}{pq} - \frac{X_{...}^2}{pqr}\right]$$

$$= E\left[\frac{1}{pq}\sum_g(pq\mu + pq\rho_g + q\sum\alpha_i + p\sum\beta_j + \sum\sum\alpha\beta_{ij} + \sum_i\sum_j\epsilon_{gij})^2 - \frac{X_{...}^2}{rpq}\right]$$

$$= rpq\mu^2 + rpq\sigma_\rho^2 + rq\sigma_\alpha^2 + rp\sigma_\beta^2 + r\sigma_{\alpha\beta}^2 + r\sigma_\epsilon^2$$

$$- E[X_{...}^2/rpq] = pq(r-1)\sigma_\rho^2 + (r-1)\sigma_\epsilon^2, \tag{VIII-17}$$

with $(r - 1)$ degrees of freedom.

The expectation of the sum of squares attributable to the factor a is

$$E\left[\sum\frac{X_{.i.}^2}{rq} - \frac{X_{...}^2}{rpq}\right] = (p-1)(\sigma_\epsilon^2 + r\sigma_{\alpha\beta}^2 + rq\sigma_\alpha^2), \tag{VIII-18}$$

with $(p - 1)$ degrees of freedom, and the expectation of the interaction sum of squares is

$$E\left[\sum\sum\frac{X_{.ij}^2}{r} - \sum\frac{X_{.i.}^2}{rq} - \sum\frac{X_{..j}^2}{rp} + \frac{X_{...}^2}{rpq}\right]$$

$$= (pq - p - q + 1)(\sigma_\epsilon^2 + r\sigma_{\alpha\beta}^2), \qquad \text{(VIII-19)}$$

with $(pq - p - q + 1)$ degrees of freedom. The remainder of the expectations are obtained from table VIII-5 and are presented in table VIII-6.

As stated before, the infinite model may be inappropriate for certain factorial experiments. Therefore, suppose that factors a and b have only p and q levels, respectively, and that $\sum\alpha_i = 0 = \sum\beta_j$; this also means that $\sum_i \alpha\beta_{ij} = 0 = \sum_j \alpha\beta_{ij}$. In this case, let $E\alpha_i^2 = \alpha_i^2$, $E\beta_j^2 = \beta_j^2$, and $E(\alpha\beta_{ij})^2 = (\alpha\beta_{ij})^2$. With these restrictions on equation (VIII-1) the expectations of the sums of squares are obtained as before. However, the form of the expectation is quite different. From the coefficients of the various components in table VIII-5 the expectations of the various mean squares are obtained (table VIII-6). In this case, it will be observed that the error mean square is appropriate for testing hypotheses about the existence of $\sum\alpha_i^2$, $\sum\beta_j^2$, and $\sum\sum(\alpha\beta_{ij})^2$. For the infinite model the $A \times B$ mean square is used to test hypotheses concerning σ_α^2 and σ_β^2.

VIII-5.2 THE $p \times q \times k$ FACTORIAL IN A RANDOMIZED COMPLETE BLOCK DESIGN

For a $p \times q \times k$ factorial arranged in a randomized complete block design, we shall assume that the yield of the $gijh$th experimental unit may be expressed by the following linear equation:

$$X_{gijh} = \mu + \rho_g + \alpha_i + \beta_j + \alpha\beta_{ij} + \gamma_h + \alpha\gamma_{ih} + \beta\gamma_{jh} + \alpha\beta\gamma_{ijh} + \epsilon_{gijh}, \qquad \text{(VIII-20)}$$

where μ = population mean, α_i = effect of the ith level of factor a, β_j = effect of the jth level of factor b, $\alpha\beta_{ij}$ = effect of the ith level of factor a at the jth level of factor b, γ_h = effect of hth level of factor c, $\alpha\gamma_{ih}$ = effect of ith level of factor a at the hth level of factor c, $\beta\gamma_{jh}$ = effect of jth level of factor b at the hth level of factor c, $\alpha\beta\gamma_{ijh}$ = effect of ith level of factor a at the jth level of factor b and the hth level of factor c, and ϵ_{gijh} = the random component of error variation associated with the $gijh$th unit.

If we assume that the ρ_g, α_i, β_j, $\alpha\beta_{ij}$, γ_h, $\alpha\gamma_{ih}$, $\beta\gamma_{jh}$, $\alpha\beta\gamma_{ijh}$, and ϵ_{gijh} are random variables independently distributed with zero mean and with variances σ_ρ^2, σ_α^2, σ_β^2, $\sigma_{\alpha\beta}^2$, σ_γ^2, $\sigma_{\alpha\gamma}^2$, $\sigma_{\beta\gamma}^2$, $\sigma_{\alpha\beta\gamma}^2$, and σ_ϵ^2, respectively, the expectations of the various sums of squares and mean squares are as given in tables VIII-7 and VIII-8 for the infinite model. If, on the other hand, we assume that these

TABLE VIII-5. Coefficients of components for a p $\times$ q factorial in a randomized complete block design

Sum of squares	Component (infinite model)						Component (finite model)					
	μ^2	σ_ρ^2	σ_α^2	σ_β^2	$\sigma_{\alpha\beta}^2$	σ_ϵ^2	μ^2	σ_ρ^2	$\Sigma\alpha_i^2$	$\Sigma\beta_j^2$	$\Sigma\Sigma(\alpha\beta_{ij})^2$	σ_ϵ^2
(1) $\Sigma\Sigma\Sigma X^2_{gij}$	pqr	pqr	pqr	pqr	pqr	pqr	pqr	pqr	qr	pr	r	pqr
(2) $X^2_{...}/pqr$	"	pq	qr	pr	1	1	"	pq	0	0	0	1
(3) $\Sigma X^2_{g..}/pq$	"	pqr	qr	"	r	r	"	pqr	0	0	0	r
(4) $\Sigma X^2_{.i.}/rq$	"	pq	pqr	"	pr	p	"	pq	qr	0	0	p
(5) $\Sigma X^2_{..j}/rp$	"	"	qr	pqr	qr	q	"	"	0	pr	0	q
(6) $\Sigma\Sigma X^2_{.ij}/r$	"	"	pqr	"	pqr	pq	"	"	qr	"	r	pq

TABLE VIII-6. Expectations of mean squares for a p $\times$ q factorial in a randomized complete block design

Source of variation	df	Expectation of mean square	
		Infinite model	Finite model
Replicate	r - 1	$\sigma_\epsilon^2 + pq\sigma_\rho^2$	$\sigma_\epsilon^2 + pq\sigma_\rho^2$
A	p - 1	$\sigma_\epsilon^2 + r\sigma_{\alpha\beta}^2 + rq\sigma_\alpha^2$	$\sigma_\epsilon^2 + qr\Sigma\alpha_i^2/(p - 1)$
B	q - 1	$\sigma_\epsilon^2 + r\sigma_{\alpha\beta}^2 + rp\sigma_\beta^2$	$\sigma_\epsilon^2 + pr\Sigma\beta_j^2/(q - 1)$
A x B	(p - 1)(q - 1)	$\sigma_\epsilon^2 + r\sigma_{\alpha\beta}^2$	$\sigma_\epsilon^2 + r\Sigma\Sigma(\alpha\beta_{ij})^2/(p - 1)(q - 1)$
Error	(r - 1)(pq - 1)	σ_ϵ^2	σ_ϵ^2
Total	rpq - 1	-	-
Correction for mean	1	-	-

are finite populations composed of the particular levels used in this experiment, then $\sum \alpha_i = \sum \beta_j = \sum \gamma_k = \sum_{i \text{ or } j} \alpha\beta_{ij} = \sum_{i \text{ or } k} \alpha\gamma_{ik} = \sum_{j \text{ or } k} \beta\gamma_{jk} = \sum_{i,j \text{ or } k} \alpha\beta\gamma_{ijk} = 0$, and the expectations of the various sums of squares are as represented in tables VIII-7 and VIII-8 for the finite model. The error mean square is appropriate for testing hypotheses about the existence of the various effects for the finite model but not for the infinite model.

VIII-5.3 ANALYSIS FOR ALL POSSIBLE SINGLE CROSSES AMONG *k* LINES IN A RANDOMIZED COMPLETE BLOCK DESIGN[1]

In genetic experiments, it may be desirable to estimate the variance components due to variation among k lines of a strain when used as males and when used as females, or of crosses and of their reciprocals.[2] In addition, the variation due to the interaction of the ith line crossed with the jth line may be of interest. Suppose that the randomized complete block design is used and that the yield of the gijth experimental unit may be represented by the linear model,

$$X_{gij} = \mu + \rho_g + \alpha_i + \beta_j + \alpha\beta_{ij} + \epsilon_{gij}, \qquad \text{(VIII-21)}$$

where $g = 1, 2, \cdots, r$; $i, j = 1, 2, \cdots, k$, except that the combinations ij for $i = j$ are omitted, resulting in $k^2 - k = k(k - 1)$ crosses; μ = population mean; ρ_g = effect due to gth replicate; α_i = effect due to the ith line; β_j = effect due to jth line; $\alpha\beta_{ij}$ = effect due to ith line being crossed with the jth line = nicking effect = effect due to dominance and epistasis; and ϵ_{gij} = random component associated with measurement of X_{gij}. To be certain that the treatments are understood, they are presented below in tabular form. The coefficients in the table represent the number of observations on each of the treatments. There are no lines crossed with themselves, or selfed.

Female lines	Male lines				
	1	2	3	...	k
1	0	r	r	...	r
2	r	0	r	...	r
3	r	r	0	...	r
.	.	.	.	.	.
.	.	.	.	...	.
.	.	.	.	.	.
k	r	r	r	...	0

[1]The proof in this section was developed by C. R. Henderson, Cornell University, and is reproduced through his courtesy.

[2]A similar situation has arisen in psychological work where subjects give a test to each other but cannot give the test to themselves.

TABLE VIII-7. Coefficients of components for a p × q × k factorial in a randomized complete block design

	Component (infinite model)									
	μ^2	σ_ρ^2	σ_α^2	σ_β^2	$\sigma_{\alpha\beta}^2$	σ_γ^2	$\sigma_{\alpha\gamma}^2$	$\sigma_{\beta\gamma}^2$	$\sigma_{\alpha\beta\gamma}^2$	σ_ϵ^2
$\Sigma\Sigma\Sigma\Sigma X^2_{gijh}$	rpqk	rpqk	rpqk	rpqk	rpqk	rpqk	rpqk	rpqk	rpqk	rpqk
$X^2_{....}/rpqk$	"	pqk	rqk	rpk	rk	rpq	rq	rp	r	1
$\Sigma X^2_{g...}/pqk$	"	rpqk	"	"	"	"	"	"	"	r
$\Sigma X^2_{.i..}/rqk$	"	pqk	rpqk	"	rpk	"	rpq	"	rp	p
$\Sigma X^2_{..j.}/rpk$	"	"	rqk	rpqk	rqk	"	rq	rpq	rq	q
$\Sigma\Sigma X^2_{.ij.}/rk$	"	"	rpqk	"	rpqk	"	rpq	"	rpq	pq
$\Sigma X^2_{...h}/rpq$	"	"	rqk	rpk	rk	rpqk	rqk	rpk	rk	k
$\Sigma\Sigma X^2_{.i.h}/rq$	"	"	rpqk	"	rpk	"	rpqk	"	rpk	pk
$\Sigma\Sigma X^2_{..jh}/rp$	"	"	rqk	rpqk	rqk	"	rqk	rpqk	rqk	qk
$\Sigma\Sigma\Sigma X^2_{.ijh}/r$	"	"	rpqk	"	rpqk	"	rpqk	"	rpqk	pqk
	Component (finite model)									
	μ^2	σ_ρ^2	$\Sigma\alpha_i^2$	$\Sigma\beta_j^2$	$\Sigma\Sigma(\alpha\beta_{ij})^2$	$\Sigma\gamma_h^2$	$\Sigma\Sigma(\alpha\gamma_{ih})^2$	$\Sigma\Sigma(\beta\gamma_{jh})^2$	$\Sigma\Sigma\Sigma(\alpha\beta\gamma_{ijh})^2$	σ_ϵ^2
$\Sigma\Sigma\Sigma\Sigma X^2_{gijh}$	rpqk	rpqk	rqk	rpk	rk	rpq	rq	rp	r	rpqk
$X^2_{....}/rpqk$	"	pqk	0	0	0	0	0	0	0	1
$\Sigma X^2_{g...}/pqk$	"	rpqk	0	0	0	0	0	0	0	r
$\Sigma X^2_{.i..}/rqk$	"	pqk	rqk	0	0	0	0	0	0	p
$\Sigma X^2_{..j.}/rpk$	"	"	0	rpk	0	0	0	0	0	q
$\Sigma\Sigma X^2_{.ij.}/rk$	"	"	rqk	rpk	rk	0	0	0	0	pq
$\Sigma X^2_{...h}/rpq$	"	"	0	0	0	rpq	0	0	0	k
$\Sigma\Sigma X^2_{.i.h}/rq$	"	"	rqk	0	0	"	rq	0	0	pk
$\Sigma\Sigma X^2_{..jh}/rp$	"	"	0	rpk	0	"	0	rp	0	qk
$\Sigma\Sigma\Sigma X^2_{.ijh}/r$	"	"	rqk	rpk	rk	"	rq	"	r	pqk

TABLE VIII-8. Expectation of mean squares for a p × q × k factorial in a randomized complete block design

Infinite model

Source of variation	df	Expectation of mean square
Replicate	$(r-1)$	$\sigma_\epsilon^2 + pqk\sigma_\rho^2$
A	$(p-1)$	$\sigma_\epsilon^2 + r\sigma_{\alpha\beta\gamma}^2 + rq\sigma_{\alpha\gamma}^2 + rk\sigma_{\alpha\beta}^2 + rqk\sigma_\alpha^2$
B	$(q-1)$	$\sigma_\epsilon^2 + r\sigma_{\alpha\beta\gamma}^2 + rp\sigma_{\beta\gamma}^2 + rk\sigma_{\alpha\beta}^2 + rpk\sigma_\beta^2$
A x B	$(p-1)(q-1)$	$\sigma_\epsilon^2 + r\sigma_{\alpha\beta\gamma}^2 + rk\sigma_{\alpha\beta}^2$
C	$(k-1)$	$\sigma_\epsilon^2 + r\sigma_{\alpha\beta\gamma}^2 + rp\sigma_{\beta\gamma}^2 + rq\sigma_{\alpha\gamma}^2 + rpq\sigma_\gamma^2$
A x C	$(p-1)(k-1)$	$\sigma_\epsilon^2 + r\sigma_{\alpha\beta\gamma}^2 + rq\sigma_{\alpha\gamma}^2$
B x C	$(q-1)(k-1)$	$\sigma_\epsilon^2 + r\sigma_{\alpha\beta\gamma}^2 + rp\sigma_{\beta\gamma}^2$
A x B x C	$(p-1)(q-1)(k-1)$	$\sigma_\epsilon^2 + r\sigma_{\alpha\beta\gamma}^2$
Error	$(r-1)(pqk-1)$	σ_ϵ^2

Finite model

Source of variation	df	Expectation of mean square
Replicate	$(r-1)$	$\sigma_\epsilon^2 + pqk\sigma_\rho^2$
A	$(p-1)$	$\sigma_\epsilon^2 + rqk\Sigma\alpha_i^2/(p-1)$
B	$(q-1)$	$\sigma_\epsilon^2 + rpk\Sigma\beta_j^2/(q-1)$
A x B	$(p-1)(q-1)$	$\sigma_\epsilon^2 + rk\Sigma\Sigma(\alpha\beta_{ij})^2/(p-1)(q-1)$
C	$(k-1)$	$\sigma_\epsilon^2 + rpq\Sigma\gamma_h^2/(k-1)$
A x C	$(p-1)(k-1)$	$\sigma_\epsilon^2 + rq\Sigma\Sigma(\alpha\gamma_{ih})^2/(p-1)(k-1)$
B x C	$(q-1)(k-1)$	$\sigma_\epsilon^2 + rp\Sigma\Sigma(\beta\gamma_{jh})^2/(q-1)(k-1)$
A x B x C	$(p-1)(q-1)(k-1)$	$\sigma_\epsilon^2 + r\Sigma\Sigma\Sigma(\alpha\beta\gamma_{ijh})^2/(p-1)(q-1)(k-1)$
Error	$(r-1)(pqk-1)$	σ_ϵ^2

If the equations resulting from partial differentiation of the error sum of squares with respect to the various effects are set equal to zero, the following normal equations are obtained:

$$rk(k-1)\hat{\mu}+k(k-1)\sum\hat{\rho}_g+r(k-1)\left(\sum\hat{\alpha}_i+\sum\hat{\beta}_j\right)+r\sum\widehat{\alpha\beta}_{ij}=X_{...}, \qquad \text{(VIII-22)}$$

$$k(k-1)(\hat{\mu}+\hat{\rho}_g)+(k-1)\left(\sum\hat{\alpha}_i+\sum\hat{\beta}_j\right)+\sum_{i\neq j}\widehat{\alpha\beta}_{ij}=X_{g..}, \qquad \text{(VIII-23)}$$

$$r(k-1)(\hat{\mu}+\hat{\alpha}_i)+(k-1)\sum\hat{\rho}_g+r\sum_{i\neq j}\left(\hat{\beta}_j+\widehat{\alpha\beta}_{ij}\right)=X_{.i.}, \qquad \text{(VIII-24)}$$

$$r(k-1)(\hat{\mu}+\hat{\beta}_j)+(k-1)\sum\hat{\rho}_g+r\sum_{i\neq j}\left(\hat{\alpha}_i+\widehat{\alpha\beta}_{ij}\right)=X_{..j}, \qquad \text{(VIII-25)}$$

and

$$r(\hat{\mu} + \hat{\alpha}_i + \hat{\beta}_j + \widehat{\alpha\beta}_{ij}) + \sum \hat{\rho}_g = X_{\cdot ij}. \tag{VIII-26}$$

If we impose the restrictions that

$$\sum \hat{\rho}_g = \sum \hat{\alpha}_i = \sum \hat{\beta}_j = \sum_{\substack{i \\ i \neq j}} \widehat{\alpha\beta}_{ij} = \sum_{\substack{j \\ j \neq i}} \widehat{\alpha\beta}_{ij} = 0, \tag{VIII-27}$$

the least squares estimates of the various effects are equal to

$$\hat{\mu} = X_{\cdots}/rk(k-1) = \bar{x}, \tag{VIII-28}$$

$$\hat{\rho}_g = \frac{X_{g\cdot\cdot}}{k(k-1)} - \bar{x} = \bar{x}_{g\cdot\cdot} - \bar{x}, \tag{VIII-29}$$

$$\hat{\alpha}_i = \{(k-1)X_{\cdot i\cdot} + X_{\cdot\cdot i} - X_{\cdots}\}/rk(k-2), \tag{VIII-30}$$

$$\hat{\beta}_j = \{(k-1)X_{\cdot\cdot j} + X_{\cdot j\cdot} - X_{\cdots}\}/rk(k-2), \tag{VIII-31}$$

and

$$\widehat{\alpha\beta}_{ij} = \frac{X_{\cdot ij}}{r} - \hat{\mu} - \hat{\alpha}_i - \hat{\beta}_j. \tag{VIII-32}$$

The expectations of the various mean squares are presented in table VIII-10 and are obtained from the table of coefficients of the various components (table VIII-9). Because of the nonorthogonality of the data the sums of squares for A, B, and $A \times B$ are in a slightly different form than previous sums of squares. If the method for unequal numbers analysis described by Snedecor [273, Ch. 11] were followed, we would obtain this form of analysis. The sum of squares for the $A \times B$ interaction is the sum of squares for the $k^2 - k$ crosses minus the sum of squares due to the $\hat{\alpha}_i$ and $\hat{\beta}_j$ constants. Likewise, the sum of squares for $\hat{\alpha}_i$ constants is the sum of squares for the $\hat{\alpha}_i$ and $\hat{\beta}_j$ constants, $\sum \hat{\alpha}_i X_{\cdot i\cdot} + \sum \hat{\beta}_j X_{\cdot\cdot j}$, minus the sum of squares due to fitting the $\hat{\beta}_j'$ constants, assuming no $\hat{\alpha}_i$, $\sum \frac{X_{\cdot\cdot j}^2}{r(k-1)} - \frac{X_{\cdots}^2}{rk(k-1)}$. If the other k treatments, i.e., the selfs $= a_ib_i$, were included in the experiment, the analysis would reduce to that presented in section VIII-5.1.

VIII-5.4 ANALYSIS FOR $k(k-1)/2$ POSSIBLE CROSSES AMONG k LINES IN A RANDOMIZED COMPLETE BLOCK DESIGN

Some breeding experiments are conducted using the $k(k-1)/2$ crosses among a group of k lines. Because the reciprocal crosses are assumed to be identical, they are not included in the experiment [111, 278]. If the design is a randomized complete block, the yield of an individual experimental unit is assumed to be represented by the linear equation,

$$X_{gij} = \mu + \rho_g + \alpha_i + \alpha_j + \alpha\beta_{ij} + \epsilon_{gij}, \tag{VIII-33}$$

where $g = 1, 2, \cdots, r$; $i \neq j = 1, 2, \cdots, k$; the double subscript ij runs from $i < j = 2, 3, \cdots, k$; α_i = effect due to ith line; α_j = effect due to jth line;

TABLE VIII-9. Coefficients of components for $k^2 - k$ treatments in a randomized complete block design

Sum of squares	Components (infinite model)					
	μ^2	σ_ρ^2	σ_α^2	σ_β^2	$\sigma_{\alpha\beta}^2$	σ_e^2
$\Sigma\Sigma\Sigma X^2_{gij}$	$rk(k-1)$	$rk(k-1)$	$rk(k-1)$	$rk(k-1)$	$rk(k-1)$	$rk(k-1)$
$X^2_{\ldots}/rk(k-1)$	"	$k(k-1)$	$r(k-1)$	$r(k-1)$	r	1
$\Sigma X^2_{g..}/k(k-1)$	"	$rk(k-1)$	"	"	"	r
$\sum_i \hat{\alpha}_i X_{.i.} + \sum_j \hat{\beta}_j X_{..j}$	0	0	$r(k-1)^2$	$r(k-1)^2$	$2r(k-1)$	$2(k-1)$
$\Sigma X^2_{.i.}/r(k-1)$	$rk(k-1)$	$k(k-1)$	$rk(k-1)$	rk	rk	k
$\Sigma X^2_{..j}/r(k-1)$	"	"	rk	$rk(k-1)$	"	"
$\sum_{i \neq j} X^2_{.ij}/r$	"	"	$rk(k-1)$	"	$rk(k-1)$	$k(k-1)$

TABLE VIII-10. Expectation of mean squares of $k^2 - k$ treatments in a randomized complete block design

Source of variation	df	ss	Expectation of mean square
Replicate	$(r - 1)$	$\frac{\Sigma X^2_{g..}}{k(k-1)} - \frac{X^2_{...}}{rk(k-1)}$	$\sigma_\epsilon^2 + k(k-1)\,\sigma_\rho^2$
Among a lines = A	$(k - 1)$	$\Sigma\hat{\alpha}_i X_{.i.} + \Sigma\hat{\beta}_j X_{..j} - \frac{\Sigma X^2_{..j}}{r(k-1)} + \frac{X^2_{...}}{rk(k-1)}$	$\sigma_\epsilon^2 + r\sigma_{\alpha\beta}^2 + \frac{rk(k-2)}{k-1}\,\sigma_\alpha^2$
Among b lines = B	$(k - 1)$	$\Sigma\hat{\alpha}_i X_{.i.} + \Sigma\hat{\beta}_j X_{..j} - \frac{\Sigma X^2_{.i.}}{r(k-1)} + \frac{X^2_{...}}{rk(k-1)}$	$\sigma_\epsilon^2 + r\sigma_{\alpha\beta}^2 + \frac{rk(k-2)}{k-1}\,\sigma_\beta^2$
A x B	$(k^2 - 3k + 1)$	$\Sigma_i\Sigma_{i \neq j}\frac{X^2_{.ij}}{r} - \Sigma\hat{\alpha}_i X_{.i.} - \Sigma\hat{\beta}_j X_{..j} - \frac{X^2_{...}}{rk(k-1)}$	$\sigma_\epsilon^2 + r\sigma_{\alpha\beta}^2$
Error	$(k^2 - k - 1)(r - 1)$	$\Sigma\Sigma\Sigma X^2_{gij} - \frac{\Sigma X^2_{g..}}{k(k-1)} - \Sigma_i\Sigma_{i \neq j}\frac{X^2_{.ij}}{r} + \frac{X^2_{...}}{rk(k-1)}$	σ_ϵ^2

$\alpha\beta_{ij}$ = effect due to the cross of the ith and jth lines; and ϵ_{gij} = random component for the gijth observation. The sums of squares and expectations of mean squares are presented in table VIII-11, assuming that these lines constitute a random sample from a large population. The sum of squares for the $k(k-1)/2$ crosses may also be obtained as

$$\frac{\sum_{i<j=2}^{k} 4\left(\frac{k(k-1)}{2}X_{.ij} - X_{...}\right)^2}{rk^2(k-1)^2}, \tag{VIII-34}$$

with $\frac{k(k-1)}{2} - 1 = (k-2)(k+1)/2$ degrees of freedom.

The equations for the various totals are

$$\frac{k(k-1)}{2}(r\mu + \sum \rho_g) + r(k-1)\sum_i \alpha_i + r\sum_{i<j=2}^{k} \alpha\beta_{ij} + \sum_{g=1}^{r}\sum_{i<j=2}^{k} \epsilon_{gij} = X_{...}, \tag{VIII-35}$$

$$r(k-1)\mu + (k-1)\sum \rho_g + r(k-1)\alpha_i + r\sum_{j=1\neq i}^{k}(\alpha_j + \alpha\beta_{ij}) + \sum_{g=1}^{r}\sum_{j=1\neq i}^{k} \epsilon_{gij} = X_{.i.}, \tag{VIII-36}$$

$$r(\mu + \alpha_i + \alpha_j + \alpha\beta_{ij}) + \sum_g (\rho_g + \epsilon_{gij}) = X_{.ij}, \tag{VIII-37}$$

and

$$\frac{k(k-1)}{2}(\mu + \rho_g) + (k-1)\sum \alpha_i + \sum_{i<j=2}^{k} (\alpha\beta_{ij} + \epsilon_{gij}) = X_{g..}. \tag{VIII-38}$$

When these totals are used, the expectations are obtained as for previous examples.

In addition, estimates of specific and general combining ability [278] for the particular set of k lines used in the experiment might be desired. If so, we may assume that

$$\sum_{j=1\neq i}^{k} \alpha\beta_{ij} = 0 = \sum \alpha_i. \tag{VIII-39}$$

From equation (VIII-39), $-\alpha_i = \sum_{j=1\neq i}^{k} \alpha_j$. With these restrictions, equations (VIII-35) and (VIII-36) now become

$$\frac{k(k-1)}{2}(r\mu + \sum \rho_g) + \sum_g \sum_{i<j} \epsilon_{gij} = \sum \frac{X_{.i.}}{2} = X_{...} \tag{VIII-40}$$

and

$$(k-1)(r\mu + \sum \rho_g) + r(k-2)\alpha_i + \sum_g \sum_{j=1\neq i}^{k} \epsilon_{gij} = X_{.i..} \tag{VIII-41}$$

The difference $\left(\frac{k}{2}X_{.i.} - X_{...}\right)$ squared has the expectation:

$$\frac{r^2(k-2)^2}{4}k^2\alpha_i^2 + \frac{rk(k-1)(k-2)}{4}\sigma_\epsilon^2. \tag{VIII-42}$$

TABLE VIII-11. Sum of squares and expectation of mean squares for $k(k-1)/2$ crosses in r replicates of a randomized complete block design

Source of variation	df	ss	Expectation of mean square
Replicate	$(r-1)$	$\Sigma \frac{2X^2_{g..}}{k(k-1)} - \frac{2X^2_{...}}{rk(k-1)}$	$\sigma_\epsilon^2 + \frac{k(k-1)}{2}\sigma_\rho^2$
Among k lines	$(k-1)$	$\frac{\Sigma 4\left(\frac{kX_{.i.}}{2} - X_{...}\right)^2}{rk^2(k-2)}$	$\sigma_\epsilon^2 + r\sigma_{\alpha\beta}^2 + r(k-2)\sigma_\alpha^2$
Within lines	$k(k-3)/2$	Subtraction of above from ss	$\sigma_\epsilon^2 + r\sigma_{\alpha\beta}^2$
Error	$(k^2-k-2)(r-1)/2$	$\sum_g \sum_{i<j} X^2_{gij} - \Sigma \frac{2X^2_{g..}}{k(k-1)} - \sum_{i<j} \frac{X^2_{.ij}}{r} + \frac{2X^2_{...}}{rk(k-1)}$	σ_ϵ^2
Total	$\frac{rk(k-1)}{2} - 1$	$\sum_g \sum_{i<j} X^2_{gij} - \frac{2X^2_{...}}{rk(k-1)}$	
Among $\frac{k(k-1)}{2}$ crosses	$\frac{k(k-1)}{2} - 1$	$\sum_{i<j} \frac{X^2_{.ij}}{r} - \frac{2X^2_{...}}{rk(k-1)}$	$\sigma_\epsilon^2 + r\sigma_{\alpha\beta}^2 + \frac{2r(k-1)}{k+1}\sigma_\alpha^2$

Therefore, we estimate $\hat{\sigma}_i^2$ as

$$\hat{\sigma}_i^2 = \frac{(k-1)}{k(k-2)}\left\{\frac{4\left(\frac{k}{2}X_{\cdot i\cdot} - X_{\cdots}\right)^2}{r^2k(k-1)(k-2)} - \frac{\hat{\sigma}_e^2}{r}\right\}. \qquad \text{(VIII-43)}$$

This is the form of the estimate of the variance for general combining ability for the *i*th line as presented by Sprague and Tatum. Strictly speaking, $\hat{\sigma}_i^2$ is not a variance as ordinarily defined.

If we make use of equation (VIII-39), the sum of squares among the $(k-1)$ crosses in which parent i is involved has the expectation:

$$E\left[\sum_{j=1\neq i}^{k}\frac{\left((k-2)X_{\cdot ij} - X_{\cdot i\cdot} - X_{\cdot\cdot j} + 2X_{\cdots}/(k-1)\right)^2}{r(k-2)(k-3)}\right]$$

$$= r(k-2)\left\{\frac{\sigma_e^2}{r} + \frac{(k-1)}{(k-3)}\sigma_{\alpha\beta}^2\right\}, \qquad \text{(VIII-44)}$$

with $(k-2)$ degrees of freedom.

Considering the $(k-1)$ lines crossed with the *i*th line as a *sample* of lines, the variance for the mean of the *i*th line is

$$E\left[\frac{X_{\cdot i\cdot}}{r(k-1)}\right]^2 - \left[E\frac{X_{\cdot i\cdot}}{r(k-1)}\right]^2 = \frac{1}{(k-1)}\left\{\frac{\sigma_e^2}{r} + \sigma_{\alpha\beta}^2\right\}, \qquad \text{(VIII-45)}$$

since $\sum \rho_i = 0$ and $\sum_{j=1\neq i}^{k} \alpha_j = -\alpha_i$.

CHAPTER IX

Confounding in Factorial Experiments

IX-1 Confounding—Use and Types

The number of treatment combinations increases rapidly as the number of factors or as the number of levels of each factor is increased. For experiments containing over ten to twelve treatments, the latin square design becomes impractical, and as the number of treatments increases it becomes exceedingly difficult to select replicates for a randomized complete block design which are relatively homogeneous. Because the variation within replicates tends to increase as the replicate size increases, resulting in a larger experimental error variance, it is desirable to keep block sizes small. In order to retain relatively small block sizes for a large number of treatments, only a portion of the treatments may be included in a small block. The resulting blocks are called *incomplete blocks* [324]. By a device known as *confounding* [126, 318, 319, 324] the necessity of including all treatments in each block (or row and column in a latin square) is side-stepped. The whole block or replicate is divided into the desired number of incomplete blocks. Then, by removing the variation among incomplete blocks (freed of treatment effects) within replicates the experimental error mean square is often much smaller than what it would have been for a randomized complete block design. The smaller error variance results in more precise comparisons among some treatments. The segregation of the blocks within replicate sum of squares results in a decrease in the degrees of freedom associated either with the error or treatment sums of squares or both. This means that, in some cases, information among some treatment contrasts may be mixed up with or *confounded* with the incomplete block differences. If a particular treatment comparison is of little or no value, then this feature may be included in the *incomplete block design*. Indiscriminate confounding may result in complete loss of information on the comparisons or contrasts of greatest importance. This means that the experimenter should confound with incomplete block differences only those contrasts or comparisons of little or no importance.

If an effect is of little or no interest, it may be confounded with the incomplete block differences in all replicates. This system of confounding is known as *complete confounding*. For example, it may be desirable to have incomplete blocks of four plots in a 2^3 factorial experiment with three repli-

cates and the effect ABC may be of little or no importance. The treatments in an incomplete block will be one of these two groups:

$(ABC)_1$	$(ABC)_0$
001	000
010	011
100	101
111	110

Thus, the comparison between the incomplete blocks is also the comparison between $(ABC)_1$ and $(ABC)_0$.

If an effect is confounded with incomplete block differences in replicate I, a second effect in replicate II, and a third effect or one of the first two in replicate III, these effects are *partially confounded* with incomplete block differences; i.e., they are confounded with incomplete block differences in some replicates and unconfounded in others. Some information is available on all comparisons, but some comparisons are more accurately determined, since they are made in all replicates instead of being made in only a portion of the replicates.

The term *balanced confounding* has been defined as the incomplete block design for which all effects of a certain order, say all two-factor interactions, are confounded with incomplete block differences an equal number of times [60, 324]. In the present book, this type of design is defined as a *partially balanced* one, and the confounding is denoted as *balanced partial confounding.* If all two-factor and three-factor interactions are confounded with incomplete block differences, the design is still partially balanced. Likewise, if *all* effects are confounded with incomplete block differences an equal number of times, the design is said to be *balanced.* The addition of the term partially balanced is deemed necessary for the discussion of a group of incomplete block designs which are known as lattices [see Chapter XI; 177].

On the other hand, if all effects of a certain degree are confounded an unequal number of times, the resulting design is denoted as *unbalanced.* The type of confounding is denoted as *unbalanced partial confounding.* In several of the factorial experiments in which partial confounding has been used, the additional computations are not difficult. However, with certain other incomplete block designs, additional computations and complexities are encountered with unbalanced arrangements.

At this point, it should be noted that the experimenter may use more replicates rather than use confounding. The resulting standard error of a treatment mean may be made as small as one wishes simply by obtaining additional replicates. In some experiments (e.g., laboratory), it may be less costly to have more replicates of a randomized complete block design than to have fewer replicates of a confounded arrangement and to perform the additional computations required for the analysis. The experimenter should keep these points in mind when designing an experiment.

IX-2 Advantages and Disadvantages of Confounding

In order to utilize confounding and confounded arrangements in factorial experiments in an efficient manner, it is necessary to comprehend the purpose, the advantages, and the disadvantages of confounding. The purpose of confounding is to stratify the experimental material into homogeneous subgroups because the material is variable, because only a portion of the treatments can be carried out in a relatively homogeneous time interval or by a single technician, or because of some other experimental condition. It is desirable to eliminate extraneous sources of variation wherever possible. In some instances the extraneous variation may be removed by covariance and in others by stratification. The randomized complete block design may have excessive variation within the block unless some subgrouping is practiced. The number of treatment combinations may be too large to use a latin square. Hence, the next plausible thing to do is to subdivide the complete block into incomplete blocks containing only a portion of the treatments. The particular treatments allocated to an incomplete block are determined by the nature of the experimental material and the relative amount of information desired for the various contrasts among the treatments. In many cases, removal of the variation among incomplete blocks within a complete block greatly reduces the residual variation. However, if the experimenter has no idea of the amount of reduction to be obtained, no realistic decision can be made concerning the question of confounding. To obtain information on this, results from previous experiments may be summarized to estimate the gains due to confounding [319]; also, uniformity or blank trial data may be analyzed to compare the variability within incomplete blocks and the variability among incomplete blocks within replicates. For either method, it is recommended that several sets of data be available. In summarizing results from previous experiments, it is recommended that thirty to forty experiments be the minimum number for reliable estimates even though fewer, say fifteen to twenty, experiments will provide useful information. The optimum number of uniformity trails to use depends upon the differences among trials, but it is recommended that the experimenter use at least ten and that he compare the layouts in two perpendicular directions.

The disadvantages of confounding are the following:

(i) the confounded contrasts are replicated fewer times than are the other contrasts,
(ii) the calculational procedures are usually more difficult, and
(iii) considerable difficulties are encountered if the treatments interact with incomplete blocks.

No comparison should be completely confounded unless there is considerable evidence to show that the comparison is of little or no value or that the

effect is nonexistent. The experimenter should ascertain the relative importance of the various contrasts and partially confound the effects of least importance.

The additional computations involved depend upon the type of confounding used. The experimenter will be well advised to set up the analysis prior to running the experiment. From among the suitable confounded arrangements, he should select the simplest computationally. The time saved on computations can usually be well spent upon the interpretation of the experimental results. Also, the experimenter may save himself considerable time and anxiety if he writes out the analysis prior to conducting the experiment.

IX-3 Complete Confounding

A particular contrast may be of little or no interest and it may be decided to confound the contrast with differences among incomplete blocks. However, before doing so, the experimenter should be certain that this is desirable, and he should consider the available comparisons among the treatments.

One further point arises concerning the analysis of experiments in which one or more effects are completely confounded with incomplete block differences, and that is the adjustment of treatment means. *If it can be assumed that the completely confounded effects are equal to zero*, then adjustments should be made. However, it is felt that this is not usually a valid assumption and that adjustments are unnecessary. If the assumption that the confounded effect equals zero is incorrect and if the treatment means are adjusted assuming a zero effect, the resulting means are biased. The unconfounded effects are not affected by the adjustment, since they represent within-incomplete block comparisons.

IX-3.1 RANDOMIZATION

The sets of treatments should be randomly allocated to the incomplete blocks with a different random allocation for each replicate or complete block. The treatments within each incomplete block should be randomly allotted to the experimental units. A different random allotment is used for each incomplete block and for each replicate. If a latin square design is used with confounding, special rules are sometimes necessary. These are described in conjunction with the particular arrangement used.

By randomly allocating the sets of treatments to the incomplete blocks, some information is available on the effects that are completely confounded. If there is a differential response (see next section) of an effect in some incomplete blocks, this may cause other effects to be overestimated. This is a further reason for randomization of the sets of treatments to the incomplete blocks.

IX-3.2 COMPLETE CONFOUNDING IN THE 2^n SERIES

In some instances the higher-ordered interactions or even the main effects (see Chapter X) may have little or no meaning, and it may be decided to

confound these comparisons with incomplete block differences in all replicates. An example of complete confounding of the ABC interaction of a 2^3 factorial experiment with incomplete block differences in all three replicates is given below.

Replicate I

000	111
110	010
101	001
011	100
I_a	I_b

Replicate II

010	101
001	011
111	110
100	000
II_b	II_a

Replicate III

001	011
111	000
100	101
010	110
III_b	III_a

The treatments in incomplete blocks I_b, II_b, and III_b are those which make up $(ABC)_1$ and those in blocks I_a, II_a, and III_a make up $(ABC)_0$. The groups are allotted to the incomplete blocks at random, and the treatment combinations within each incomplete block are allotted to the plots at random. The comparison between incomplete block totals in a replicate also represents the contrast between $(ABC)_1$ and $(ABC)_0$. The partitioning of the degrees of freedom in the analysis of variance follows:

Source of variation	Degrees of freedom		Mean square
Replicates	2		
ABC	1		—
Replicates × ABC	2		E_b'
Treatments	6		
A		1	
B		1	
AB		1	
C		1	
AC		1	
BC		1	
Residual	12		E_e
Total	23		

The contrast of the levels $(ABC)_1$ and $(ABC)_0$ is not well determined, since only three replicates are available for the contrast and since the replicates × ABC mean square, which is the error term for testing the above comparison, is determined with only two degrees of freedom. Thus, most of the information on the ABC effect has been sacrificed at the expense of having blocks of four instead of blocks of eight plots. The increase in precision due to confounding (ignoring the difference in the degrees of freedom for r replicates of the incomplete and complete block designs) is obtained from the formula,

$$\frac{E_e'}{E_e} = \frac{(r-1+1)E_b' + [6+6(r-1)]E_e}{[r-1+1+6+6(r-1)]E_e} = \frac{E_b' + 6E_e}{7E_e}, \qquad \text{(IX-1)}$$

where E_b' and E_e are the two error mean squares in the previous example, r is the number of replicates, and the ABC interaction is not assumed negligible. In the event that the interaction is assumed negligible, the efficiency of the incomplete block relative to the randomized complete block design is equal to

$$\frac{E_e'}{E_e} = \frac{rE_b + [6 + 6(r-1)]E_e}{[r + 6 + 6(r-1)]E_e} = \frac{E_b + 6E_e}{7E_e}, \tag{IX-2}$$

where E_b is the incomplete blocks within replicate mean square. The experimenter should usually use the former measure of efficiency, since no assumptions are made relative to the magnitude of the confounded effect.

In the event that the groups of treatments making up the levels $(ABC)_1$ and $(ABC)_0$ are not allotted to the incomplete blocks at random, it is not permissible to partition the 5 degrees of freedom for the blocks sum of squares, since the design is systematic. Also, some difficulties may be encountered if the various error terms making up the residual mean square are not homogeneous. These have been discussed by Yates [319; 324, p. 32], and others [172; 175, sec. 14.5; 60, sec. 6.21]. A differential response of the factor a in the incomplete blocks with responses due to the factors b and c results in a large BC effect owing to the arrangement of the incomplete blocks. The possibility of this happening should not be overlooked in experiments, nor should it be overemphasized.

As a further variation of complete confounding of the ABC effect in the 2^3 factorial, Yates [324, sec. 8a and 8b] presents an unusual design using complete confounding and two latin squares. In the first plan [324, table 26], Yates gives the systematic arrangement for 2^3 treatments arranged in two 4×4 latin squares. The ABC effect is confounded with columns and BC with rows. As a variant of this design the ABC effect is confounded with squares; the four treatments making up $(ABC)_1$ are put in one 4×4 latin square and the remaining four in a second 4×4 latin square. The two 4×4 latin squares represent a random selection of two of the four standard squares; for example, the following two squares might have been chosen with four of the eight treatments in each square:

Square I

000	110	101	011
110	101	011	000
101	011	000	110
011	000	110	101

Square II

100	010	001	111
010	111	100	001
001	100	111	010
111	001	010	100

In accordance with the rules for randomizing latin squares the columns and all rows except the first row are randomly arranged. The next step is to interlace the squares by randomly allotting the members of the first pair of columns (or rows) to the first two "columns" (or "rows") and so forth for the remaining pairs. The resulting design then has eight columns and four rows; the arrangement might be of the following form:

001	011	000	111	101	100	110	010
100	000	110	001	011	010	101	111
111	101	011	010	110	001	000	100
010	110	101	100	000	111	011	001

The analysis of variance for the above design is

Source of variation	Degrees of freedom
Between squares (= ABC)	1
Among rows within squares	3 + 3 = 6
Among columns within squares	3 + 3 = 6
Among treatments (A, B, AB, C, AC, BC)	6
Error = residual	12
Total (corrected for the mean)	31

Other variants of confounding in latin squares are discussed in various places [e.g., 47, 60, 175; Chapter XV].

A 2^4 factorial experiment may be arranged in blocks of two, four, or eight treatments. Likewise, the 2^5 may be arranged in blocks of two, four, eight, or sixteen treatments and the process continued for larger values of n in the 2^n factorial series. If 2^n treatments are put into blocks of 2^s treatments, then $(2^{n-s} - 1)$ effects are confounded in each replicate. For complete confounding, the same treatments with a different randomization appear together in an incomplete block. In most cases, complete confounding is not advisable. If only one replicate of a 2^n factorial in blocks of 2^s is used and the higher-order interactions are used for experimental error [324, sec. 7; 60, sec. 6.31; 175, sec. 14.7; Chapter VII] then one of the schemes of confounding for one of the replicates as described by Yates [324, sec. 5] and others [11, 60, plans 6.1 to 6.6; 175, p. 261] may be used. For example, it may be desired to have 2^5 treatments in blocks of 2^3 plots. If it is desired to completely confound two three-factor interactions, say ABC and CDE, then the interaction of these two effects,

$ABC \times CDE = ABDE$, which is a four-factor interaction, is also confounded. The schematic arrangement of treatments in the four incomplete blocks is

Block									Effect
I	00000	00011	11000	11011	01101	01110	10101	10110	$(ABC)_0$, $(CDE)_0$, $(ABDE)_0$
II	00001	00010	11001	11010	01100	01111	10100	10111	$(ABC)_0$, $(CDE)_1$, $(ABDE)_1$
III	10000	10011	00101	00110	01000	01011	11101	11110	$(ABC)_1$, $(CDE)_0$, $(ABDE)_1$
IV	10001	10010	00100	00111	01001	01010	11100	11111	$(ABC)_1$, $(CDE)_1$, $(ABDE)_0$

The four incomplete blocks are assigned to the four sites at random, and the eight treatments within each block are randomly assigned to the eight experimental units in each incomplete block. If the unconfounded three-, four-, and five-factor interactions are used as an estimate of the experimental error, the breakdown of the 31 degrees of freedom is

Source of variation	Degrees of freedom
Blocks (= ABC, CDE, ABDE)	3
Main effects (A, B, C, D, E)	5
Two-factor interactions	10
Residual	13
Total (corrected for the mean)	31

Likewise, $s \times s$ latin squares may be used for 2^n factorials in which some effects are completely confounded. Yates [324, sec. 8] and Kempthorne [175, sec. 15.6] have discussed complete confounding in latin squares and have presented some plans [also, see Chapter XV].

Incomplete blocks of size 2 have an important place in the biological world. A leaf may be divided into half-leaves. The roasts from the left and right sides of an animal are naturally paired. The two arms or two legs of a patient are naturally paired. Likewise, identical twins in humans and animals form pairs of homogeneous experimental units. Since a wide range of experimental material occurs in pairs, the confounded designs with incomplete blocks of size 2 form an important class of designs. For larger blocks the experimenter may use litter mates or other homogeneous experimental material for an incomplete block.

Example IX-1. To illustrate the computational procedure for complete confounding, an example discussed by Yates [319] has been selected. The 2^3 treatments of two levels of nitrogen, n_0 and n_1, two levels of phosphate, p_0 and p_1, and two levels of potash, k_0 and k_1, in all combinations were applied to peas. The resulting yields of peas in pounds per plot are presented in table IX-1. The NPK effect was completely confounded with the differences among the incomplete blocks of four treatments. Three replicates were used. The totals, effect totals, and the analysis of variance are presented in table IX-1. The computations follow those described in Chapter VII for

TABLE IX-1. A 2^3 fertilizer experiment on peas (pounds/plot)

Replicate I				Replicate II				Replicate III			
(001)	45.5	(010)	44.2	(101)	49.8	(011)	48.8	(011)	53.2	(000)	56.0
(100)	62.0	(111)	48.8	(110)	52.0	(000)	51.5	(101)	57.2	(110)	59.0
(000)	46.8	(101)	57.0	(001)	55.5	(010)	56.0	(100)	69.5	(001)	55.0
(011)	49.5	(110)	62.8	(100)	59.8	(111)	58.5	(010)	62.8	(111)	55.8
			416.6				431.9				468.5

Effect	Treatment totals								Sum of		Total effect
	$X_{.000}$ 154.3	$X_{.100}$ 191.3	$X_{.010}$ 163.0	$X_{.110}$ 173.8	$X_{.001}$ 156.0	$X_{.101}$ 164.0	$X_{.011}$ 151.5	$X_{.111}$ 163.1	+'s	-'s	
Total	+	+	+	+	+	+	+	+	1317.0	0	1317.0
N	-	+	-	+	-	+	-	+	692.2	624.8	67.4
P	-	-	+	+	-	-	+	+	651.4	665.6	-14.2
NP	+	-	-	+	+	-	-	+	647.2	669.8	-22.6
K	-	-	-	-	+	+	+	+	634.6	682.4	-47.8
NK	+	-	+	-	-	+	-	+	644.4	672.6	-28.2
PK	+	+	-	-	-	-	+	+	660.2	656.8	3.4
NPK	-	+	+	-	+	-	-	+	673.4	643.6	29.8

	Replicate			Total
	I	II	III	
$(NPK)_0$	216.1	202.1	225.4	643.6
$(NPK)_1$	200.5	229.8	243.1	673.4

Analysis of variance

Source of variation	df		ss		ms
Replicate	2		177.803		88.90
NPK	1		37.002		-
NPK x replicate = Error (a)	2		128.490		64.245
Treatment	6		347.785		57.96
N		1		189.282	
P		1		8.402	
K		1		95.202	
NP		1		21.282	
NK		1		33.135	
PK		1		0.482	
Residual = Error (b)	12		185.285		15.4404
Total	23		876.365		-
Correction for mean	1		72270.375		-
Total uncorrected	24		73146.740		-

Adjusted treatment totals

$X'_{.000}$	$X'_{.100}$	$X'_{.010}$	$X'_{.110}$	$X'_{.001}$	$X'_{.101}$	$X'_{.011}$	$X'_{.111}$	Total
158.025	187.575	159.275	177.525	152.275	167.725	155.225	159.375	1317.000

the 2^3 factorial.[1] The only new computations are those for the NPK $\times$ replicate or the error (a) sum of squares; the error (a) sum of squares is equal to

$$\frac{(216.1-200.5)^2+(202.1-229.8)^2+(225.4-243.1)^2}{4(1^2+(-1)^2)=8}-\frac{(673.4-643.6)^2}{24}$$

$= 128.490$, with 2 degrees of freedom.

The totals are the incomplete block totals from the two-way table of totals for the levels of the three-factor interaction and replicates (table IX-1).

The *lsd* for the total effects, (N), (P), (NP), (K), (NK), and (PK) is obtained from the error (b) mean square; thus, $lsd = t_{05}(12df)\sqrt{2(3)(4)E_e} = 2.179\sqrt{24(15.4404)} = 41.95$. The two totals (N) = 67.4 and (K) = -47.8 exceed this value. The confidence interval for an effect total is the effect total minus the *lsd* to the effect total plus the *lsd*; e.g., the confidence interval for (K) is from $-47.8 - 41.95 = -89.75$ to $-47.8 + 41.95 = -5.85$. The addition of nitrogen increased the yield, whereas the addition of potash decreased the yield of peas. The deleterious effect of potash on the yield of peas as well as the beneficial effect of nitrogen requires explanation in light of biological knowledge of the physiology of the pea plant.

The NPK effect is subject to a different error than the remainder of the effects. The *lsd* for the total for the NPK effect is $t_{05}(2df)\sqrt{2(3)(4)E_b} = 4.303\sqrt{24(64.2450)} = 168.96$. The NPK effect is small in comparison to its *lsd*, and the effect is assumed to be negligible. The confidence interval for (NPK) is from $29.8 - 168.96 = -139.16$ to $29.8 + 168.96 = 198.76$.

The efficiency of the incomplete block design relative to the corresponding randomized complete block design is computed from formula (IX-1) corrected for the difference in degrees of freedom:

$$\frac{64.2450+6(15.4404)}{7(15.4404)}\left(\frac{12+1}{12+3}\right)\left(\frac{14+3}{14+1}\right) = 143 \text{ per cent,}$$

or a gain of 43 per cent in efficiency. Approximately four replicates of an incomplete block design would be equivalent to six replicates of a randomized complete block design.

If the NPK effect is assumed equal to zero, the treatment totals are adjusted for differences in yield of the incomplete blocks by the formula,

$$X_{.ijh}' = \frac{(A)_i+(B)_j+(AB)_{i+j}+(C)_h+(AC)_{i+h}+(BC)_{j+h}}{4}+\frac{X_{....}}{8}-\frac{6X_{....}}{8}$$
$$= \frac{1}{8}\Big\{X_{....}+(-1)^{i-1}(A)+(-1)^{j-1}(B)+(-1)^{i+j}(AB)+(-1)^{h-1}(C)$$
$$+(-1)^{i+h}(AC)+(-1)^{j+h}(BC)+0\Big\} \qquad \text{(IX-3)}$$

For example, the adjusted total for treatment 010 is equal to

$$X_{.010}' = \frac{624.8+651.4+669.8+682.4+644.4+656.8}{4}-\frac{5(1317.0)}{8}$$
$$= 159.275.$$

The remaining adjusted totals are obtained similarly, or they may be obtained by subtracting 29.8/8 from all the treatments having a plus sign in the NPK row and

[1] The signs of the effects follow the sign for treatment $X_{.100}$. No immediate explanation for the large yield of this treatment is available.

adding 29.8/8 to the remaining treatments. The effect totals are unchanged. The NPK effect should be omitted from the table of effects [324, p. 20]. Again it is emphasized that these adjustments should be made only if NPK is assumed equal to zero. Ordinarily, these adjustments are not made because it is unsafe to assume NPK = 0.

IX-3.3 COMPLETE CONFOUNDING IN THE 3^n SERIES

The 3^2 factorial may be arranged in incomplete blocks of three treatments with either the AB effect or the AB^2 effect confounded with incomplete block differences; thus, the arrangements are

$(AB)_0$	00	12	21
$(AB)_1$	01	10	22
$(AB)_2$	02	11	20

$(AB^2)_0$	00	11	22
$(AB^2)_1$	02	10	21
$(AB^2)_2$	01	12	20

If one or the other grouping is followed in all replicates, this effect is completely confounded with incomplete block differences. If AB and AB^2 are both estimates of the same quantity, partial information is obtainable on the $A \times B$ interaction even if one of the components is completely confounded.

If AB is confounded in all replicates, if the levels of AB are randomly allotted to the incomplete blocks in each replicate, and if a different random allotment of the treatments within each incomplete block is made, the analysis of variance is

Source of variation	df	Sum of squares
Replicate	$r - 1$	$\sum X_{s..}^2/9 - X_{...}^2/9r$
AB	2	$\sum (AB)_{i+j}^2/3r - X_{...}^2/9r$
AB × replicate = error (a)	$2(r-1)$	by subtraction
A	2	see Chapter VII
B	2	see Chapter VII
AB^2	2	see Chapter VII
Residual = error (b)	$6(r-1)$	by subtraction
Total (corrected for mean)	$9r - 1$	$\sum\sum\sum X_{sij}^2 - X_{...}^2/9r$

The error (a) mean square $= E_b$ is used to test the existence of the AB effect, and the error (b) $= E_e$ mean square is the error mean square for effects A, B, and AB^2. The efficiency of this incomplete block design relative to the corresponding randomized complete block design is (ignoring the difference in degrees of freedom)

$$\frac{[2(r-1)+2]E_b + [6+6(r-1)]E_e}{[2(r-1)+2+6+6(r-1)]E_e} = \frac{E_b + 3E_e}{4E_e}. \qquad \text{(IX-4)}$$

If desired and if realistic, the treatment totals may be adjusted to zero AB effects by the formula,

$$X_{\cdot ij}' = \frac{(A)_i + (B)_j + (AB^2)_{i+2j}}{3} + \frac{X_{\dots}}{9} - \frac{X_{\dots}}{3}. \qquad \text{(IX-5)}$$

As with the 2^n series, this adjustment is not applicable if the AB effect is not equal to zero. The standard errors for the effect levels are obtained as before, except that E_e is the error mean square for levels of A, B, and AB^2, while E_b is the error for the levels of AB.

The 3^3 factorial arrangement may be designed in incomplete blocks of three or of nine treatments. For example, suppose that only litters of size 3 (or three homogeneous roasts) are available, and it is desired to compare 3^3 treatments. It is necessary to confound 8 degrees of freedom with incomplete blocks. Therefore, suppose that we wish to confound AB, AC, and their interactions, AB^2C^2 and BC^2. The following nine sets of three treatments are obtained:[1]

Treatments			Level of effect			
000	122	211	$(AB)_0$,	$(AC)_0$,	$(BC^2)_0$,	$(AB^2C^2)_0$
001	120	212	$(AB)_0$,	$(AC)_1$,	$(BC^2)_2$,	$(AB^2C^2)_2$
002	121	210	$(AB)_0$,	$(AC)_2$,	$(BC^2)_1$,	$(AB^2C^2)_1$
010	102	221	$(AB)_1$,	$(AC)_0$,	$(BC^2)_1$,	$(AB^2C^2)_2$
011	100	222	$(AB)_1$,	$(AC)_1$,	$(BC^2)_0$,	$(AB^2C^2)_1$
012	101	220	$(AB)_1$,	$(AC)_2$,	$(BC^2)_2$,	$(AB^2C^2)_0$
020	112	201	$(AB)_2$,	$(AC)_0$,	$(BC^2)_2$,	$(AB^2C^2)_1$
021	110	202	$(AB)_2$,	$(AC)_1$,	$(BC^2)_1$,	$(AB^2C^2)_0$
022	111	200	$(AB)_2$,	$(AC)_2$,	$(BC^2)_0$,	$(AB^2C^2)_2$

If the sets of treatments are randomly assigned to the nine incomplete blocks within each replicate, if the same effects are confounded in all replicates, and if the treatments within each incomplete block are randomly assigned, the analysis of variance is

[1] $AB \times AC = \sum_{u=1}^{p-1} AB(AC)^u = AB^2C^2 + BC^2$

Source of variation	Degrees of freedom	ms
Replicate	$r-1$	
AB	2	
AC	2	
BC^2	2	
AB^2C^2	2	
Error (a)	$8(r-1)$	E_b
A	2	
B	2	
C	2	
AB^2	2	
AC^2	2	
BC	2	
$A \times B \times C - AB^2C^2$	6	
Error (b)	$18(r-1)$	E_e
Total (corrected for the mean)	$27r-1$	

The 3^4 factorial may be arranged in an incomplete block design with incomplete blocks of three, nine, or twenty-seven treatments. The 3^5 factorial may be arranged in an incomplete block design with three, nine, twenty-seven, or eighty-one treatments per incomplete block, depending upon the nature of the experimental material and upon the information desired on the various interactions.

IX-3.4 COMPLETE CONFOUNDING IN OTHER FACTORIALS

Other p^n factorials may be arranged in incomplete blocks of p^s treatments in the same manner as described for the 2^n and 3^n series [177]. It is also possible to arrange other factorial combinations like the $k \times p^m \times q^n$ in various incomplete block sizes [175, 194, 215, 217, 223-5, 228, 324]. As an illustration of complete confounding in a $k \times p^m \times q^n$ factorial, a $2^4 \times 3^2$ factorial arrangement of treatments in a time and motion study has been selected. Two weights of a milk pail, two types of pail, two methods of carrying the pail, two heights of pouring, three distances of carrying the pails, and three types of path (straight hall, a hall with one right-angle turn, and a hall with stairs in it) were used in all combinations, resulting in 144 treatments.[1] Six boys were available to carry out the experiment. It was expected that they would differ in the characteristics observed. Therefore, the six boys were the incomplete blocks and each boy would perform twenty-four treatments. Also, it was suspected that none of the interactions were of any appreciable magnitude.

Various schemes of confounding could be used. The forty-eight treatments could be conducted by two boys at each of the distances (or types of path). This completely confounds a main effect. Also, one could use four boys and have each boy do thirty-six treatments. One particular scheme would be to completely confound effects ABC, AD, and BCD with differences between

[1]From a study by W. H. M. Morris, Cornell University.

boys. Alternatively, if each boy carries out twenty-four treatments, one effect, $ABCD$, could be completely confounded and some effects, EF and interactions with EF, could be partially confounded with the differences between boys or incomplete blocks. The plan and breakdown of the degrees of freedom for the latter design are presented in table IX-2. Since the experiment could be performed in less than one hour and since it was desired to utilize the services of all six boys, the experimental arrangement described in table IX-2 appeared to be the most desirable for this study.

IX-3.5 APPLYING TREATMENTS TO STRATA WHICH ARE DESIGNED TO CONTROL HETEROGENEITY

One consideration that occasionally escapes the attention of the experimenter is that the purpose of stratification of experimental material into homogeneous subgroups is to control extraneous variation. If treatments are applied to the strata, then the meaning of the experimental results may be questionable; for example, suppose that we have a randomized complete block experiment of p levels of the factor a in r replicates. Furthermore, suppose that the experimenter applies r levels of the factor b to the r replicates. *If an interaction,* $A \times B$, is present and if these levels represent the levels of interest, no valid estimate of the experimental error is available. The analysis of variance (see Chapter VII) is

Source of variation	df	Expectation of mean square
Replicate and B effect	$r - 1$	$\sigma_e^2 + f(\text{B effect and replicate})$
A	$p - 1$	$\sigma_e^2 + f(A)$
Residual (error and A $\times$ B)	$(r - 1)(p - 1)$	$\sigma_e^2 + f(A \times B)$
Total	$rp - 1$	

where f (B effect and replicate) is a function of the factor b effects and the replicate effects, $f(A)$ is a function of the factor a effects, and the $f(A \times B)$ is a function of the interaction of the factors a and b. It could turn out that the residual mean square is significantly larger than the other two mean squares. If there is a block $\times$ treatment interaction, the same effect could be observed. The experimenter must know his material and ascertain whether or not an interaction is present and whether or not this affects the tests of his hypothesis; i.e., whether the finite or infinite model is assumed.

The same comments hold for the latin square. Suppose that we have three levels of the factor a and that we apply three levels of factor b to the rows of a 3×3 latin square; thus:

b_0	a_0	a_1	a_2		00	10	20
b_1	a_1	a_2	a_0	=	11	21	01
b_2	a_2	a_0	a_1		22	02	12

TABLE IX-2. Experimental plan for a $2^4 \times 3^2$ factorial in incomplete blocks of 3×2^3 = 24 treatments

Item no.	Block I	Block II	Block III	Block IV	Block V	Block VI
1	000000	000010	000020	000100	000110	000120
2	000001	000011	000021	000101	000111	000121
3	000002	000012	000022	000102	000112	000122
4	001110	001120	001100	001010	001020	001000
5	001111	001121	001101	001011	001021	001001
6	001112	001122	001102	001012	001022	001002
7	010120	010100	010110	010020	010000	010010
8	010121	010101	010111	010021	010001	010011
9	010122	010102	010112	010022	010002	010012
10	011000	011001	011002	011100	011101	011102
11	011012	011010	011011	011112	011110	011111
12	011021	011022	011020	011121	011122	011120
13	100100	100110	100120	100000	100010	100020
14	100101	100111	100121	100001	100011	100021
15	100102	100112	100122	100002	100012	100022
16	101010	101020	101000	101110	101120	101100
17	101011	101021	101001	101111	101121	101101
18	101012	101022	101002	101112	101122	101102
19	110020	110000	110010	110120	110100	110110
20	110021	110001	110011	110121	110101	110111
21	110022	110002	110012	110122	110102	110112
22	111101	111102	111100	111001	111002	111000
23	111110	111111	111112	111010	111011	111012
24	111122	111120	111121	111022	111020	111021

Analysis of variance

Source of variation	df	Source of variation	df
Blocks or boys	5	A x F	2
Main effects	8	B x C	1
A	1	B x D	1
B	1	B x E	2
C	1	B x F	2
D	1	C x D	1
E	2	C x E	2
F	2	C x F	2
Two-factor interactions	26	D x E	2
A x B	1	D x F	2
A x C	1	EF^2	2
A x D	1	EF' (partially confounded)	2
A x E	2	Residual	104

If we select one of the twelve 3×3 latin squares at random, the following analysis of variance is appropriate:

Source of variation	df	Expectation of mean square
Row and B effect	2	σ_e^2 + f(B and row)
Column and AB^2 effect	2	σ_e^2 + f(AB^2 and column)
Treatment = A effect	2	σ_e^2 + f(A)
Residual (AB effect and error)	2	σ_e^2 + f(AB)
Total	8	——

If there is an interaction of the factors *a* and *b* and if we are interested in these particular levels of the factors (the finite model), no appropriate error mean square is available. The same type of comment holds for incomplete block and other types of designs. Instead of obtaining more information, the experimenter may actually end up with less by having no appropriate error mean square for making tests of significance.

IX-4 Partial Confounding

Partial information may be desired on all effects in a factorial experiment with a large number of treatments, and at the same time a small block size may be desirable. A scheme of confounding may be followed in which some effects are confounded in one replicate, other effects in the second replicate, and still others in the remaining replicates. The scheme of partial confounding allows for within incomplete block information on all effects.

The treatment means are *always* adjusted in partially confounded arrangements. The reason for adjustment is that estimates of effects are obtainable, and, hence, the effect of the incomplete block is estimable. The treatments within an incomplete block receive a negative adjustment if the incomplete block effect is positive and a positive adjustment if the incomplete block effect is negative. These adjustments tend to remove the mean effects of the incomplete blocks, resulting in a more reliable estimate of the treatment totals.

IX-4.1 PARTIAL CONFOUNDING IN THE 2^n SERIES

In a 2^3 factorial experiment with four replicates, information may be desired on all seven effects. It may be considered that blocks of eight plots cover too much heterogeneity and that blocks of four plots would be more efficient. The main effects *A*, *B*, and *C* are of most interest, with the secondary interest on the *AB*, *AC*, *BC*, and *ABC* interactions. Complete confounding of any of the interactions may be undesirable. The interaction *ABC* could be confounded with block differences in replicate I, *AC* in replicate II, *BC*

in replicate III, and *AB* in replicate IV. Some such field plan as the following might be obtained:

Interaction confounded	ABC		AC		BC		AB	
Block	I_a	I_b	II_b	II_a	III_a	III_b	IV_a	IV_b
	001	101	000	011	000	101	001	011
	100	011	111	110	100	010	110	100
	111	110	010	001	111	110	000	010
	010	000	101	100	011	001	111	101
	Rep. I		Rep. II		Rep. III		Rep. IV	

where the confounded effect is allotted to the replicate at random and the level of the effect to the incomplete block within a replicate at random. The treatments within an incomplete block are assigned to the plots at random. For such a design the effects *AB*, *AC*, *BC*, and *ABC* are estimated from the three replicates in which they are unconfounded. Thus, the interactions are evaluated from twenty-four plots instead of the entire thirty-two plots, as are the main effects *A*, *B*, and *C*. The amount of information on the interactions, ignoring interblock comparisons, is then $\frac{3}{4}$. The breakdown of the degrees of freedom for the above design is

Source of variation	df		ms
Replicate	3		
Blocks within replicates (ignoring treatments)	4		E_b
Treatment (eliminating block effects)	7		
Main effects from all replicates		3	
Interactions from replicates in which effect is unconfounded		4	
Residual	17		E_e
Total	31		--

The gain in precision due to confounding is (ignoring the difference in degrees of freedom) equal to

$$\frac{4E_b + (17 + 7)E_e}{(4 + 7 + 17)E_e} = \frac{E_b + 6E_e}{7E_e}, \tag{IX-6}$$

if the confounded effects are assumed to be negligible [324, p. 31]. If not, the treatment effects must be removed from E_b before insertion in (IX-6).

The following relationship among sums of squares holds: blocks within replicates (ignoring treatments) + treatments (eliminating blocks) = blocks within replicates (eliminating treatments) + treatments (ignoring blocks). The treatment (ignoring block) sum of squares is obtained in the same manner as for a randomized complete block design. The blocks-within-replicates (eliminating treatments) sum of squares is computed from the above relation-

ship, and the corresponding mean square E_b' is computed. Since the expected value of E_b' is $\sigma_e^2 + \frac{(r-1)}{r}k\sigma_b^2$, an unbiased estimate of $\sigma_e^2 + k\sigma_b^2$ is obtained from the formula,

$$\frac{rE_b' - E_e}{r-1} = E_b'', \tag{IX-7}$$

where r = number of replicates and k = number of treatments in an incomplete block. The value E_b'' is substituted for E_b in equation (IX-6) to estimate the efficiency of the incomplete block design with recovery of interblock information relative to the corresponding complete block design; if E_b'' is inserted for E_b in formula (IX-6), the efficiency of the two designs is equal to $(4E_b' + 17E_e)/21E_e$.

The treatment totals are adjusted by the formula[1]:

$$\begin{aligned} X_{\cdot ijh}' &= \frac{1}{8}\{X_{\cdots\cdot} + (-1)^{i-1}(A) + (-1)^{j-1}(B) + (-1)^{h-1}(C)\} \\ &+ \frac{1}{6}\{(-1)^{i+j}(AB') + (-1)^{i+h}(AC') + (-1)^{j+h}(BC') + (-1)^{i+j+h-1}(ABC')\} \\ &= \frac{(A)_i + (B)_j + (C)_h}{4} + \frac{(AB')_{i+j} + (AC')_{i+h} + (BC')_{j+h} + (ABC')_{i+j+h}}{3} \\ &- \frac{3X_{\cdots\cdot}}{4}, \end{aligned} \tag{IX-8}$$

where the effects with a prime are derived from the replicates in which the effects are unconfounded with incomplete block differences. This type of analysis does not make use of the interblock information. The method for utilizing interblock information is described in section IX-4.5.

If, in the above example, the effect ABC is confounded with incomplete block differences in replicates I and IV and if AB is unconfounded, formula (IX-8) becomes

$$\begin{aligned} X_{\cdot ijh}' = {} & \frac{(A)_i + (B)_j + (AB)_{i+j} + (C)_h}{4} + \frac{(AC')_{i+h} + (BC')_{j+h}}{3} \\ & + \frac{(ABC')_{i+j+h}}{2} - \frac{3X_{\cdots\cdot}}{4}, \end{aligned} \tag{IX-9}$$

where $(ABC')_{i+j+h}$ is estimated from replicates II and III. The analysis of variance table is changed accordingly. The type of arrangement is unbalanced partial confounding.

Yates [324, sec. 5], Cochran and Cox [60, plans 6.1 to 6.6], Barnard [11], and Kempthorne [175, p. 261] present schemes of confounding for 2^n factorial systems in incomplete blocks of 2^s treatments. A number of the schemes of confounding are balanced in that the effects of a certain order are confounded with incomplete block differences an equal number of times. Such a balanced

[1]Coefficients for effects are obtained by dividing coefficient for full intrablock information by amount of intrablock information, e.g. $\frac{1/4}{3/4} = \frac{1}{3}$.

set of arrangements for 2^4 treatments in blocks of 2^2 plots [324, table 19] in four replicates results in the following:

Replicate	Effects confounded		
I	AB,	CD,	ABCD
II	AC,	BD,	ABCD
III	AD,	ABC,	BCD
IV	BC,	ABD,	ACD

All two-factor interactions and all three-factor interactions are confounded with incomplete block differences in one of the replicates. Thus, three-quarters of the total information, ignoring interblock information, is obtained on the two- and three-factor interactions. One-half of the information is obtained on the four-factor interaction.

Example IX-2. An experiment was conducted in the greenhouse at Ames, Iowa, to determine the effect of two types of soil (b_0 = soil mixed with sand and b_1 = soil with compost added) and two levels of soil moisture (c_0 = dry soil and c_1 = wet soil) on the yields of hay from two soybean varieties, a_0 and a_1. The scheme of confounding is the same as for the design in the first part of this section. The three two-factor interactions and the three-factor interaction are each confounded with incomplete block differences in one of the four replicates; the scheme of confounding is balanced partial confounding.

The analytical procedure for the first part of the analysis follows that for the 2^3 factorial in a randomized complete block design (see example VII-2). In addition to computing the effects over all replicates, it is necessary to compute the interactions from the replicates in which these effects are unconfounded with incomplete block differences. The AB' effect total (the prime is used to signify that full information is not available on the effect) may be computed directly from replicates I, III, and IV (AB is confounded in replicate II), $355 - 334 = 21$, or from the total effect minus the difference in the block totals from replicate II, $(AB)_0 - (AB)_1 - (\text{II}_a - \text{II}_b) = 481 - 464 - (126 - 130) = 21$, or the AB effect may be computed in each replicate in which it is unconfounded and summed; thus, $(106 - 112) + (108 - 104) + (141 - 118) = -6 + 4 + 23 = 21$ (see bottom portion of table IX-3). The adjusted effect totals (AC'), (BC'), and (ABC') are computed similarly and are given in table IX-3.

The analysis of variance for these data is given in table IX-4. The total, replicate, and treatment (ignoring block) sums of squares are computed in the usual manner for a randomized complete block design. The treatment (eliminating block effect) sum of squares is obtained by computing the sums of squares for the adjusted effects in the right-hand column of the middle portion of table IX-3. The sum of squares for A is $231^2/4(1 + 1 + 1 + 1 + 1 + 1 + 1 + 1) = 1667.531$. The sums of squares for B and C are obtained similarly. The divisors for these sums of squares are identical with those described in Chapter VII. The sum of squares for AB' is $21^2/3(1 + 1 + 1 + 1 + 1 + 1 + 1 + 1) = 18.375$. The divisor is 24 instead of 32, since $r = 3$ replicates are used to obtain this sum of squares instead of $r = 4$ replicates. The sums of squares for AC', BC', and ABC' are obtained similarly; thus, $(-9)^2/24 = 3.375$, $1^2/24 = 0.042$, and $24^2/24 = 24.000$.

The blocks-within-replicate (ignoring treatment effects) sum of squares is equal to

$$\frac{103^2 + 115^2}{4} - \frac{218^2}{8} + \cdots + \frac{143^2 + 116^2}{4} - \frac{259^2}{8}$$
$$= \frac{(103 - 115)^2 + (126 - 130)^2 + (99 - 113)^2 + (143 - 116)^2}{8}$$
$$= 135.625, \text{ with 4 degrees of freedom.}$$

TABLE IX-3. Yields (ounces) of soybean hay for a 2^3 factorial in an incomplete block design

Replicate	I		II		III		IV	
Inc. block	I_a	I_b	II_a	II_b	III_b	III_a	IV_a	IV_b
Effect confounded	$(BC)_0$	$(BC)_1$	$(AB)_0$	$(AB)_1$	$(AC)_1$	$(AC)_0$	$(ABC)_1$	$(ABC)_0$
	000- 7	010- 24	110- 36	011- 31	100- 28	101- 31	111- 66	000- 11
	111- 39	101- 31	000- 19	101- 36	001- 24	010- 19	100- 31	011- 29
	100- 30	001- 21	111- 41	010- 30	110- 35	000- 13	001- 21	101- 33
	011- 27	110- 39	001- 30	100- 33	011- 26	111- 36	010- 25	110- 43
Totals	103	115	126	130	113	99	143	116
Differences		-12		-4		-14		27
Rep. totals		218		256		212		259

Totals and effect totals

Treatment total	$X_{.000}$ 50	$X_{.100}$ 122	$X_{.010}$ 98	$X_{.110}$ 153	$X_{.001}$ 96	$X_{.101}$ 131	$X_{.011}$ 113	$X_{.111}$ 182	Sum of +'s	Sum of -'s	Effect total unadj.	Effect total adj.
Total	+	+	+	+	+	+	+	+	945	0	945	945
A	-	+	-	+	-	+	-	+	588	357	231	231
B	-	-	+	+	-	-	+	+	546	399	147	147
AB	+	-	-	+	+	-	-	+	481	464	17	21
C	-	-	-	-	+	+	+	+	522	423	99	99
AC	+	-	+	-	-	+	-	+	461	484	-23	-9
BC	+	+	-	-	-	-	+	+	467	478	-11	1
ABC	-	+	+	-	+	-	-	+	498	447	51	24
Adj. total	56.67	118.42	94.08	153.83	92.08	131.63	114.17	183.92	-	-	-	-

Levels of effects per replicate

Replicate	$(A)_1$	$(A)_0$	diff.	$(B)_1$	$(B)_0$	diff.	$(C)_1$	$(C)_0$	diff.	$(AB)_0$	$(AB)_1$	diff.
I	139	79	60	129	89	40	118	100	18	106	112	-6
II	146	110	36	138	118	20	138	118	20			
III	130	82	48	116	96	20	117	95	22	108	104	4
IV	173	86	87	163	96	67	149	110	39	141	118	23
Total			231			147			99			21

	$(AC)_0$	$(AC)_1$	diff.	$(BC)_0$	$(BC)_1$	diff.	$(ABC)_1$	$(ABC)_0$	diff.
I	101	117	-16				114	104	10
II	126	130	- 4	124	132	-8	134	122	12
III				103	109	-6	107	105	2
IV	135	124	11	137	122	15			
Total			- 9			1			24

TABLE IX-4. Analysis of variance of data in table IX-3

Source of variation	df		ss		ms
Replicate	3		228.59		76.20
Blocks within replicate	4		135.625		33.906
Treatment (eliminating block)	7		2694.885		-
A		1		1667.531	-
B		1		675.281	-
AB'		1		18.375	-
C		1		306.281	-
AC'		1		3.375	-
BC'		1		0.042	-
ABC'		1		24.000	-
Residual	17		546.87		32.169
Treatment (ignoring block)	7		2759.72		-
Block (eliminating treatment)	4		70.79		17.698
Total	31		3605.97		-
Correction for mean	1		27907.03		-
Total uncorrected	32		31513.00		-

The blocks-within-replicate (eliminating treatment effect) sum of squares is obtained from the relation given in the preceding section, 135.625 − (2759.72 − 2694.885) = 70.79, or from the formula,

$$\frac{[(AB)_0 - 4\mathrm{II}_a]^2 + [(AB)_1 - 4\mathrm{II}_b]^2}{k(1+1+1+3^2)} - \frac{[X_{\ldots.} - 4X_{\mathrm{II}\ldots}]^2}{2k(12)}$$

$$+\frac{[(AC)_0 - 4\mathrm{III}_a]^2 + [(AC)_1 - 4\mathrm{III}_b]^2}{12k} - \frac{[X_{\ldots.} - 4X_{\mathrm{III}\ldots}]^2}{24k}$$

$$+\frac{[(BC)_0 - 4\mathrm{I}_a]^2 + [(BC)_1 - 4\mathrm{I}_b]^2}{12k} - \frac{[X_{\ldots.} - 4X_{\mathrm{I}\ldots}]^2}{24k}$$

$$+\frac{[(ABC)_1 - 4\mathrm{IV}_a]^2 + [(ABC)_0 - 4\mathrm{IV}_b]^2}{12k} - \frac{[X_{\ldots.} - 4X_{\mathrm{IV}\ldots}]^2}{24k} \tag{IX-10}$$

$$= \frac{(-23)^2 + (-56)^2 + 65^2 + 32^2 + 55^2 + 18^2 + (-74)^2 + (-17)^2}{48}$$

$$- \frac{(-79)^2 + 97^2 + 73^2 + (-91)^2}{96} = 70.79, \text{ with four degrees of freedom.}$$

This sum of squares represents the contrast of the level of the effect in the unconfounded replicates with the level of the effect in the replicate in which the effect is confounded. The above method of computing the blocks-within-replicate (eliminating treatment effects) sum of squares is the one presented in Chapter XI for lattices.

The residual sum of squares is obtained by subtracting the block (eliminating treatment) sum of squares from the total minus replicate and treatment (ignoring block) sums of squares, 3605.97 − 228.59 − 2759.72 − 70.79 = 546.87, with (31 − 3 − 7) − 4 = 17 degrees of freedom. The block × treatment sum of squares from the randomized complete block analysis is partitioned into two parts, block (eliminating treatment) = 70.79 and residual = 546.87. The residual sum of squares may also be computed directly. To do this, it is necessary to compute the effects in each replicate and to set up tables like those given in the bottom part of table IX-3. The residual sum

of squares is the sum of the interaction sum of squares in each of the two-way tables, divided by the appropriate divisor:

$$\frac{60^2 + 36^2 + 48^2 + 87^2}{k(1 + 1) = 8} - \frac{231^2}{32} + \frac{40^2 + 20^2 + 20^2 + 67^2}{8} - \frac{147^2}{32} + \cdots$$
$$+ \frac{10^2 + 12^2 + 2^2}{8} - \frac{24^2}{24} = 546.87, \text{ with } 3 + 3 + 3 + 2 + 2 + 2 + 2$$
$$= 17 \text{ degrees of freedom.}$$

This method of reconstructing the residual sum of squares is quite instructive and provides a check on the correctness of the computations of the other sums of squares.

If it is desired to compute the adjusted treatment totals, formula (IX-8) may be used. For example, the adjusted total for treatment 010 is equal to

$$\begin{aligned} X_{\cdot 010}' &= \tfrac{1}{8}\{945 - 231 + 147 - 99\} + \tfrac{3}{8}\{-21 + (-9) - 1 + 24\} \\ &= \tfrac{1}{4}\{357 + 546 + 423 - 3(945)\} + \tfrac{1}{8}\{334 + 362 + 363 + 355\} = 94.08. \end{aligned}$$

Yates [324, p. 21] presents another procedure for obtaining the adjusted total,

$$X_{\cdot 010} + \frac{1}{8}\left\{ -\left[\frac{(BC')}{3} - (\mathrm{I}_a - \mathrm{I}_b)\right] - \left[\frac{(AB')}{3} - (\mathrm{II}_a - \mathrm{II}_b)\right]\right.$$
$$\left. + \left[\frac{(AC')}{3} - (\mathrm{III}_a - \mathrm{III}_b)\right] + \left[\frac{(ABC')}{3} - (\mathrm{IV}_a - \mathrm{IV}_b)\right]\right\}$$
$$= 98 + \frac{1}{8}\left\{\frac{-37}{3} - 11 + 11 - 19\right\} = 94.08.$$

The signs are obtained by noting whether the particular treatment falls in the plus or minus group for the confounded effect. The effects are divided by 3, since they are derived from three replicates and the mean effect per replicate is compared with the effect in the replicate in which it is confounded. The other adjusted treatment totals may be obtained in a similar manner. The adjusted treatment means are obtained by dividing by $r = 4 =$ the number of replicates.

The adjusted effect totals may be reconstructed from the adjusted treatment totals if desired. This provides a check on the adjusted treatment total. The values for main effects A, B, and C are unchanged. The value for (AB') is computed as

$$\begin{aligned} &\tfrac{3}{4}(56.67 - 118.42 - 94.08 + 153.83 + 92.08 - 131.83 - 114.17 + 183.92) \\ &= \tfrac{3}{4}(28) = 21. \end{aligned}$$

The plus and minus signs are obtained from the AB row in table IX-3, and the factor $\frac{3}{4}$ is necessary because the AB effect is evaluated in only $\frac{3}{4}$ of the replicates. The effect totals for AC', BC', and ABC' may be obtained similarly.

The efficiency of this incomplete block design relative to a randomized complete block design is less than 100 per cent, since E_b' is less than E_e. The design might be considered as a randomized complete block design [see 175, sec. 23.7]. If it were considered as a randomized complete block design, no adjustments to the treatments or effects would be made.

The standard error of the main effect totals is equal to $\sqrt{32.169(16 + 16)} = 32.1$. The standard error of the totals for the effects which are partially confounded is equal

to $\sqrt{32.169(12 + 12)} = 27.8$. The average intrablock variance of a difference between two adjusted means is $\frac{2}{7}\left\{\frac{4}{3w} + \frac{3}{4w}\right\} = 25E_e/42$ (from eq. (IX-17) with $w' = 0$).

IX-4.2 PARTIAL CONFOUNDING IN THE 3^n SERIES

The 3^2 factorial may be arranged in blocks of three experimental units with different effects confounded in each replicate. Suppose that it is desired to confound AB with incomplete block differences in two of the four replicates and to confound AB^2 with incomplete block differences in the other two replicates. (The plan for this scheme of confounding is given in section IX-3.3.) Full information is available on the main effects, and one-half information (ignoring interblock information) is available on the two components of the interaction. With a random allocation of the sets to the replicates, of the groups to the incomplete blocks, and of the treatments within the incomplete blocks, the analysis of variance is

Source of variation	Degrees of freedom		ms
Replicate	3		
Block (ignoring treatment effect)	$4 \times 2 = 8$		E_b
Treatment (eliminating block effect)	8		
A		2	
B		2	
AB′		2	
AB²′		2	
Residual	16		E_e
A × replicate		6	
B × replicate		6	
AB × replicate in which AB is unconfounded		2	
AB² × replicate in which AB² is unconfounded		2	
Total (corrected for the mean)	35		

The effects are evaluated from the replicates in which they are unconfounded. The adjusted treatment totals for this particular scheme of confounding are obtained from the formula

$$X_{\cdot ij}' = \frac{(A)_i + (B)_j}{3} + \frac{(AB')_{i+j} + (AB^{2\prime})_{i+2j}}{3/2} - \frac{X_{\dots}}{3}, \qquad \text{(IX-11)}$$

where the levels of AB and AB^2 are evaluated from the replicates in which these effects are unconfounded with incomplete blocks. The standard error of a difference between two adjusted means is $\sqrt{3E_e/r}$. The residual sum of squares may be computed directly as for the 2^n series. Also, the sum of squares for block (eliminating treatment effect) may be computed by either of the methods described in the previous section.

In the 3^3 factorial, blocks of either three or nine may be used. Suppose

that only partial information is desired on the three-factor interaction, $A \times B \times C$, and that blocks of nine are to be used. Furthermore, suppose that four replicates are to be used. Then, ABC may be confounded with sets of nine treatments in one replicate, ABC^2 in a second replicate, AB^2C in a third replicate, and AB^2C^2 in a fourth replicate. Such a scheme would allow blocks of nine experimental units to be utilized and would allow for full information on main effects and two-factor interactions and for three-fourths information on the three-factor interaction. The analysis of variance table for such a design is

Source of variation	Degrees of freedom		ms
Replicate	3		
Block (ignoring treatment effect)	8		E_b
Treatment (eliminating block effect)	26		
Main effects (A, B, C)		6	
Two-factor interactions (A $\times$ B, A $\times$ C, B $\times$ C)		12	
ABC'		2	
$ABC^{2\prime}$		2	
AB^2C'		2	
$AB^2C^{2\prime}$		2	
Residual	70		E_e
Total (corrected for the mean)	107		

The formula for obtaining the adjusted treatment totals is

$$X_{\cdot ijh}' = \frac{1}{9}\Big\{(A)_i + (B)_j + (AB)_{i+j} + (AB^2)_{i+2j} + (C)_h + (AC)_{i+h} + (AC^2)_{i+2h} + (BC)_{j+h} + (BC^2)_{j+2h}\Big\} + \frac{4}{9(3)}\Big\{(ABC')_{i+j+h} + (ABC^{2\prime})_{i+j+2h} + (AB^2C')_{i+2j+h} + (AB^2C^{2\prime})_{i+2j+2h}\Big\} - \frac{4}{9}X_{\cdots\cdot}\,. \tag{IX-12}$$

The average variance of a difference between two adjusted means is $43E_e/78$ and between two adjusted totals is $344E_e/39$.

It may be that sets of nine experimental units are not homogeneous, and in order to obtain homogeneity it is necessary to use incomplete blocks of three experimental units. This means that 8 degrees of freedom or four effects must be confounded with incomplete block differences in each replicate. In order not to confound a main effect, to use four replicates, and to obtain as much information as possible on the two-factor interactions, the following scheme of confounding is suggested:

AB, AC, and interactions in one replicate,
AB, AC², " " " a second replicate,
AB², AC, " " " a third " , and
AB², AC², " " " a fourth " .

This scheme confounds each component of the two-factor interactions with incomplete block differences in two of the replicates and each component of the three-factor interaction in one of the replicates. This results in one-half information on the two-factor interactions and three-fourths on the three-factor interaction. The analysis of variance for this design is

Source of variation	Degrees of freedom		ms
Replicate	3		
Block (ignoring treatment effect)	32		E_b
Treatment (eliminating block effect)	26		
Main effects (A, B, C)		6	
Two-factor interactions (AB′, AB²′, AC′, AC²′, BC′, BC²′)		12	
Three-factor interaction (ABC′, ABC²′, AB²C′, AB²C²′)		8	
Residual	46		E_e
Total (corrected for the mean)	107		

The formula for obtaining the adjusted treatment totals for this particular design is equal to

$$X_{.ijh}' = \frac{1}{9}\Big\{(A)_i + (B)_j + (C)_h - 4X_{\dots}\Big\} + \frac{2}{9}\Big\{(AB')_{i+j} + (AB^{2\prime})_{i+2j}$$
$$+ (AC')_{i+h} + (AC^{2\prime})_{i+2h} + (BC')_{j+h} + (BC^{2\prime})_{j+2h}\Big\} + \frac{4}{3(9)}\Big\{(ABC')_{i+j+h}$$
$$+ (ABC^{2\prime})_{i+j+2h} + (AB^2C')_{i+2j+h} + (AB^2C^{2\prime})_{i+2j+2h}\Big\}. \qquad \text{(IX-13)}$$

The average variance of a difference between two means is $17E_e/26$.

Other situations for the 3^n series of factorials in which certain effects are partially confounded with incomplete block differences may be treated similarly.

IX-4.3 PARTIAL CONFOUNDING IN OTHER FACTORIALS

The p^n system of factorials may be arranged in incomplete blocks of p^s treatments by partially confounding effects of less interest. The procedure follows that outlined for the 2^n and 3^n systems [177].

Partial confounding of some effects in the $k \times p^m \times q^n$ system of factorials presents considerably more analytical difficulties. A full discussion of this type of partial confounding is not attempted herein. For a more complete discussion the reader is referred to the writings of Yates [324], Kempthorne [175, Ch. 18], Nair [215, 217], Li [194], and Nair and Rao [223-5, 228]. For the present discussion, partial confounding schemes for the 3×2^2 and for the $3^2 \times 2$ factorials in blocks of six treatments are illustrated.

For the arrangement of the $3 \times 2 \times 2$ factorial in blocks of six with the BC and $A \times B \times C$ interactions partially confounded with incomplete block

differences, Yates [324, sec. 13] proposes the following balanced arrangement for the treatments $a_ib_jc_h = ijh$:

Block	I_a	I_b	II_a	II_b	III_a	III_b
	001	000	000	001	000	001
	010	011	011	010	011	010
	100	101	101	100	100	101
	111	110	110	111	111	110
	200	201	200	201	201	200
	211	210	211	210	210	211

The main effects are unconfounded in each replicate, since all levels of each factor appear in each incomplete block. To observe which effects are confounded, it has been found instructive to set up the above table in another manner:

I_a	I_b	II_a	II_b	III_a	III_b
$a_0(BC)_1$	$a_0(BC)_0$	$a_0(BC)_0$	$a_0(BC)_1$	$a_0(BC)_0$	$a_0(BC)_1$
$a_1(BC)_0$	$a_1(BC)_1$	$a_1(BC)_1$	$a_1(BC)_0$	$a_1(BC)_0$	$a_1(BC)_1$
$a_2(BC)_0$	$a_2(BC)_1$	$a_2(BC)_0$	$a_2(BC)_1$	$a_2(BC)_1$	$a_2(BC)_0$

In the above form, it will be observed that the BC effect at the zero level of factor a (a_0BC or BCa_0, which is equal to $\frac{a_0}{2}[(BC)_0 - (BC)_1]$) is also a between incomplete blocks contrast. The BC effect is not orthogonal within incomplete blocks, since either the one or zero level is repeated twice in an incomplete block and the other level appears once. The 3×2^2 is a mixed-primes factorial, and the modulo notation described in Chapter VII is not appropriate for all three factors but is for the 2^2 part of the factorial. This fact facilitates the construction of partially confounded designs for a factorial arrangement of treatments.

The 2×3^2 factorial may be arranged in blocks of six treatments. The modulo notation of Chapter VII applies to factors b and c but not to all three. The following arrangement results in partial confounding of the BC component of the $B \times C$ interaction and of the $A \times B \times C$ interaction:

I_a	I_b	I_c	II_a	II_b	II_c
$a_0(BC)_0$	$a_0(BC)_1$	$a_0(BC)_2$	$a_0(BC)_1$	$a_0(BC)_0$	$a_0(BC)_2$
$a_1(BC)_1$	$a_1(BC)_2$	$a_1(BC)_0$	$a_1(BC)_0$	$a_1(BC)_2$	$a_1(BC)_1$

which gives the following sets of treatments in the incomplete blocks:

I_a	I_b	I_c	II_a	II_b	II_c
000	001	002	001	000	002
012	010	011	010	012	011
021	022	020	022	021	020
101	102	100	100	102	101
110	111	112	112	111	110
122	120	121	121	120	122

If an additional two replicates are available, the BC^2 component should be confounded in the third and fourth replicates, resulting in equal confounding on the 4 degrees of freedom for the $B \times C$ interaction and in equal confounding on the 4 degrees of freedom for the $A \times B \times C$ interaction; the arrangements are

III_a	III_b	III_c	IV_a	IV_b	IV_c
$a_0(BC^2)_0$	$a_0(BC^2)_1$	$a_0(BC^2)_2$	$a_0(BC^2)_1$	$a_0(BC^2)_0$	$a_0(BC^2)_2$
$a_1(BC^2)_1$	$a_1(BC^2)_2$	$a_1(BC^2)_0$	$a_1(BC^2)_0$	$a_1(BC^2)_2$	$a_1(BC^2)_1$

III_a	III_b	III_c	IV_a	IV_b	IV_c
000	002	001	002	000	001
011	010	012	010	011	012
022	021	020	021	022	020
102	101	100	100	101	102
110	112	111	111	112	110
121	120	122	122	120	121

A numerical example of the above design and instructions for calculating the analysis of variance table are given by Cochran and Cox [60, sec. 6.19].

Example IX-3. A $3 \times 2 \times 2$ factorial arrangement of applications of nitrogen and clippings in the fall and spring was used by M. L. Peterson [246] to study the effect on yields of hay from alfalfa. The treatments were

a_0 = no nitrogen,
a_1 = 60 lbs. of nitrogen applied on March 15,
a_2 = 30 lbs. of nitrogen on Sept. 15 and 30 lbs. on March 15,
b_0 = no spring deferment of clipping,
b_1 = spring deferment of clipping,
c_0 = no fall deferment of clipping, and
c_1 = fall deferment of clipping

in all combinations. The twelve combinations were laid out in three replicates with incomplete blocks of six plots. The interactions $A \times B \times C$ and $B \times C$ were partially

confounded with incomplete block differences. The specified group of treatments was assigned to the incomplete blocks within a replicate at random, and the treatments were randomly assigned to the plots within the incomplete block. The systematic arrangement of the treatment combinations and the yields of alfalfa in grams per plot are given in table IX-5. The treatments are arranged in a partially balanced incomplete block design in such a manner that the $B \times C$ and $A \times B \times C$ interactions are partially confounded with block differences *in each replicate*. Interaction $B \times C$ is confounded as little as possible in this design.

The statistical analysis for the $3 \times 2 \times 2$ factorial experiment on alfalfa yields is first made ignoring the confounding [324, p. 39]; i.e., assuming the twelve treatments were completely randomized within blocks of twelve plots. Most of the calculations are required for the analysis of the incomplete block design; also, the reader may observe the differences in the analyses for the randomized complete block and for the incomplete block design. The totals (table IX-6) for the twelve treatments are obtained in the usual manner. Summing the treatment totals over the factor a, the totals in column 5 are obtained. The four figures in column 6 are obtained as follows:

$$789 + 771 = 1560;$$
$$947 + 882 = 1829;$$
$$771 - 789 = -18;$$
$$882 - 947 = -65.$$

The figures in columns 7, 8, and 9 are obtained in the same manner from columns 3, 4, and 5, respectively. Columns 10, 11, 12, and 13 are obtained by the same procedure applied to columns 6, 7, 8, and 9, respectively. For example, the figure 15 in column 13, which is the B effect $= (B)_1 - (B)_0$, is obtained from column 9 as $85 + (-70) = 15$, or as $-83 - 156 + 254 = 15$. Several checks for the computations in the top portion of table IX-6 are available. The sum of each of the columns 2, 3, 4, and 5 should equal the first figure of columns 10, 11, 12, and 13, respectively. Likewise, the sum of the numbers over the factor a serves as a check for the figures obtained by the sum and differences.

The four tables at the bottom of table IX-6 may be used to compute the $A \times B$, $A \times C$, $B \times C$, and $A \times B \times C$ interactions, or they may be computed directly from the top part of table IX-6. The sums of squares of the main and interaction effects are

$$A\colon \frac{(A)_0^2 + (A)_1^2 + (A)_2^2}{12} - \frac{X\ldots.^2}{36} = \frac{3389^2 + 6296^2 + 6090^2}{12} - \frac{15775^2}{36} = 438{,}569.06.$$

$$B\colon \frac{(B)_0^2 + (B)_1^2}{18} - \frac{X\ldots.^2}{36} = \frac{[(B)_1 - (B)_0]^2}{36} = \frac{15^2}{36} = 6.25.$$

$$C\colon \frac{(C)_0^2 + (C)_1^2}{18} - \frac{X\ldots.^2}{36} = \frac{[(C)_1 - (C)_0]^2}{36} = \frac{1395^2}{36} = 54{,}056.25.$$

$$A \times B\colon \frac{(Ba_0)^2 + (Ba_1)^2 + (Ba_2)^2}{12} - \frac{(B)^2}{36}$$
$$= \frac{(-83)^2 + (-156)^2 + (254)^2}{12} - \frac{15^2}{36} = 7{,}972.17$$

TABLE IX-5. Systematic arrangement of yields (grams) from a $3 \times 2 \times 2$ factorial experiment in three replicates with incomplete blocks of six plots (The interactions $B \times C$ and $A \times B \times C$ are partially confounded with block differences)

	Replicate I				Replicate II				Replicate III			
	Block number				Block number				Block number			
	I_a		I_b		II_a		II_b		III_a		III_b	
	T[a]	Y[b]	T	Y	T	Y	T	Y	T	Y	T	Y
	001	294	000	249	000	232	001	267	000	308	001	386
	010	226	011	340	011	254	010	235	011	288	010	310
	100	403	101	523	101	523	100	410	100	617	101	750
	111	520	110	404	110	404	111	450	111	730	110	562
	200	370	201	457	200	411	201	455	201	565	200	630
	211	487	210	481	211	436	210	399	210	539	211	830
Block total		2300		2454		2260		2246		3047		3468
Rep. total				4754				4506				6515
Grand total												15775

[a]Treatments are designated by the subscript ijh of the treatment combination $a_i b_j c_h$, where $i = 0, 1, 2$; $j = 0, 1$; and $h = 0, 1$.
[b]Yield.

TABLE IX-6. Treatment combination totals and calculations for estimating the main effects and interaction effects from the 3 × 2 × 2 factorial experiment and alternative tables for computing the interaction sums of squares (Treatment combination $a_ib_jc_h$ = ijh)

	Yield (grams per plot)				Sums and differences				Effects			
	a_0	a_1	a_2	Sum	a_0	a_1	a_2	Sum	a_0	a_1	a_2	Sum
b_0c_0	789	1430	1411	3630	1560	2800	2830	7190	3389	6296	6090	15775 = total
b_1c_0	771	1370	1419	3560	1829	3496	3260	8585	-83	-156	254	15 = (B)
b_0c_1	947	1796	1507	4250	-18	-60	8	-70	269	696	430	1395 = (C)
b_1c_1	882	1700	1753	4335	-65	-96	246	85	-47	- 36	238	155 = (BC)
Totals	3389 = $(A)_0$	6296 = $(A)_1$	6090 = $(A)_2$	15775 = total								

	0j·	1j·	2j·	Total
10·	1736	3226	2918	7880 = $(B)_0$
11·	1653	3070	3172	7895 = $(B)_1$
Total	3389 = $(A)_0$	6296 = $(A)_1$	6090 = $(A)_2$	15775 = total

	0·h	1·h	2·h	Total
1·0	1560	2800	2830	7190 = $(C)_0$
1·1	1829	3496	3260	8585 = $(C)_1$
Total	3389 = $(A)_0$	6296 = $(A)_1$	6090 = $(A)_2$	15775 = total

	·j0	·j1	Total
·0h	3630	4250	7880 = $(B)_0$
·1h	3560	4335	7895 = $(B)_1$
Total	7190 = $(C)_0$	8585 = $(C)_1$	15775 = total

	0jh	1jh	2jh	Total
100	789	1430	1411	3630
110	771	1370	1419	3560
101	947	1796	1507	4250
111	882	1700	1753	4335
Total	3389 = $(A)_0$	6296 = $(A)_1$	6090 = $(A)_2$	15775 = total

TABLE IX-7a. Analysis of variance for the 3 × 2 × 2 factorial experiment of table IX-5, neglecting the effects due to confounding

Source of variation	df	ss	ms
Replicate	2	199964.06	-
Treatment (ignoring block)	11	513366.32	-
A	2	438569.06	-
B	1	6.25	-
C	1	54056.25	-
A x B	2	7972.17	-
A x C	2	7750.17	-
B x C (uncorrected)	1	667.36	-
A x B x C (uncorrected)	2	4345.06	-
Residual	22	89123.26	4051.06
Total	35	802453.64	-
Correction for mean	1	6912517.36	-
Total uncorrected	36	7714971.00	-

TABLE IX-7b. Analysis of variance for the incomplete block design

Source of variation	df	ss	ms
Replicate	2	199964.06	99982.03
Blocks within reps. (ignoring trt.)	3	16762.75	5587.58
Treatment (eliminating block)	11	513350.72	-
A	2	438569.06	219284.53
B	1	6.25	6.25
C	1	54056.25	54056.25
A x B	2	7972.17	3986.08
A x C	2	7750.17	3875.08
BC' (corrected)	1	3655.12	3655.12
A x B x C' (corrected)	2	1341.70	670.85
Error	19	72376.11	3809.27
Total	35	802453.64	-

$$A \times C:\ \frac{(Ca_0)^2 + (Ca_1)^2 + (Ca_2)^2}{12} - \frac{(C)^2}{36} = \frac{269^2 + 696^2 + 430^2}{12} - \frac{1395^2}{36} = 7{,}750.17.$$

$$B \times C\,(\text{uncorrected}):\ \frac{(BC)_0^2 + (BC)_1^2}{18} - \frac{X\ldots.^2}{36} = \frac{[(BC)_0 - (BC)_1]^2}{36} = \frac{155^2}{36} = 667.36.$$

$$A \times B \times C\,(\text{uncorrected}):\ \frac{(BCa_0)^2 + (BCa_1)^2 + (BCa_2)^2}{12} - \frac{(BC)^2}{36}$$

$$= \frac{(-47)^2 + (-36)^2 + 238^2}{12} - \frac{155^2}{36} = 4{,}345.06.$$

The sum of the above sums of squares = 513,366.32. As a check the sum of squares for treatments is

$$\frac{789^2 + 1430^2 + \cdots + 1700^2 + 1753^2}{3} - \frac{15775^2}{36} = 513{,}366.31.$$

The total, replicate, and error sums of squares are obtained in the usual manner. The analysis of variance, neglecting the effects due to confounding, is presented in table IX-7a.

The sums of squares for BC and $A \times B \times C$ contain some effects due to block differences. The next step is to eliminate the block effects from the estimates of $(BC)_0$, $(BC)_1$, (BCa_0), (BCa_1), and (BCa_2), and from the corresponding sums of squares. These sums of squares, added to those for effects A, B, C, $A \times B$, and $A \times C$, yield the treatment sum of squares adjusted for block effects (table IX-7b). Yates [324] adjusts the BC effect by adding a correction to the unadjusted effect; thus:

$$\begin{aligned} 3Q &= 3[BC(\text{unadjusted})] + \text{I}_b - \text{I}_a + \text{II}_b - \text{II}_a + \text{III}_b - \text{III}_a \\ &= 155 + 2454 - 2300 + 155 + 2246 - 2260 + 155 + 3468 - 3037 \\ &= 3(155) + 154 - 14 + 421 = 1026. \end{aligned}$$

The adjusted sum of squares for BC is $1026^2/288 = 3655.12$.

Before proceeding further, an explanation of the divisors, etc. is in order. In the first place, the quantity Q is unaffected by block differences. This is apparent from the following, where the treatment yield is designated by the treatment number and the replicate number by I, II, and III:

$$\begin{aligned} 3Q &= 3(BC)(\text{unadjusted}) + \text{I}_b - \text{I}_a + \text{II}_b - \text{II}_a + \text{III}_b - \text{III}_a \\ &= 3(000+011+100+111+200+211-001-010-101-110-201-210)(\text{I}+\text{II}+\text{III}) \\ &\quad +(000+011+101+110+201+210-001-010-100-111-200-211)(\text{I}) \\ &\quad +(001+010+100+111+201+210-000-011-101-110-200-211)(\text{II}) \\ &\quad +(001+010+101+110+200+211-000-011-100-111-201-210)(\text{III}) \\ &= [000(3\text{I}+3\text{II}+3\text{III}+\text{I}-\text{II}-\text{III}) \\ &\quad +011(3\text{I}+3\text{II}+3\text{III}+\text{I}-\text{II}-\text{III}) \\ &\quad +100(3\text{I}+3\text{II}+3\text{III}-\text{I}+\text{II}-\text{III}) \\ &\quad +111(3\text{I}+3\text{II}+3\text{III}-\text{I}+\text{II}-\text{III}) \\ &\quad +200(3\text{I}+3\text{II}+3\text{III}-\text{I}-\text{II}+\text{III}) \\ &\quad +211(3\text{I}+3\text{II}+3\text{III}-\text{I}-\text{II}+\text{III})] \\ &\quad -[001(3\text{I}+3\text{II}+3\text{III}+\text{I}-\text{II}-\text{III}) \\ &\quad +010(3\text{I}+3\text{II}+3\text{III}+\text{I}-\text{II}-\text{III}) \\ &\quad +101(3\text{I}+3\text{II}+3\text{III}-\text{I}+\text{II}-\text{III}) \\ &\quad +110(3\text{I}+3\text{II}+3\text{III}-\text{I}+\text{II}-\text{III}) \\ &\quad +201(3\text{I}+3\text{II}+3\text{III}-\text{I}-\text{II}+\text{III}) \\ &\quad +210(3\text{I}+3\text{II}+3\text{III}-\text{I}-\text{II}+\text{III})] \\ &= [000(4\text{I}+2\text{II}+2\text{III})+011(4\text{I}+2\text{II}+2\text{III}) \\ &\quad +100(2\text{I}+4\text{II}+2\text{III})+111(2\text{I}+4\text{II}+2\text{III}) \\ &\quad +200(2\text{I}+2\text{II}+4\text{III})+211(2\text{I}+2\text{II}+4\text{III})] \\ &\quad -[001(4\text{I}+2\text{II}+2\text{III})+010(4\text{I}+2\text{II}+2\text{III}) \\ &\quad +101(2\text{I}+4\text{II}+2\text{III})+110(2\text{I}+4\text{II}+2\text{III}) \\ &\quad +201(2\text{I}+2\text{II}+4\text{III})+210(2\text{I}+2\text{II}+4\text{III})] \\ &= (BC)_0 \text{ adjusted } - (BC)_1 \text{ adjusted} = (BC) \text{ adjusted.} \end{aligned}$$

From the above, it is apparent that the quantity $3Q$ is the difference between two quantities each of which is composed of 48 plot yields. Therefore, the quantity

$3Q/48 = Q/16$ is the BC effect adjusted for the effects of incomplete blocks on a single plot basis. The divisor for the sum of squares of the quantity Q is $(2(6)/3^2)(4^2 + 2^2 + 2^2) = 32$, and for $3Q$ is $2(6)(4^2 + 2^2 + 2^2) = 288$. The sum of squares for the BC effect adjusted for block differences may be compared with the BC effect and sum of squares from the unconfounded experiment. The BC effect (unadjusted) on a single plot basis is $(BC)/18 = [(BC)_0 - (BC)_1]/18$, and the sum of squares is $[(BC)_0 - (BC)_1]^2/36$. The error variance for the BC effect for the unconfounded experiment is $2E_e/18 = E_e/9$ and is $2E_e/16 = E_e/8$ in an experiment with the confounding as described above. The loss in information due to confounding, *if there is no reduction in the error variance*, is $1 - 1/9/1/8 = 1 - 8/9 = 1/9$, since the relative information from the two designs is $E_e/9/E_e/8 = 8/9$. If there is a sizeable reduction in the error mean square, then the BC interaction may be estimated more accurately in the incomplete block design than in the randomized complete block experiment, even though it is partially confounded with incomplete block differences.

The estimate of the $A \times B \times C$ interaction is obtained similarly. This interaction is expressible as the variation of the BC interaction over the three levels of the factor a. The notation (BCa_0) means that the BC interaction is estimated at the zero level of factor a. In a like manner, (BCa_1) and (BCa_2) represent the BC interaction at the second and third levels of the factor a. The comparison among the three quantities, corrected for the mean, yields the $A \times B \times C$ sum of squares with 2 degrees of freedom. Since these quantities contain block differences in addition to the effect, it is necessary to adjust them. The adjusted quantities are

$$\begin{aligned}
3R_0 &= 3(BCa_0)\text{ unadj.} + \mathrm{I}_a - \mathrm{I}_b + \mathrm{II}_b - \mathrm{II}_a + \mathrm{III}_b - \mathrm{III}_a \\
&= 3(-47) - 154 - 14 + 421 = 112. \\
3R_1 &= 3(BCa_1)\text{ unadj.} + \mathrm{I}_b - \mathrm{I}_a + \mathrm{II}_a - \mathrm{II}_b + \mathrm{III}_b - \mathrm{III}_a \\
&= 3(-36) + 154 + 14 + 421 = 481. \\
3R_2 &= 3(BCa_2)\text{ unadj.} + \mathrm{I}_b - \mathrm{I}_a + \mathrm{II}_b - \mathrm{II}_a + \mathrm{III}_a - \mathrm{III}_b \\
&= 3(238) + 154 - 14 - 421 = 433.
\end{aligned}$$

As a partial check the sum is equal to

$$3R_0 + 3R_1 + 3R_2 = 3Q = 112 + 481 + 433 = 1026.$$

The adjusted BC effects at the three levels of a on a single plot basis are

$$\begin{aligned}
&\frac{1}{10}(3R_0 - Q + 3R_1 - Q + 3R_2 - Q) \\
&= \frac{1}{10}\left(112 - \frac{1026}{3} + 481 - 342 + 433 - 342\right) \\
&= -23.0 + 13.9 + 9.1 = 0.
\end{aligned}$$

The coefficient 10 in the denominator may be verified by the process given above for adjusting the BC effect. In the unconfounded experiment the BCa_0 effect on a single plot basis would be $(1/6)(BCa_0)\text{unadj.} - (BC)/18 = (-47 - 155/3)/6$, the

BCa_1 effect would be $(1/6)(BCa_1)$unadj. $- (BC)/18 = (-36 - 155/3)/6$, and the BCa_2 effect would be $(1/6)(BCa_2)$unadj. $- (BC)/18 = (238 - 155/3)/6$, with the error variance $2E_e/6 = E_e/3$. In this incomplete block experiment the error variance of the adjusted effects is $V(R_i\text{dev.}) = (3 \times 2/10)E_e = 3E_e/5$. Hence, the ratio of the relative information on the effects BCa_0, BCa_1, and BCa_2 deviations in the two designs is $(1/3)/(3/5) = 5/9$ if there is no reduction in the error variance. The loss in information is $1 - 5/9 = 4/9$. Yates [324] points out that the loss in information on each degree of freedom confounded adds up to the single degree of freedom confounded with block differences, $1(1/9) + 2(4/9) = 1$. This feature is a property of the balanced arrangements described by Yates.

The sum of squares for the ABC effect adjusted for block differences is

$$\frac{1}{60}[(3R_0 - Q)^2 + (3R_1 - Q)^2 + (3R_2 - Q)^2] = \frac{1}{60}[(-230)^2 + 139^2 + 91^2] = 1341.70.$$

The treatment sum of squares adjusted for block differences is the sum of the effect sums of squares unaffected by block differences plus the sums of squares for the partially confounded effects adjusted for block differences; thus:

$$\begin{aligned}&(438{,}569.06 + 6.25 + 54{,}056.25 + 7972.17 + 7750.17)\\ &+ (3655.12 + 1341.70) = 513{,}350.72.\end{aligned}$$

The blocks-within-replicate sum of squares is

$$\begin{aligned}&\frac{(\mathrm{I}_b - \mathrm{I}_a)^2}{2k} + \frac{(\mathrm{II}_b - \mathrm{II}_a)^2}{2k} + \frac{(\mathrm{III}_b - \mathrm{III}_a)^2}{2k}\\ &= \frac{154^2}{12} + \frac{(-14)^2}{12} + \frac{421^2}{12} = 16{,}762.75.\end{aligned}$$

The sums of squares are summarized in table IX-7b.

The blocks-within-replicate sum of squares adjusted for treatment effect is obtained from the following relation of sums of squares: blocks unadjusted + treatments adjusted = blocks adjusted + treatments unadjusted. Therefore, the sum of squares for blocks-within-replicates adjusted for treatments = block unadjusted + treatments adjusted − treatments unadjusted = 16,762.75 + 513,350.72 − 513,366.32 = 16,747.15, with the resulting mean square of 16,747.15/3 = 5,582.38 = E_b'.

The efficiency of this incomplete block design relative to what it would have been had a complete block design of twelve plots been used is

$$\frac{3\left(\dfrac{3E_b - E_e}{2}\right) + (11 + 19)E_e}{(3 + 11 + 19)E_e} = \frac{9(5582.38) + 57(3809.27)}{66(3809.27)} = 106 \text{ per cent},$$

or an increase in efficiency of 6 per cent. In this case, it is not assumed that the confounded effects are negligible.

In the event that it is desired to partition the 11 treatment degrees of freedom into eleven independent constrasts, it is possible to do so. The 2 degrees of freedom for the A effect may be partitioned into linear and quadratic effects, and the inter-

actions of these contrasts with the other factors yield the eleven comparisons listed below:

Effect	Treatment combinations												Divisors
	000	100	200	010	110	210	001	101	201	011	111	211	
A_L	−	0	+	−	0	+	−	0	+	−	0	+	3(8)
A_Q	+	−2	+	+	−2	+	+	−2	+	+	−2	+	3(24)
B	−	−	−	+	+	+	−	−	−	+	+	+	3(12)
C	−	−	−	−	−	−	+	+	+	+	+	+	3(12)
BC	+	+	+	−	−	−	−	−	−	+	+	+	3(12)
A_LB	+	0	−	−	0	+	+	0	−	−	0	+	3(8)
A_LC	+	0	−	+	0	−	−	0	+	−	0	+	3(8)
A_LBC	−	0	+	+	0	−	+	0	−	−	0	+	3(8)
A_QB	−	+2	−	+	−2	+	−	+2	−	+	−2	+	3(24)
A_QC	−	+2	−	−	+2	−	+	−2	+	+	−2	+	3(24)
A_QBC	+	−2	+	−	+2	−	−	+2	−	+	−2	+	3(24)
Total	+	+	+	+	+	+	+	+	+	+	+	+	3(12)

where L refers to linear effect and Q refers to quadratic effect and is not the same Q as used previously. The experimenter might argue that the above breakdown is not realistic and that one should compare a_1 with a_2 and the mean of a_1 and a_2 with a_0. There is no reason why this cannot be done if the experimenter desires. The coefficients are the same as in the rows of the above table, but the columns are interchanged.

IX-4.4 GENERAL METHODS OF ANALYSIS

The analysis of partially confounded factorial experiments may be generalized to a single procedure if one desires. Analyses of incomplete block designs appear to differ only because the shortest and easiest computational schemes are usually presented. Rao [255] and Kempthorne [175, Ch. 18] present general methods of analysis for incomplete block designs. It should be noted that these methods are applicable to the incomplete block designs presented in the following chapters, although the general method can be greatly simplified for a number of the designs.

The purpose here is to present the ideas and *not the computational procedure.* Consider the data of example IX-2 and suppose that we wish to compute the sum of squares for treatments eliminating block effects without resorting to the method described previously. The sum of squares may be obtained from the formula $\sum \hat{\tau}_{ijh} Q_{ijh}$. The $\hat{\tau}_{ijh}$ are the treatment effects for the eight treatments. The method for obtaining the $\hat{\tau}_{ijh}$ is often difficult. Since we already have the adjusted treatment totals in example IX-2, the treatment effects may be estimated from the quantity $\{X_{.ijh}' - X_{....}/8\}/r = \hat{\tau}_{ijh}$. The block effect has been removed from the treatment means. The Q_{ijh} values represent

the treatment total minus the mean of the block totals containing a particular treatment. The computations for the data of example IX-2 are

Treatment	Total $= X_{.ijh}$	Sum of blocks containing treatment $= B_{ijh}$	$4Q_{ijh} = (4X_{.ijh} - B_{ijh})$	$4\hat{\tau}_{ijh} = (X_{.ijh}' - X_{....}/8)$
000	50	444	−244	−61.455
100	122	489	−1	0.295
010	98	487	−95	−24.045
110	153	470	142	35.705
001	96	497	−113	−26.045
101	131	460	64	13.705
011	113	462	−10	−3.955
111	182	471	257	65.795
Total	945	4(945)	0	0

The $\frac{\sum(4\hat{\tau}_{ijh})(4Q_{ijh})}{16} = \frac{43118.180}{16} = 2694.886$, corresponding, but for a rounding error, to the value 2694.885 obtained in table IX-4. This procedure is presented for instructive and not computational purposes, since in practice we would have to compute the $\hat{\tau}_{ijh}$. Quantities like the Q_{ijh} are used frequently in the analysis of lattice designs. Kempthorne [175, Ch. 18] and Rao [255] describe the method for obtaining the $\hat{\tau}_{ijh}$ when the adjusted totals, $X_{.ijh}'$, are not available.

In order to use Rao's [255] method of analysis, it is necessary to compute the parameters of the design. This is done by noting the number of treatments $= v$, the number of replicates $= r$, the number of incomplete blocks $= b$, and the number of treatments in an incomplete block $= k$, and by determining the associations among treatments in the incomplete blocks. For the design in example IX-2 the following treatments appear together in an incomplete block in the various replicates (1, 2, 3, 4):

	000	100	010	110	001	101	011	111
000	—	1	3	2, 4	2	3, 4	1, 4	1, 2, 3
100	1	—	2, 4	3	3, 4	2	1, 2, 3	1, 4
010	3	2, 4	—	1	1, 4	1, 2, 3	2	3, 4
110	2, 4	3	1	—	1, 2, 3	1, 4	3, 4	2
001	2	3, 4	1, 4	1, 2, 3	—	1	3	2, 4
101	3, 4	2	1, 2, 3	1, 4	1	—	2, 4	3
011	1, 4	1, 2, 3	2	3, 4	3	2, 4	—	1
111	1, 2, 3	1, 4	3, 4	2	2, 4	3	1	—

There are three treatments, $n_1 = 3$, which appear together in an incomplete block in only one replicate, $\lambda_1 = 1$; these are first associates. Three treatments, $n_2 = 3$, appear together in an incomplete block in two of the replicates, $\lambda_2 = 2$; these are second associates. Also, only one treatment, $n_3 = 1$, appears with

any one other treatment in an incomplete block in three of the replicates, $\lambda = 3$; these are third associates. Therefore, Rao's first system of parameters is

$$v = 8,\ b = 8,\ k = 4,\ r = 4$$
$$\lambda_1 = 1,\ n_1 = 3;\ \lambda_2 = 2,\ n_2 = 3;\ \lambda_3 = 3,\ n_3 = 1.$$

The second system of parameters has to do with associations of other treatments with a particular pair of treatments. For example, treatments 000 and 100 are first associates, $\lambda_1 = 1$. These two treatments have no first associates in common. Two treatments, 010 and 001, are first associates of 000 and second associates, $\lambda_2 = 2$, of 100. There are no first associates of treatment 000 which are third associates, $\lambda_3 = 3$, of treatment 100. After the second associates and third associates of treatment 000 are compared with the associates of treatment 100, the following matrix is obtained:

$$p_{ij}^1 = \begin{pmatrix} 0 & 2 & 0 \\ 2 & 0 & 1 \\ 0 & 1 & 0 \end{pmatrix} = \begin{pmatrix} p_{11}^1 & p_{12}^1 & p_{13}^1 \\ p_{21}^1 & p_{22}^1 & p_{23}^1 \\ p_{31}^1 & p_{32}^1 & p_{33}^1 \end{pmatrix}.$$

Likewise, if we consider a pair of treatments which are second associates, e.g., 000 and 110, and a pair of treatments which are third associates, e.g., 000 and 111, the following two matrices are obtained:

$$p_{ij}^2 = \begin{pmatrix} 2 & 0 & 1 \\ 0 & 2 & 0 \\ 1 & 0 & 0 \end{pmatrix} \quad \text{and} \quad p_{ij}^3 = \begin{pmatrix} 0 & 3 & 0 \\ 3 & 0 & 0 \\ 0 & 0 & 0 \end{pmatrix}.$$

The above three matrices form the second system of parameters for the general method of analysis described by Rao [255].

The two systems of parameters for the experiment described in example IX-3 are

$$\begin{aligned} v &= 12, & b &= 6, & r &= 3, & k &= 6; \\ \lambda_0 &= 0, & n_0 &= 2; & \lambda_2 &= 2, & n_2 &= 4; \\ \lambda_1 &= 1, & n_1 &= 4; & \lambda_3 &= 3, & n_3 &= 1. \end{aligned}$$

$$p_{ij}^0 = \begin{pmatrix} 0 & 0 & 0 & 1 \\ 0 & 0 & 4 & 0 \\ 0 & 4 & 0 & 0 \\ 1 & 0 & 0 & 0 \end{pmatrix}, \quad p_{ij}^1 = \begin{pmatrix} 0 & 0 & 2 & 0 \\ 0 & 2 & 0 & 1 \\ 2 & 0 & 2 & 0 \\ 0 & 1 & 0 & 0 \end{pmatrix},$$

$$p_{ij}^2 = \begin{pmatrix} 0 & 2 & 0 & 0 \\ 2 & 0 & 2 & 0 \\ 0 & 2 & 0 & 1 \\ 0 & 0 & 1 & 0 \end{pmatrix}, \quad \text{and} \quad p_{ij}^3 = \begin{pmatrix} 2 & 0 & 0 & 0 \\ 0 & 4 & 0 & 0 \\ 0 & 0 & 4 & 0 \\ 0 & 0 & 0 & 0 \end{pmatrix}$$

Both of the above designs are partially balanced incomplete block designs.

IX-4.5 RECOVERY OF INTERBLOCK INFORMATION IN PARTIAL CONFOUNDING

In general, it is not recommended that interblock information in incomplete block designs be recovered *unless* the number of degrees of freedom associated with E_b' is greater than 12 to 14. If the degrees of freedom for E_b' are greater than 12 to 14, additional information may be obtained from this source. There are two reasons for the above statement: (i) estimates of variances with less than 12 to 14 degrees of freedom are subject to relatively large errors of estimation, and (ii) the number of degrees of freedom for a variance composed of a linear combination of E_b' and E_e is equal to some value between the degrees of freedom associated with E_b' and with E_e. Five per cent significance levels change rather slowly when the degrees of freedom are larger than 14. Also, if the population parameters of E_b' and E_e are equal, the combined variance has degrees of freedom equal to the sum of the individual degrees of freedom.

The analysis of variance for the design of example IX-2 recovering interblock (between incomplete block) information is of the form,

Source of variation	df	ms	Expected value of ms
Replicate	3	—	—
Treatment (ignoring block)	7	—	—
Block (eliminating treatment)	4	E_b'	$\sigma_\epsilon^2 + \frac{3}{4} 4\sigma_\beta^2$
Residual or intrablock	17	E_e	σ_ϵ^2
Total (corrected for the mean)	31	—	—

The expectation of E_b' is equal to $\sigma_\epsilon^2 + (r-1)k\sigma_\beta^2/r$ for incomplete block designs in which an effect is confounded in one of the r replicates and unconfounded in the other $r-1$ replicates with incomplete blocks of size k. The interactions are evaluated with variance $\sigma_\epsilon^2 + 4\sigma_\beta^2$ in the replicates in which they are confounded and with variance σ_ϵ^2 in the replicates in which they are unconfounded with incomplete block differences. The relative amounts of information are the reciprocal of the variances; they are estimated by $w' = (r-1)/(rE_b' - E_e)$ = interblock information and by $w = 1/E_e$ = intrablock information. If the levels of an effect, say $(BC)_{j+h}$, are weighted by the amount of information, then the weighted level of an effect, such as $(BC)_{j+h}$, is obtained from the formula,

$$\frac{4[w'(BC)_{j+h} \text{ in replicate I} + w(BC)_{j+h} \text{ in other replicates}]}{w' + 3w} = (BC'')_{j+h}. \quad \text{(IX-14)}$$

The other weighted effects are obtained from similar formulae. The adjusted treatment totals are obtained from the formula,

$$X_{\cdot ijh}'' = \frac{1}{4}\{(A)_i + (B)_j + (C)_h + (AB'')_{i+j} + (AC'')_{i+h} + (BC'')_{j+h} + (ABC'')_{i+j+h} - 3X_{\cdots\cdot}\}\cdot \tag{IX-15}$$

The double-prime values are not the same as the single-prime values obtained without recovery of interblock information.

The average standard error of a mean difference between two adjusted treatment means is

$$s_{\bar{d}} = \sqrt{\frac{2}{7}\left\{\frac{4}{3w + w'} + \frac{3}{4w}\right\}}\cdot \tag{IX-16}$$

The average effective error variance is

$$\frac{4}{2}s_{\bar{d}}^2 = \frac{4}{7}\left\{\frac{4}{3w + w'} + \frac{3}{4w}\right\}\cdot \tag{IX-17}$$

In the design of section IX-4.1, three replicates might have been selected instead of four; all two-factor interactions could have been confounded with the incomplete block differences. The selection of replicates II, III, and IV would result in such an arrangement. This design, like the design with four replicates, is partially balanced, since all two-factor interactions are confounded an equal number of times. Likewise, five replicates may be selected so that ABC is confounded in replicates I and V, and AB, AC, and BC are confounded with block differences in replicates IV, II, and III, respectively. Such a design would be unbalanced.

If, on the other hand, full information is desired on the interaction AB and the main effects in an experiment with four replicates, ABC could be confounded with block differences in replicates I and IV, AC in replicate II, and BC in replicate III. This is an unbalanced design. The breakdown of the total degrees of freedom, neglecting interblock information, is

Source of variation	df	ms
Replicate	3	
Blocks within replicates (ignoring treatment)	4	E_b
Main effects from all replicates	3	
Interactions (from unconfounded replicates)	4	
Error	17	E_e
Total	31	

and making use of interblock information is

Source of variation	df		ms	Expectation of ms
Replicate	3			
Block (eliminating treatment)	4		E_b'	$\sigma_e^2 + 3\sigma_\beta^2$
component (a)		1		$\sigma_e^2 + 4\sigma_\beta^2$
component (b)		1		$\sigma_e^2 + 2\sigma_\beta^2$
component (c)		2		$\sigma_e^2 + 3\sigma_\beta^2$
Treatment (ignoring block)	7			
Error	17		E_e	σ_e^2
Total	31			

where component (a) is the sum of squares for the interaction of levels of effect ABC with replicates I and IV; component (b) is the sum of squares for the comparison of the levels of effect ABC in replicates I and IV with the unconfounded levels in replicates II and III; component (c) is the sum of squares for the comparison of the levels of effects AC and BC in the replicate in which the effect is confounded with block differences with the corresponding levels of the effect in the replicates in which they are unconfounded with incomplete block differences.

The efficiency [see formulae (IX-6) and (IX-7)] of the above incomplete block design relative to the randomized complete block design, assuming the confounded interactions are negligible, is

$$\frac{4E_b + (7 + 17)E_e}{(4 + 7 + 17)E_e} = \frac{(E_b + 6E_e)}{7E_e},$$

and not assuming the confounded effects negligible, is

$$\frac{4[(4E_b' - E_e)/3] + (7 + 17)E_e}{(4 + 7 + 17)E_e} = \frac{4E_b' + 17E_e}{21E_e}.$$

The above formulae do not consider the information on interblock comparisons, in which case the efficiency is

$$(4E_b' + 17E_e)/21 \Big/ \frac{4E_e}{7}\left\{\frac{1}{2 + 2w'/w} + \frac{2}{3 + w'/w} + \frac{4}{4}\right\}, \qquad \text{(IX-18)}$$

where $w = 1/E_e$ and $w' = \dfrac{3}{4E_b' - E_e}$.

IX-5 Fractional Replication

If the number of treatments of a p^n factorial becomes quite large, a single replicate may involve too many experimental units for the available resources. In order to get around this difficulty, one may use a fraction of a complete replicate. The particular fraction of treatments of a p^n factorial to use has been discussed by Finney [118, 121] and Kempthorne [173] under the topic of *fractional replication* and by Plackett and Burman [249] under the topic of

optimum multifactorial experiments. Kempthorne [173] showed that the designs suggested by Plackett and Burman are fractionally replicated designs. In order to make use of the designs, certain high-order interactions must be negligible. Also, fractional replication in factorial experiments is likely to be most useful for the comparison of a large number of factors. For example, the experimenter might wish to use seven factors each at three levels in all combinations. By using a one-ninth replicate of $3^7 = 2187$ treatments, only 243 experimental units are required. Two hundred forty-three treatments out of the 2187 may be so selected that information is obtained on all main effects and two-factor interactions; also, a design of nine incomplete blocks of twenty-seven treatments, each using only a single replicate, may be utilized.[1] Such a scheme is in the realm of practicability.

The procedure for constructing a fractional replication is closely related to the concept of confounding. In the 2^n series the fractional replicate is in fractions of $1/2^s$ for $s < n$. Thus, a one-half replicate of a 2^3 involves four treatments, and a one-fourth replicate involves only two treatments. Likewise, in the 3^n series the fractional replicate consists of fractions of $1/3^s$ treatments. Finney [118, 121] sets up a *defining contrast* and the *aliases*. In the 2^n system the defining contrast consists of the treatments associated with the + signs in the highest-order interaction. The aliases consist of setting up equalities of effects. To illustrate, consider the 2^3 factorial. The defining contrast divides the eight treatments into two halves, the treatments with + values being 100, 010, 001, and 111. The A effect is determined from the relation 111 + 100 − 010 − 001, which is the same contrast as obtained for the BC contrast. Therefore, BC is an *alias* of A. The aliases for a one-half replicate of a 2^3 factorial are

Effect	Alias	
A	$BC = A \times ABC = A^2BC$	(mod 2)
B	$AC = B \times ABC = AB^2C$	"
C	$AB = C \times ABC = ABC^2$	"
I	$ABC = ABC \times ABC = A^2B^2C^2$	"

In a 2^4 factorial the half replicate would consist of the treatments 0000, 1100, 1010, 1001, 0110, 0101, 0011, and 1111, which are the treatments making up $(ABCD)_0$. The effects and aliases are

Effect	Alias	
A	$A \times ABCD = BCD$	(mod 2)
B	$B \times ABCD = ACD$	"
C	$C \times ABCD = ABD$	"
D	$D \times ABCD = ABC$	"
AB	$AB \times ABCD = CD$	"
AC	$AC \times ABCD = BD$	"
AD	$AD \times ABCD = BC$	"
I	$ABCD \times ABCD = I$	"

[1]See citation to problems.

Fractional replication of factorials may be combined with confounding. For example, a half replicate of a 2^6 factorial may be arranged in two blocks of sixteen treatments each [60, p. 192] or in four blocks of eight treatments each [173, p. 267]. Other schemes are described by Finney [121], and other arrangements are given by Kitagawa and Mitome [187].

IX-6 Missing Data

If an experimental unit has been damaged or destroyed in a completely confounded incomplete block design of the type described in example IX-1, the yield may be estimated by the ordinary formula for a randomized complete block design (see Chapter V). Only the incomplete blocks in which the treatment appears plus the incomplete block in which the yield for this treatment is missing are used in the formula. These blocks replace the replicates in the formula; only the treatments appearing with the missing one are considered.

In partial confounding the situation is more complex [60, sec. 6.34; 175, sec. 15.8]. Cochran and Cox discuss three situations for partial confounding; i.e., partial confounding in the factorials of the type p^n; partial confounding in factorials of the type $k \times p^n \times q^m$; and for a single replicate of the p^n factorial. The formula may be obtained by differentiating the algebraic expression for the residual sum of squares, setting the resulting equation equal to zero, and solving for the missing unit; or a covariance analysis may be performed with zeros used as the covariate of all values present and a one used as the covariate for the missing unit (see Chapters V, VI, and XVI).

CHAPTER X

Factorial Experiments with Main Effects Confounded —Split Plot and Split Block Designs with Variations

X-1 The Split Plot Design

X-1.1 INTRODUCTION

The very nature of the levels of one factor, say a, may be such as to exclude the use of small plots or units, or the experimenter may know that the levels of the factor usually differ in yield. In such circumstances the levels of factor a ($a_0, a_1, \cdots, a_{p-1}$) may be laid out in relatively large units (whole plots) designed as a randomized complete block, latin square, or other design. Since the *whole plots* are large *by necessity or by design*, it may be desirable to compare levels of another factor, say b, on each plot, the several levels—$b_0, b_1, \cdots, b_{q-1}$—being allotted to the *split plots* or *sub-plots* of each whole plot at random. Such an arrangement is a factorial arrangement of the factors a and b with p and q levels, respectively, in which the main effect A, with $p - 1$ degrees of freedom, is completely confounded with incomplete block or whole plot differences. Thus, the split plot design is an incomplete block design.

The whole plot treatments may themselves represent a factorial arrangement, say $k^n \times s^m$; the split plot treatments may also represent a factorial arrangement. The terminology "whole" or "split plot" treatment does not necessarily refer to the levels of a single factor, even though the explanation is given in these terms (see section IX-3).

Numerous examples of factors which require large experimental units are available in all fields of research. A few of these are listed below to illustrate the diversity of material for which split plot designs are suitable.

(i) In experimental education a movie film may be used by several teachers and on several sets of students; the film is a single experimental unit as far as replication on the film is concerned. Perhaps replication on films could be obtained by filming the material on different types of film under different conditions, by different operators and cameras, etc.

(ii) In greenhouse temperature studies, it may be necessary to keep the entire greenhouse at a constant temperature. Several treatments may be conducted in the greenhouse, but the greenhouse is used as a unit. Heat chambers, storage cellars, freezing units, baking ovens, etc. must also be utilized as a single experimental unit.

(iii) In research on milking machines a relatively large amount of milk is required. Methods of cooling or pasteurizing require smaller amounts of milk and may be utilized as split plot treatments.
(iv) In the preparation of metal alloys a smelting or blast furnace requires large amounts of material, whereas some treatments, such as types of mold, require relatively small amounts.
(v) In palatability studies, one judge, the whole plot, is usually capable of testing several items, the sub-plots. Likewise, in rating material a single individual is capable of scoring a number of items.
(vi) In crop tillage or harvesting studies a large amount of material is usually required. Hence, sub-plot treatments may be applied to each whole plot.
(vii) Insecticidal or fungicidal research with ordinary farm operational machinery requires large amounts of materials; these may be subdivided into smaller lots for sub-plot treatments.
(viii) In feeding trials the unit is an animal. The carcass may be subdivided to study methods of cooking, storage, tenderizing, etc.
(ix) In baking studies the unit is the batch mixed at one time. The batch may be subdivided either before or after baking or both before and after baking, to study other treatments.
(x) The smallest unit in some plant-response studies is a single plant, but the plant may be subdivided into subsamples to study methods of chemical analyses to determine plant composition. Likewise, to study plant response the whole plant is necessary in some treatments, whereas a leaf or half leaf is suitable for other treatments.
(xi) The store or farm may be the unit in certain management studies, while methods of displaying or producing the commodity may be compared within the store or on the farm.

For certain types of material, such as the above, and for cases in which the experimental procedure is not clearly stated or understood, it is sometimes assumed that the $p \times q$ factorial is arranged in a randomized complete block design, when in reality the design is a split plot. Such a mistake results in incorrect estimates of the treatment variances and in incorrect tests of hypotheses. It is imperative that the conduct of an experiment be thoroughly understood in order that correct analyses may be made.

X-1.2 ADVANTAGES AND DISADVANTAGES

The advantages of the split plot design are

(i) Experimental units which are large by necessity or design may be utilized to compare subsidiary treatments.
(ii) Increased precision over a randomized complete block design of the pq treatments is attained on the sub-plot treatments and on the interaction of sub-plot and whole plot treatments.
(iii) The over-all precision of the split plot design relative to the randomized complete block design of the pq treatments may be increased by designing the whole plot treatments in a latin square design or in an incomplete latin square design.

The disadvantages of the split plot design are:

(i) The whole plot treatments are measured with less precision than they are in a randomized complete block design of pq treatments.

(ii) When missing data occur, the increase in complexity of the analysis for the split plot design is greater than for the randomized complete block design.

X-1.3 RANDOMIZATION

The randomization procedure for the main or whole plots is determined by the particular design chosen. If a latin square or randomized complete block is selected, the experimental layout follows that described in Chapters V and VI. If one of the incomplete block designs described in Chapter IX is used, the randomization procedure is as described therein. Also, one of the more complex designs described in later chapters may be utilized.

The sub-plot treatments are randomly allotted to the units within each whole plot. A different randomization is used within each whole plot. Alternatively, the sub-plot treatments may be designed in various ways to control variation among sub-plots within a whole plot. The particular design chosen will determine the randomization procedure (section X-1.4.3).

X-1.4 ANALYSIS

X-1.4.1 Whole plots in a randomized complete block design. For an experiment in which the whole plots are laid out in a randomized complete block design with $3 = r$ replicates, $4 = p$ levels of the factor a, and $3 = q$ levels of the factor b, the breakdown of the degrees of freedom is as presented in table X-1.

TABLE X-1. Experimental lay-out and analysis of variance for a split plot design

Replicate I

a_3	a_1	a_2	a_0
b_2	b_2	b_1	b_1
b_0	b_0	b_2	b_2
b_1	b_1	b_0	b_0

Replicate II

a_1	a_0	a_2	a_3
b_1	b_0	b_0	b_1
b_2	b_2	b_1	b_2
b_0	b_1	b_2	b_0

Replicate III

a_1	a_3	a_0	a_2
b_2	b_2	b_0	b_1
b_1	b_0	b_2	b_0
b_0	b_1	b_1	b_2

Analysis of variance

Source of variation	df
Whole plots	
Replicate	$r-1 = 2$
A	$p-1 = 3$
Error (a)	$(r-1)(p-1) = 6$
Split plots	
B	$q-1 = 2$
AxB	$(q-1)(p-1) = 6$
Error (b)	$p(q-1)(r-1) = 16$
Total	$pqr-1 = 35$

Since the levels of factor a are compared in three replicates of a randomized complete block design, error (a) (the replicate $\times$ A interaction mean square) is the appropriate error for comparing the A effect. Likewise, error (b), which is a composite of the interaction sums of squares for replicate $\times$ B and replicate $\times$ A $\times$ B, is the appropriate error mean square for testing the $A \times B$ interaction, and for testing the B effect in some instances [126, sec. 65]. The replicate $\times$ B and replicate $\times$ A $\times$ B effects are confounded with each other and are not separable as such [108]. Even though the calculation of these two interactions is possible arithmetically, both interactions are estimates of the same quantity and should be pooled together in the error (b) sum of squares. This quantity may appropriately be called a split-plot $\times$ replicate (B $\times$ replicate) sum of squares within whole plots.

The above discussion brings up the question—how does this incomplete block design, the split plot design, compare with a randomized complete block design with regard to the contrasts on the main effects and the interactions? In making this comparison, several points need to be considered. The B and $A \times B$ effects are usually estimated more accurately than the whole plot treatments (the A effects), since the variation within incomplete blocks is usually smaller than among the incomplete blocks. Also, the number of degrees of freedom available for whole plot comparisons is smaller than for split plot comparisons. Since the *average* standard error of a difference is the same for both the incomplete block and randomized complete block designs, there is no over-all gain in accuracy due to using the incomplete block design. The increased accuracy on the B and $A \times B$ effects is obtained by sacrificing accuracy on the A effects. Also, both sets of comparisons have fewer degrees of freedom available for the error variances, which makes the randomized complete block design somewhat superior for all comparisons.

Disregarding the difference in number of degrees of freedom, the efficiency of the split plot design relative to the randomized complete block design on the B and $A \times B$ comparisons is

$$\frac{[(p-1)+(p-1)(r-1)]E_a+[(q-1)+(q-1)(p-1)+p(r-1)(q-1)]E_b}{(pqr-r)E_b}$$

$$=\frac{(p-1)E_a+p(q-1)E_b}{(pq-1)E_b}=\frac{E_e'}{E_b}, \tag{X-1}$$

where p = number of whole plot treatments, q = number of split plot treatments, r = number of replicates, E_a = error (a) mean square, and E_b = error (b) mean square. On the other hand, the efficiency on the A effects or the whole plot comparisons would be decreased, the formula in this case being E_e'/E_a.

Example X-1. During the spring of 1944 a seed-germination test on forty-nine

varieties of guayule was conducted in a greenhouse. The following four seed treatments were applied to lots of seed from the varieties:

(i) 1943 collected seed was threshed but not treated with sodium hypochlorite $= b_0$;
(ii) 1943 collected seed was neither threshed nor treated with sodium hypochlorite $= b_1$;
(iii) 1942 collected seed was not threshed but treated with sodium hypochlorite $= b_2$;
(iv) 1943 collected seed was not threshed but treated with sodium hypochlorite $= b_3$.

Since the comparisons on seed treatments and on the interaction of varieties and seed treatments were considered to be more important than those on variety mean germinations, a split plot design with varieties as whole plots was used. Six replicates of a randomized complete block design were used for the varieties. The whole plot was a greenhouse flat subdivided into four sections (the split plots). One hundred seeds were planted in each split plot. The data recorded were number of plants emerged.

For an illustrative example, eight varieties, $a_0, a_1, \cdots, a_7$ were selected from the first three replicates of this planting. The number of plants that emerged from a hundred seeds for the variety and seed treatments are recorded in table X-2 for each split plot in the three replicates.

The necessary totals for the analysis of variance are given in table X-2. The correction term is equal to

$$\frac{X_{\dots}^2}{\text{total no. of plots}} = \frac{2429^2}{8(3)(4)} = 61{,}458.76 = CT(1df).$$

The total sum of squares is equal to

$$12^2 + 10^2 + \cdots + 11^2 + 15^2 - CT = 98{,}195 - CT = 36{,}736.24(95\ df).$$

The replicate sum of squares is equal to

$$\frac{825^2 + 781^2 + 823^2}{8(4) = 32} - CT = 38.58(2df).$$

The variety sum of squares is equal to

$$\frac{296^2 + 322^2 + \cdots + 333^2}{4(3) = 12} - CT = 763.16(7df).$$

The variety by replicate or error (a) sum of squares is equal to

$$\frac{97^2 + 89^2 + \cdots + 101^2 + 110^2}{4} - 38.58 - 763.16 - CT = 1377.25(14df).$$

The seed-treatment sum of squares is equal to

$$\frac{1340^2 + 334^2 + 481^2 + 274^2}{3(8) = 24} - CT = 30{,}774.28(3df).$$

The treatment by variety sum of squares is equal to

$$\frac{199^2 + 190^2 + \cdots + 30^2 + 36^2}{3} - CT - 763.16 - 30{,}774.28 = 2620.13(21df).$$

TABLE X-2. Number of plants germinating from 100 seeds for each of four seed treatments on each of eight guayule varieties in a split plot design of three replicates, and totals for the analysis of variance

Replicate I

	a_0b_1	a_2b_3	a_3b_0	a_5b_2	a_4b_2	a_1b_1	a_7b_3	a_6b_1	
	12	10	52	28	9	26	9	12	
	a_0b_2	a_2b_0	a_3b_3	a_5b_3	a_4b_3	a_1b_2	a_7b_1	a_6b_2	
	13	51	13	14	12	27	14	26	
	a_0b_0	a_2b_1	a_3b_2	a_5b_1	a_4b_0	a_1b_0	a_7b_2	a_6b_3	
	66	8	19	8	45	77	30	15	
	a_0b_3	a_2b_2	a_3b_1	a_5b_0	a_4b_1	a_1b_3	a_7b_0	a_6b_0	
	6	20	4	59	20	15	49	56	
Total	97	89	88	109	86	145	102	109	825

Replicate II

	a_3b_2	a_6b_0	a_7b_3	a_4b_1	a_0b_3	a_1b_2	a_5b_1	a_2b_1	
	16	38	15	13	12	5	8	16	
	a_3b_1	a_6b_2	a_7b_0	a_4b_3	a_0b_0	a_1b_0	a_5b_2	a_2b_2	
	15	16	41	12	63	47	32	30	
	a_3b_3	a_6b_1	a_7b_2	a_4b_0	a_0b_2	a_1b_1	a_5b_3	a_2b_0	
	9	16	28	51	13	11	21	81	
	a_3b_0	a_6b_3	a_7b_1	a_4b_2	a_0b_1	a_1b_3	a_5b_0	a_2b_3	
	40	8	20	10	10	4	66	14	
Total	80	78	104	86	98	67	127	141	781

Replicate III

	a_6b_3	a_7b_2	a_5b_0	a_4b_2	a_3b_2	a_2b_2	a_0b_1	a_1b_1	
	7	36	49	12	7	29	13	18	
	a_6b_2	a_7b_1	a_5b_2	a_4b_0	a_3b_0	a_2b_1	a_0b_3	a_1b_0	
	24	25	29	52	59	14	7	66	
	a_6b_1	a_7b_0	a_5b_3	a_4b_1	a_3b_1	a_2b_3	a_0b_0	a_1b_2	
	16	54	16	16	11	10	70	11	
	a_6b_0	a_7b_3	a_5b_1	a_4b_3	a_3b_3	a_2b_0	a_0b_2	a_1b_3	
	45	12	8	11	7	63	11	15	
Total	92	127	102	91	84	116	101	110	823

TABLE X-2. (*continued*).

Variety and treatment totals

Treatments	Varieties								Total
	a_0	a_1	a_2	a_3	a_4	a_5	a_6	a_7	
b_0	199	190	195	151	148	174	139	144	1340
b_1	35	55	38	30	49	24	44	59	334
b_2	37	43	79	42	31	89	66	94	481
b_3	25	34	34	29	35	51	30	36	274
Total	296	322	346	252	263	338	279	333	2429

TABLE X-3. Analysis of variance of data in table X-2

Source of variation	df	ss	ms
Whole plot analysis			
Replicate	2	38.58	19.29
Variety = A	7	763.16	109.02
Error (a)	14	1377.25	98.38
Split or sub-plot analysis			
Treatments = B	3	30774.28	10258.09
b_0 vs $b_1 + b_2 + b_3$	1	29829.03	-
b_1 vs $b_2 + b_3$	1	52.56	-
b_2 vs b_3	1	892.69	-
A x B	21	2620.13	124.77
A x b_0 vs $b_1 + b_2 + b_3$	7	1456.72	208.10
A x b_1 vs $b_2 + b_3$	7	632.94	90.42
A x b_2 vs b_3	7	530.48	75.78
Error (b)	48	1162.84	24.23
Total	95	36736.24	-

The error (b) (treatment by replicate plus treatment by replicate by variety) sum of squares with $2(3 + 21) = 48$ degrees of freedom is equal to

$$36{,}736.24 - 38.58 - 763.16 - 1377.25 - 30{,}774.28 - 2620.13 = 1162.84.$$

Since certain contrasts among the seed treatments were of interest, the 3 degrees of freedom for seed treatments were partitioned into individual degrees of freedom. The b_0 seed treatment was compared with the mean of the remaining three. This contrast accounted for the major portion of the sum of squares for treatments; thus:

$$\frac{[3(1340) - 334 - 481 - 274]^2}{24(9 + 1 + 1 + 1)} = 29{,}829.03.$$

The sum of squares for the comparison of the unthreshed untreated = b_1 mean with the mean of b_2 and b_3 is

$$\frac{[2(334) - 481 - 274]^2}{24(4 + 1 + 1)} = 52.56,$$

and for the comparison of the two unthreshed seed collections in different years treated with sodium hypochlorite is

$$\frac{(481 - 274)^2}{24(1 + 1)} = 892.69.$$

Another comparison[1] of interest was the one for the treated and untreated samples from 1943 collected seed; thus:

$$\frac{(334 - 274)^2}{24(1 + 1)} = 75.00.$$

The interaction of varieties and seed treatments is larger, $F = 124.77/24.23 = 5.15 > F_{01}$, than is expected under the hypothesis of no interaction. The variety $\times$ treatment sum of squares may be partitioned into three sets of 7 degrees of freedom, corresponding to the interaction of the three contrasts with varieties. All are significant at the 5 per cent level, even though the contrast of threshed with unthreshed accounts for a large proportion of the interaction sum of squares. The calculation of these sums of squares is left as an exercise for the reader [273, Ch. 15].

The correct error term for testing differences between variety means is error (a) *if it is assumed that these particular seed treatments are not a random sample of seed treatments but are the only ones of interest.*[2] If the seed treatments are considered to be a random sample, no appropriate error, as such, exists. The F test of the hypothesis of no difference among varieties in the germination of 1200 seeds with the four seed treatments b_0, b_1, b_2, and b_3 is $F = 109.02/98.38 = 1.11$, which is near the mean value of $F[E(F) = \text{error } df/(\text{error } df - 2) = 14/12]$. The replicate mean square is small but not significantly so, $F = 98.38/19.29 = 5.10$, which is less than the tabulated $F(= 19.42)$ value at the 10 per cent level of probability for a two-tailed test.

The choice of the experimental error variance for comparing the variation among mean germination for the four seed treatments depends upon the hypothesis being tested [126, sec. 65; Chapter V]. *If the eight varieties constitute a random sample from a population of guayule varieties and if a single seed treatment is being recommended for all guayule varieties*, the $A \times B$ interaction mean square is the appropriate error for making an F test; thus, $F = 10{,}258.09/124.77 = 82.2 > F_{01} = 4.87$. If, on the other hand, these eight guayule varieties *represent the only varieties of interest* and if the recommended seed treatment is for the eight varieties, then the error (b) mean square (24.23) is the correct error for comparing the seed-treatment means. In either case the interpretation is the same for this example.

A third situation may arise in which it is assumed that the eight varieties *constitute a random sample of guayule varieties, but a different seed treatment will be recommended for each variety*, since there is known to be a variety by seed-treatment inter-

[1]This comparison is not independent of the above three comparisons.

[2]If one is interested in testing variety differences, not solely differences among means, the error (a) mean square is the appropriate mean square for either the finite or the infinite model.

action. In this case the correct experimental error mean square for a particular mean $\bar{x}_{\cdot ij}$ is the error (*b*) mean square; the standard error of $\bar{x}_{\cdot ij}$ is $\sqrt{\text{error } (b)\ ms/r}$.

Standard errors of a mean difference are available [60, 219, 287] for the several comparisons that may be made among the $4 \times 8 = 32$ seed-treatment and variety combinations. The standard error of a difference between two variety means on a split plot basis is

$$s_{\bar{d}} = \sqrt{\frac{2E_a}{rq}} = \sqrt{\frac{2(98.38)}{3(4)}} = 4.05, \tag{X-2}$$

where E_a = error (*a*) and E_b = error (*b*) mean squares.

The standard error of a difference between two seed-treatment means on a single plot basis for the first hypothesis cited above is

$$s_{\bar{d}} = \sqrt{\frac{2(\text{interaction } ms)}{pr}} = \sqrt{\frac{2(124.77)}{8(3)}} = 3.22, \tag{X-3}$$

and for the second case given above is

$$s_{\bar{d}} = \sqrt{\frac{2E_b}{pr}} = \sqrt{\frac{2(24.23)}{8(3)}} = 1.42. \tag{X-4}$$

The standard error of a difference between two *b* (seed-treatment) means at one level of the factor *a* (varieties) is

$$s_{\bar{d}} = \sqrt{\frac{2E_b}{r}} = \sqrt{\frac{2(24.23)}{3}} = 4.02. \tag{X-5}$$

The standard error of a difference between two *a* (variety) means at the same level of the factor *b* (the same seed treatment) is

$$s_{\bar{d}} = \sqrt{\frac{2[(q-1)E_b + E_a]}{rq}} = \sqrt{\frac{2[3(24.23) + 98.38]}{3(4)}} = 5.34. \tag{X-6}$$

The degrees of freedom for the standard errors of a mean difference given by formulae (X-2) to (X-5) are those associated with the particular mean square used. However, the number of degrees of freedom associated with $s_{\bar{d}}$ from formula (X-6) must be approximated [55, 287].

The above standard errors are appropriate *if the whole plot treatments* have been randomized within the complete blocks. If a systematic arrangement of the varieties had been used, then only the second, third, and fourth standard errors (formulae (X-3) to (X-5)) of a mean difference would be applicable. The first and fifth standard errors would not be applicable for a systematic arrangement of the varieties. Because of these complications, systematic arrangements of the whole plot treatments are to be avoided.

The efficiency of this split plot design on the B and $A \times B$ comparisons relative to what it would be for the thirty-two combinations allotted at random to the thirty-two plots in each replicate is estimated to be

$$\frac{(7 + 14)(98.38) + (3 + 21 + 48)(24.23)}{93(24.23)} = \frac{40.97}{24.23} = 169 \text{ per cent,}$$

and for the A comparisons is $40.97/98.38 = 42$ per cent. On the former comparisons a gain in precision of 69 per cent was obtained, while for the latter comparisons, the

variety comparisons, this design was less than half as efficient as the randomized complete block design.

X-1.4.2 Whole plots in a latin square design. In some instances depending upon number of replicates and the experimental conditions, the whole plots may be arranged in a latin square design. By so doing the comparisons on the A effects or whole plot contrasts may be *more precise* than if the pq combinations of the factors a and b had been laid out in a randomized complete block design, since the latin square design is often more efficient than the randomized complete block design [201, 319]. The breakdown of the total degrees of freedom in the analysis of variance is

Source of variation	df	ms
Row	$p - 1$	E_r
Column	$p - 1$	E_c
A = whole plot treatments	$p - 1$	
Error (a)	$(p - 1)(p - 2)$	E_e
B = sub-plot treatments	$q - 1$	
A × B	$(p - 1)(q - 1)$	
Error (b)	$p(p - 1)(q - 1)$	E_b
Total	$qp^2 - 1$	

For the comparisons among whole plot treatments the efficiency of this design relative to a randomized complete block is estimated by

$$\frac{E_e'}{E_e} = \frac{(E_c + (p-1)E_e)(p-1)/p + p(q-1)E_b}{(pq-1)E_e}. \tag{X-7}$$

From experimental evidence on field crops [201, 319], it has been found that comparisons of whole plots arranged in a latin square are almost as precise as if a randomized complete block design had been used for the pq treatments. Thus, little information may be lost on whole plot comparisons and considerable gains in information may be made on sub-plot comparisons by designing the whole plots in a latin square. This feature adds considerably to the attractiveness of split plot designs.

X-1.4.3 Whole plots and split plots arranged in latin square designs. Another variation of the split plot design is to arrange the treatments so that each split plot treatment appears once in each order in each replicate for a particular main plot treatment. For example, the illustrative design in table X-1 could be designed as in table X-4, where the split plot treatments occupy all orders in the three replicates. Four of the twelve arrangements of a 3 × 3 latin square are selected at random and laid out in each of the four treatments a_0, a_1, a_2, and a_3. In such a design the ordinary error (b) sum of squares is partitioned into the two portions: orders within treatments and the residual variation.

If the split plot treatments are equal to the whole plot treatments in number and are arranged to occupy all orders within whole plot treatments for a particular replicate, the sum of squares for orders also represents one component of the $A \times B$ interaction. Such an arrangement is undesirable if the $A \times B$ interaction component is not equal to zero. If the whole by split plot interaction is nonexistent, it may be desirable to confound one of the

TABLE X-4. Field lay-out for a split plot design with the whole plots in a randomized complete block arrangement and the split plots in a latin square arrangement within whole plot treatments

	Replicate I					Replicate II					Replicate III			
Order	a_3	a_1	a_2	a_0	Order	a_1	a_0	a_2	a_3	Order	a_1	a_3	a_0	a_2
1	b_2	b_2	b_0	b_2	1	b_0	b_1	b_2	b_1	1	b_1	b_0	b_0	b_1
2	b_1	b_0	b_2	b_0	2	b_1	b_2	b_1	b_0	2	b_2	b_2	b_1	b_0
3	b_0	b_1	b_1	b_1	3	b_2	b_0	b_0	b_2	3	b_0	b_1	b_2	b_2

Analysis of variance

Source of variation	df
Replicate	2
A = whole plot treatment	3
Error (a)	6
B = sub-plot treatment	2
AxB	6
Orders within each whole plot	8
Residual = error (b)	
Total	35

components of the $A \times B$ interaction with orders within whole plot treatments for each replicate to control this source of variation.

For $p = q$, a variant of the design of section X-1.4.2 is to arrange the whole plot treatments in a $p \times p$ latin square and to arrange the split plot treatments in orders within each treatment. The form of the analysis for whole plots is the same as described in section X-1.4.2, and for the sub-plots as given in table X-4.

In designing the whole or split plots in latin squares, account should be taken of the number of degrees of freedom associated with the error (a) and error (b) mean squares. For small values of p the decrease in the error mean squares may be offset by the loss in the associated degrees of freedom. For the design described in table X-4, only 8 degrees of freedom are available for the residual mean square, whereas 16 degrees of freedom are available if orders are ignored. For a number of experiments the decrease in the error mean square may be more than offset by the loss in degrees of freedom, resulting in less precise comparisons.

X-1.4.4 Repetitions of experimental results. A particular type of experimentation involves repetition of the treatments in a design (for example, the randomized complete block, the latin square, or other designs) over several intervals of time, over several locations, or over several groups of material. It might be argued that these designs are not *true* split plot designs, but since the design and analytical features are of the same nature as split plot designs, there is little reason to set up a separate category for these designs.

Warriner [303] conducted an experiment on the readability of fifteen high school physics textbooks. Sample passages were selected from ten comparable parts of each book and three students were assigned to take tests on a given part of every book. In all, thirty students participated in the test. The key-out of the degrees of freedom for this design is

Source of variation	df	ms
Part of book	9	—
Among students within parts	20	E_a
Text	14	—
Text × part of book	126	—
Text × student within parts	280	E_b
Total	449	—

The mean square for parts of the book are compared with E_a. The mean squares for texts and the text by part of book interaction are each compared with E_b, since these texts and parts of text represent the only items of interest.

A design similar to that described above is the randomized complete block experiment repeated at several locations. The numbering of the replicates at the various locations is purely arbitrary, just as was the numbering of the students in the above experiment. If location, treatment, and replicate are substituted for part of book, textbook, and student, respectively, the key-out of the degrees of freedom for a randomized complete block design laid out at several locations has the same analysis of variance as given above (see example X-2). The tests of significance are the same if a treatment is to be recommended for each location. If the locations are considered as a sample of locations, the location × treatment interaction is used as the error mean square (see table X-16).

Similar analyses are suitable for annual crops repeated over several time periods (for example, years) at one location or at one laboratory. However, for perennial crops repeated over several years, for several harvests of one crop in a single year, or for several scorings by judges, the analysis is similar to that for a $p \times q \times r$ factorial, but the tests of significance are different. The breakdown of the degrees of freedom in the analysis of variance for y years

= A effect and t treatments = B effect in r replicates of a randomized complete block of a perennial crop is

Source of variation	df	ms
Replicate	$r - 1$	—
Year = A	$y - 1$	—
A × replicate	$(r - 1)(y - 1)$	E_a
Treatment = B	$t - 1$	—
B × replicate	$(r - 1)(t - 1)$	E_b
A × B	$(y - 1)(t - 1)$	—
A × B × replicate	$(r - 1)(y - 1)(t - 1)$	E_e
Total	$ryt - 1$	—

The appropriate error mean square for a particular comparison depends upon the nature of the experimental material and the hypothesis tested [47, 273, 279, 291]. One way of looking at the above experiment is to consider that the experimental unit is represented by the total yield obtained in the y years. The analysis then reduces to that for an ordinary randomized complete block design. If, on the other hand, the above form of the analysis of variance is desired and if the years are considered to be the only ones of interest, then E_b is the appropriate mean square for testing the treatment mean square. Likewise, for these particular treatments, E_a is appropriate for testing the A mean square, even though only one sample is available on any one year. For the infinite model, no appropriate error mean square is available for testing the A and B mean squares; if desired, it is possible to construct a synthetic mean square, and to compute the approximate number of degrees of freedom associated with it [55, 287].

If a single experiment is harvested several times in a single season or if several readings are taken by a judge or scorer, the above discussion on perennial crops applies here as well.

In the analyses of experiments the question arises concerning the independence of the results obtained from observing a particular experimental unit several times. For example, in a forage experiment a high yield of the experimental unit in the first cutting may result in a lower yield, relatively, for the second cutting because more available soil resources were utilized during the first growing period, leaving less for the plants during the second period. Correlation of subsequent observations affects tests of significance in the analysis of variance. One method for investigating the independence of results from experiments of this nature is to use some form of multivariate analysis [279, 291]. However, if total yields over the period are used, the correlation from cutting to cutting (competition in the broad sense) may be ignored. From data on forage crops (grasses) there is some evidence to indicate that the harvest-to-harvest correlation within one year may be appreciable

but that the year-to-year harvests may not be appreciably related. Whether or not this is true for other perennial crops is not known, but it is suspected that year-to-year harvests of some tree crops are rather highly correlated. Application of multivariate analyses on a large number of experiments would yield the evidence on the independence of observations in perennial crop experiments.

Cochran [47], Cochran and Cox [60], Lowe [197], Steel [279], Tukey [291], Wishart and Hines [315], and Yates and Cochran [332] present discussions on analyses for several repetitions of an experiment. The paper by Yates and Cochran is especially enlightening.

Example X-2. Twelve districts of approximately equal size are set up in Iowa for the purpose of conducting corn yield trials [111]. Yield tests on corn are conducted annually in each district. All entries in the trials within a region are the same, but may differ from region to region. The entries from year to year seldom remain the same, although a few are tested more than one year.

In Districts I and II, six double-cross corn hybrids were tested in 1942 and in 1943. The yield data for 1942 are given in table X-5, and for 1943 in problem X-1. The trials were grown on a farm (supposedly selected at random) within each district, the farm being designated as a location. The design was a randomized complete block with four replicates in each district each year. The plot size was 2 × 10 hills of corn with three plants per hill. The data recorded were yield of ear corn per 2 × 10 hill plot. The replicates were numbered I, II, III, and IV in District I and V, VI, VII, and VIII in District II; replicate I in District I has nothing in common with the first replicate in District II. The analysis of variance for each of the districts and for the combined analysis is presented in table X-6.

The separate analyses should always be made prior to pooling the results. Hidden features of the data may be brought to light in the individual analyses that are obscured in the pooled analysis. Outside of computing the mean squares, no additional computations are necessary for the individual analyses that are not required for the pooled analysis. The sum of the individual correction terms minus the over-all correction for the mean yields the sum of squares attributable to locations,

$$27{,}027.88 + 33{,}870.11 - 60{,}705.19 = 192.80 = (805.4 - 901.6)^2/48,$$

with a single degree of freedom. The total sum of squares corrected for the mean is

$$27{,}172.12 + 34{,}442.80 - 60{,}705.19 = 909.73.$$

The variety × replicate within location or place sum of squares is the sum of the sums of squares for the individual analyses, 56.27 + 287.33 = 343.60, with 15 + 15 = 30 degrees of freedom. The replicates within location sum of squares is the sum of the replicate sum of squares for the separate analyses,

$$17.61 + 44.71 = 62.32, \text{ with } 3 + 3 = 6 \text{ degrees of freedom.}$$

The variety within locations sum of squares, 70.36 + 240.65 = 311.01, contains the variety or hybrid and the hybrid × location sums of squares. The former is equal to

$$\frac{263.6^2 + 309.5^2 + \cdots + 284.5^2}{8} - 60{,}705.19 = 191.51,$$

TABLE X-5. Yield data (pounds of ear corn per 2 × 10 hill plot) for six double-cross corn hybrids planted in Districts I and II of Iowa in 1942. Systematic arrangement of the yields in the four replicates

Doublecross designation	District I					District II					Doublecross totals
	Replicate Number				Total	Replicate Number				Total	
	I	II	III	IV		V	VI	VII	VIII		
1-1	34.6	33.4	36.5	33.0	137.5	33.1	24.6	33.8	34.6	126.1	263.6
2-2	34.5	39.1	35.4	35.6	144.6	46.4	36.9	36.3	45.3	164.9	309.5
4-3	30.1	30.8	35.0	33.3	129.2	32.3	38.7	37.5	37.6	146.1	275.3
15-43	31.3	29.3	29.7	33.2	123.5	37.5	39.2	39.1	34.1	149.9	273.4
8-38	32.8	35.7	36.0	34.0	138.5	31.2	40.8	46.1	44.1	162.2	300.7
7-39	30.7	35.5	35.3	30.6	132.1	35.8	38.2	38.8	39.6	152.4	284.5
Total	194.0	203.8	207.9	199.7	805.4	216.3	218.4	231.6	235.3	901.6	1707.0

TABLE X-6. Analyses of variance of the data in table X-5

Source of variation	District I df	District I ss	District I ms	District II df	District II ss	District II ms
Replicate	3	17.61	5.87	3	44.71	14.90
Hybrid	5	70.36	14.07	5	240.65	48.13
Error	15	56.27	3.75	15	287.33	19.16
Total	23	144.24	-	23	572.69	-
Correction for mean	1	27027.88	-	1	33870.11	-
Total uncorrected	24	27172.12	-	24	34442.80	-

Combined analysis: Source of variation	df	ss	ms
Location	1	192.80	192.80
Replicates within locations	6	62.32	10.39
Hybrid	5	191.51	38.30
Hybrid x location	5	119.50	23.90
Hybrid x replicates within locations	30	343.60	11.45
Total	47	909.73	-
Correction for mean	1	60705.19	-
Total uncorrected	48	61614.92	-

and the latter is 311.01 − 191.51 = 119.50; each sum of squares has 5 degrees of freedom. The sum of the above sums of squares should add to the total sum of squares within rounding errors.

Tests of significance may be made in the manner discussed previously. If it is assumed that the fields on which the six hybrids were planted represent a random sample of fields or locations, and if it is desired to recommend one hybrid for the area comprised by Districts I and II, the interaction mean square, 23.90, is an unbiased estimate of the experimental error for comparing the differences among hybrid means. The F test, $F = 38.30/23.90 = 1.60$, indicates little evidence for rejecting the null hypothesis. However, further inquiry into the data indicates that the relatively large, though not significant, interaction mean square is mainly due to the relative difference in yields for hybrid 1-1 at the two locations (figure X-1). Hybrid 2-2 was highest at both locations, and hybrid 8-38 was second highest at both locations.

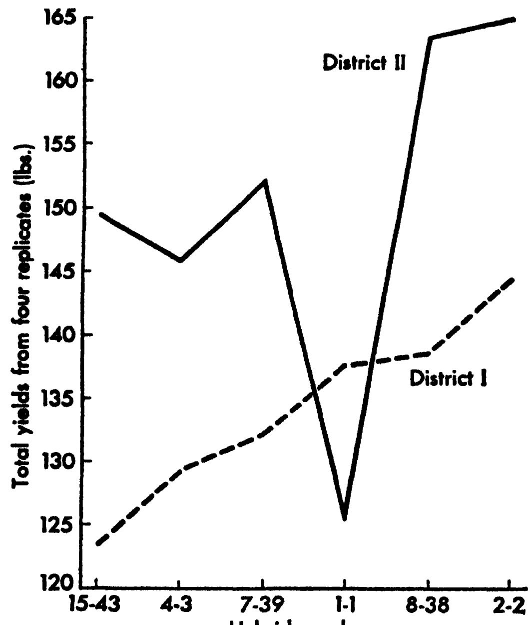


Figure X-1. Graphical representation of yields of six hybrids in two districts

The F test of the error variances at the two places, $F = 19.16/3.75 = 5.11 > F_{.05} = 2.86$, indicates heterogeneity of the error variances. Despite this, the pooled error may be considered the best estimate of the local experimental error, since the coefficients of variation,

$$\frac{24\sqrt{3.75}}{805.4} = 6 \text{ per cent for District I}$$

and

$$\frac{24\sqrt{19.16}}{901.6} = 12 \text{ per cent for District II,}$$

are expected to be about 8 to 12 per cent for corn yield trials in Iowa, and District I appears to have a smaller error mean square than would usually be expected. Unless some explanation is available as to the reason for the difference in the two error variances, the experimenter may justifiably regard the pooled-error mean square, 11.45, as the appropriate one to use in making tests of significance. The analysis of data assuming unequal variances has been discussed to some extent in previous chapters [179; 273, p. 83; 279].

In making recommendations for the area for which a random sample of two fields is available the standard error of a difference between two hybrid totals is

$$s_{\bar{d}} = \sqrt{2(4)(2)(23.90)} = 19.56,$$

and for making recommendations for each place separately the standard error is

$$s_{\bar{d}} = \sqrt{2(4)(2)(11.45)} = 13.54.$$

X-1.5 MISSING DATA

In the event that a whole plot yield is missing, the appropriate formula for estimating the yield is determined by the design used for the whole plots. If a randomized complete block design is used, the appropriate formula is (V-5). If the whole plots are designed in a latin square, formula (VI-20) is the appropriate one. The missing whole plot is estimated, and the whole plot analysis is completed as prescribed by the particular whole plot design. The sub-plot analysis for a design of the type given in table X-1 is obtained by the method of fitting constants [273, Ch. 11; 318a].

The formula for estimating the yield of a missing sub-plot unit for the design in table X-1 has been developed by Anderson [3] and Khargonkar [181]. If the total yield of the whole plot with the missing sub-plot is designated as $X_{gi.}$, the total yield of the ijth treatment as $X_{.ij}$, and the total yield for the ith whole plot treatment as $X_{.i.}$, from an experiment of r replicates, p whole plot treatments, and q sub-plot treatments, the yield of a missing sub-plot may be estimated from the formula,

$$X_{gij}' = \frac{rX_{gi.} + qX_{.ij} - X_{.i.}}{(r-1)(q-1)}. \qquad \text{(X-8)}$$

The estimated value is inserted in the table of yields, and the analysis is completed in the same manner as for no missing yields, *except* that 1 degree of freedom is subtracted from the error (*b*) degrees of freedom for each value estimated. If two or more sub-plot units are missing, one may use the iterative procedure described in Chapters V and VI, or one may derive the exact formulae for estimating the particular combination of missing values obtained in the experiment.

Anderson [3] discusses the resulting biases in the mean squares; all mean squares except error (*b*) are slightly overestimated [3; 60; p. 229]. Also, these authors [3, 60] present standard errors for particular treatment contrasts.

If several missing observations occur in designs with three-way and higher

classifications, reference may be made to the works of Hazel [152], Henderson [156], Krishna Iyer [190], Quenouille [251], Stevens [281], Wilks [307], and Yates [318, 318a].

X-2 The Split Block Design

X-2.1 INTRODUCTION

In some cases the treatments a_i and the treatments b_j may be of relatively little interest compared to the $A \times B$ interaction. In other situations, it may be difficult or impossible to carry out the experiment in the manner illustrated in table X-1. Examples of the latter situation are fertilizer and tillage treatments with ordinary farm implements; spraying treatments on experiments in randomized complete block or latin square designs in the field; etc. To cope with situations such as those described above a design variously denoted as split blocks [193, 318], two-way whole plots, strip trials [181, 219], or subunits in strips [60] has been proposed. In this design the treatments a_i are laid out in a randomized complete block, latin square [297, 318], or other design, and then the treatments b_j are laid out across *all* of the a treatments in one replicate (table X-7). Such a design would effectively have the a treatments and the b treatments designed as whole plots; the only sub-plot information is on the $A \times B$ interaction.

X-2.2 ADVANTAGES AND DISADVANTAGES

The advantages of the design are

(i) The subunits may be kept relatively small, even though the whole plots for both factors must be relatively large experimental units.
(ii) Precise information is obtained on the $A \times B$ interaction.

The disadvantages of the design are:

(i) Information on the A and B effects is less than is available from the ordinary randomized complete block design.
(ii) The analysis of the design is more complex than the ordinary randomized complete block design. This is especially true for nonorthogonal situations and for covariance analyses.

X-2.3 RANDOMIZATION AND EXPERIMENTAL LAYOUTS

The randomization procedure for the a treatments follows that for the particular design in question. The randomization procedure for the b treatments follows that for the selected design for these treatments. Two examples of the experimental layout are given in table X-7.

X-2.4 ANALYSIS

Two field plans and the breakdown of the degrees of freedom in the analysis of variance for each design are given in tables X-7(a) and X-7(b). The first table illustrates the design and analysis for an experiment in which the main

TABLE X-7. Field designs and analyses of variance for a split block experiment with three levels of factor b, four levels of factor a, and three replicates

(a) Factors a and b in randomized complete block designs

Replicate I

	a_3	a_1	a_0	a_2
b_2				
b_0				
b_1				

Replicate II

	a_1	a_0	a_3	a_2
b_2				
b_1				
b_0				

Replicate III

	a_2	a_0	a_3	a_1
b_1				
b_2				
b_0				

Analysis of variance

Source of variation	df	ms
Replicate	2	–
A	3	–
Replicate x A = error (a)	6	E_a
B	2	–
Replicate x B = error (b)	4	E_b
AxB	6	–
Error (c)	12	E_c
Total	35	

(b) Factor a in a randomized complete block and factor b in a latin square design

Replicate I

	a_3	a_1	a_0	a_2
b_2				
b_0				
b_1				

Replicate II.

	a_1	a_0	a_3	a_2
b_1				
b_2				
b_0				

Replicate III

	a_2	a_0	a_3	a_1
b_0				
b_1				
b_2				

Analysis of variance

Source of variation	df	ms
Replicate	2	–
A	3	–
Replicate x A = error (a)	6	E_a
B	2	–
Order within replicates	2	–
Error (b)	2	E_b
AxB	6	–
Error (c)	12	E_c
Total	35	

effects are in randomized complete block designs with each of the levels of one factor running across all levels of the second factor. The comparisons among levels of both the a and b factors are made on three replicates and three replicates only. The standard error of a difference between two means on a plot basis for the a_i levels is

$$\sqrt{\frac{2E_a}{rq}} = \sqrt{\frac{2E_a}{9}}, \tag{X-9}$$

and for the b_j levels is $\sqrt{\frac{2E_b}{rp}}$ $\sqrt{\frac{2E_b}{12}}$, (X-10)

where r = number of replicates, q number of levels of factor b, and p = number of levels of factor a.

In the second design the order of the b treatments within a replicate is taken into account. This type of design may be used when there is a gradient from one replicate to another and when the number of replicates equals the number of treatments. Since there are only 2 degrees of freedom associated with the error sum of squares in a 3×3 latin square, it is inadvisable to use this design for three replicates. The standard errors of a mean difference are obtained by the same formulae given above except that E_a and E_b are obtained from table X-7(b).

The F test of the A, B, and $A \times B$ effects employs E_a, E_b, and E_c, respectively, as the experimental error mean squares. It is expected that the error (c) mean square, E_c, will generally be smaller than E_a or E_b, since it represents intrablock or intra-whole plot variation, while the other two represent interblock or inter-whole plot variation. Thus, the $A \times B$ interaction is estimated more precisely than are the main effects A or B. The increased accuracy on the $A \times B$ interaction is obtained by sacrificing accuracy on the whole plot comparisons. If the pq = 12 treatments of table X-7(a) had been completely randomized within each of the replicates, the A and B effects usually would be more accurately estimated and the $A \times B$ interaction less accurately than with the present design.

Despite the importance of some main effect comparisons, it may be impractical to design an experiment in any way other than in a split block design. In a pasture experiment, eight methods of cultivation and four cutting treatments (time and height of cutting) were studied to observe the effect of the treatments on forage yields. Regular farm machinery was used in the experiment, thereby necessitating the use of large plots. Four replicates of the thirty-two treatment combinations were used. If thirty-two plots had been included in each randomized complete block, the entire experiment would have been spread over a large area. Therefore it was decided to have smaller plots by using a split block design, the first replicate of which follows:

		Methods of cultivation							
		2	8	6	5	1	4	3	7
Cutting treatments	b								
	c								
	a								
	d								

The use of smaller plots allows the choice of a more homogeneous area for the complete block and requires that less material be weighed and handled. Another advantage of this type of design for field experiments is that the minimum amount of turning of farm machinery is required.

The disadvantage of the above design is the same as for all split block designs, i.e., the main effects A and B are estimated with the least precision and the $A \times B$ interaction with the most precision. In the above example, there are only four replicates on cutting treatments and on methods of cultivation.

Nair [219] and Cochran and Cox [60] present standard errors for contrasts other than those presented above.

X-2.5 MISSING DATA

A missing datum for a subunit in a split block design may be estimated in the manner described by Khargonkar [181]. A missing datum for an entire whole plot would be estimated by the method appropriate for the particular design used.

X-3 The Split Split Plot Design and Further Subdivisions

Several levels of three factors in all combinations (an $n \times q \times p$ factorial arrangement of the factors) may be of interest to an experimenter where factor a is of little interest, b is of somewhat more interest, and c and the interactions with the factor c are of most importance. In such a case the whole plots containing the factor a would be split for the b treatments. The split plots containing the b treatments would then be further split for the levels of factor c. The incomplete block containing the comparison of the levels of factor c and the levels of the interactions with c are made as homogeneous as possible, since these comparisons are of most importance.

The experimental layout and analysis are presented in table X-8 for a split split plot design. The $n = 4$ levels of the factor a are randomized within each complete block or replicate. Within each level of factor a the $q = 3$ levels of b are allotted to the split plots at random, and then within each level of b the $p = 2$ levels of the factor c are allotted to the split split plots at random.

Under the hypothesis that these are the only levels of the factors a, b, and c of interest, the experimental error for the A effect mean square is E_a, for the B and $A \times B$ effects is E_b, and for the C, $A \times C$, $B \times C$, and $A \times B \times C$ effects is E_c. In general, it is expected that $E_c < E_b < E_a$ and that the precision with which the split split plot comparisons are measured is greater than for the other comparisons. The B and $A \times B$ comparisons are measured with more precision than are the A comparisons, just as they are in an ordinary split plot design.

TABLE X-8. Field design and analysis of variance for a split split plot design of nqp = 4 × 3 × 2 = 24 combinations of the factors a, b, and c in three replicates

Replicate I

a_3	a_1	a_0	a_2
c_0 b_0 c_1	c_0 b_0 c_1	c_1 b_1 c_0	c_0 b_1 c_1
c_1 b_1 c_0	c_0 b_2 c_1	c_1 b_2 c_0	c_1 b_2 c_0
c_1 b_2 c_0	c_1 b_1 c_0	c_0 b_0 c_1	c_0 b_0 c_1

Replicate II

a_1	a_0	a_3	a_2
c_1 b_0 c_0	c_0 b_1 c_1	c_1 b_0 c_0	c_1 b_2 c_0
c_1 b_1 c_0	c_0 b_0 c_1	c_1 b_2 c_0	c_0 b_1 c_1
c_1 b_2 c_0	c_1 b_2 c_0	c_1 b_1 c_0	c_1 b_0 c_0

Replicate III

a_2	a_0	a_3	a_1
c_1 b_1 c_0	c_0 b_1 c_1	c_1 b_0 c_0	c_0 b_2 c_1
c_1 b_0 c_0	c_0 b_2 c_1	c_1 b_1 c_0	c_0 b_1 c_1
c_0 b_2 c_1	c_0 b_0 c_1	c_0 b_2 c_1	c_1 b_0 c_0

Analysis of variance

Source of variation	df	ms
Replicate	2	–
A	3	–
Rep. x A = Error (a)	6	E_a
B	2	–
AxB	6	–
Error (b)	16	E_b
C	1	–
AxC	3	–
BxC	2	–
AxBxC	6	–
Error (c)	24	E_c
Total	71	

The standard errors of a difference between two means on a split split plot basis are

for the comparison of two levels of factor *a*

$$\sqrt{\frac{2E_a}{qpr}}, \tag{X-11}$$

for the comparison of two levels of factor *b*

$$\sqrt{\frac{2E_b}{npr}}, \tag{X-12}$$

for the comparison of two levels of factor *b* at the same level of factor *a*

$$\sqrt{\frac{2E_b}{pr}}, \tag{X-13}$$

for the comparison of two levels of factor *a* at the same level of factor *b*

$$\sqrt{\frac{2[(q-1)E_b + E_a]}{rqp}}, \tag{X-14}$$

for the comparison of two levels of factor c

$$\sqrt{\frac{2E_c}{nqr}}, \tag{X-15}$$

for the comparison of two levels of c at the same level of a

$$\sqrt{\frac{2E_c}{rq}}, \tag{X-16}$$

for the comparison of two levels of a at the same level of c

$$\sqrt{\frac{2[(p-1)E_c + E_a]}{rpq}}, \tag{X-17}$$

for the comparison of two levels of c at the same level of b

$$\sqrt{\frac{2E_c}{rn}}, \tag{X-18}$$

and for the comparison of two levels of b at the same level of c

$$\sqrt{\frac{2[(p-1)E_c + E_b]}{rnp}}. \tag{X-19}$$

The number of degrees of freedom associated with the standard errors given in formulae (X-11), (X-12), (X-13), (X-15), (X-16), and (X-18) is the same as for the error mean square used. The remaining standard errors have an unknown number of degrees of freedom associated with them, but the number of degrees of freedom may be approximated [55, 287].

The split split plot design may be necessary because of the nature of the experimental material rather than because of the desire of the experimenter for more precision on some of the comparisons by sacrificing accuracy on others. If this is true, the experimenter may change the plot shape and incomplete block shapes in order to increase the accuracy on the other factors. For example, suppose that it is impossible to lay out the levels of a in any other than large plots. The experimenter can choose the shape of whole plots (usually long and narrow) and the shape of the replicate (usually square, in the absence of any knowledge of soil variation) so as to have the best comparisons possible on the factor a. Or, the factor b may be of considerable importance; in this case the whole plot should be as nearly square as possible, with rectangular split plots. This gives the best comparisons on the B and $A \times B$ effects. In the third instance, if the c factor and interactions are of most importance, then the split plot should be square or nearly so, with the split split plots being rectangular in shape. From the above considerations, then, the experimenter may change the precision on certain comparisons by changing plot shape, even though the experimental material requires a split split plot design.

Further examples of split split plot designs are afforded by experiments conducted at two or more locations over a period of years for either an annual or a perennial crop. Of course, the analyses for annual crops and for perennial crops are different, but the concepts are similar. Experiments harvested more than once during the year for two or more years [197] represent another example of this type of design [section X-5.3].

For some experiments, further subdivision may be desirable either because of the additional information on the factor or factors in the smallest unit or because of the ease or timeliness of application of the additional factors. Suppose that r replicates of n irrigation treatments (I) each with p varieties (V) as the sub-plot treatments are available. Suppose that it is decided to apply fertilizer treatments to the above design after the experiment has been laid out as a split plot design. Suppose further that the fertilizer treatments are composed of q levels of nitrogen (N) and s levels of potash (K) in all combinations, and that the $q \times s$ treatments are applied to the split

TABLE X-9. Key-out of degrees of freedom for a split split split plot design

Source of variation	df	ms	Source of variation	df	ms
Replicate	r-1	–	Fungicide (F)	t-1	–
Irrigation (I)	n-1	–	IxF	(n-1)(t-1)	–
Error (a)	(r-1)(n-1)	E_a	VxF	(p-1)(t-1)	–
Variety (V)	p-1	–	IxVxF	(n-1)(p-1)(t-1)	–
IxV	(n-1)(p-1)	–	NxF	(q-1)(t-1)	–
Error (b)	n(r-1)(p-1)	E_b	KxF	(s-1)(t-1)	–
Nitrogen (N)	q-1	–	NxKxF	(q-1)(s-1)(t-1)	–
Potash (K)	s-1	–	IxNxF	(n-1)(q-1)(t-1)	–
NxK	(q-1)(s-1)	–	IxKxF	(n-1)(s-1)(t-1)	–
IxN	(n-1)(q-1)	–	IxNxKxF	(n-1)(q-1)(s-1)(t-1)	–
IxK	(n-1)(s-1)	–	VxNxF	(p-1)(q-1)(t-1)	–
IxNxK	(n-1)(q-1)(s-1)	–	VxKxF	(p-1)(s-1)(t-1)	–
VxN	(p-1)(q-1)	–	VxNxKxF	(p-1)(q-1)(s-1)(t-1)	–
VxK	(p-1)(s-1)	–	IxVxNxF	(n-1)(p-1)(q-1)(t-1)	–
VxNxK	(p-1)(q-1)(s-1)	–	IxVxKxF	(n-1)(p-1)(s-1)(t-1)	–
IxVxN	(n-1)(p-1)(q-1)	–	IxVxNxKxF	(n-1)(p-1)(q-1)(s-1)(t-1)	–
IxVxK	(n-1)(p-1)(s-1)	–	Error (d)	npqs(t-1)(r-1)	E_d
IxVxNxK	(n-1)(p-1)(q-1)(s-1)	–			
Error (c)	np(qs-1)(r-1)	E_c	Total	rnpqst - 1	

split plots at random within each split plot. In addition, suppose that each split split plot is subdivided further to allow the application of t fungicide spraying treatments (F); i.e., each split split plot is subdivided into t units and the t fungicides are randomly allotted to the split split split plots within each split split plot. The key-out of the degrees of freedom for this design is given in table X-9. In this particular design and for the finite model, four error

terms, E_a, E_b, E_c, and E_d, are available for comparison with the other mean squares. E_a is used to test the variation among irrigation treatments; E_b is used to test the existence of varietal effects and of the $I \times V$ interaction; E_c is compared with the mean squares for nitrogen, for potash, for their interaction, and for the interaction of these fertilizers with varieties and irrigation treatments; and E_d may be compared with the mean squares for fungicides and for all interactions involving fungicides. For the infinite model, synthetic error variances must be constructed to test a number of the mean squares [55]. However, for treatments of the nature described here the finite model is usually more appropriate.

If one of the effects, say irrigation, is large compared to the other effects, it is possible that all interactions with this effect will be significant, and the significance of four- and five-factor interactions usually is difficult to explain. Since the components making up the various mean squares probably are heterogeneous, a combined analysis assuming homogeneity of errors may be undesirable. Because of this a separate analysis is computed for the plots from each irrigation treatment. Relatively large four- and five-factor interactions may merely indicate that it is incorrect to generalize over a wide range of certain treatments. This is especially true in experiments where one of the factors, such as water, exerts a dominating influence.

X-4 Some Variations

Some modifications of the split plot design are illustrated in problems X-5 and X-8. The number of replicates at the various locations is unequal for the experiments in both problems, but this feature does not complicate the analysis unduly. As long as the number of replicates for the treatments or varieties remains the same at each location, the feature of proportionality is not affected. Problems X-7 and X-9 present a more complicated variation of the split plot and the split block designs, respectively. In these cases the latin square arrangements are used for both the whole plot treatments and the split plot treatments. Hedayetullah *et al.* discuss split block designs with split plots [154]. When the split plot treatments represent a factorial arrangement, they may be put into incomplete blocks within the whole plot. Confounding some split plot comparisons with incomplete blocks may increase the accuracy on the whole plot comparisons. Finney [120], Kempthorne [171], and Singh [265] discuss other forms of confounding in split plot designs.

In some instances, variation in split plot designs is due to errors in laying out the experiment. The design illustrated in table X-10 was presented as a split plot design. Upon inquiry, it was found that the "split plot treatments" (methods of application) were laid out in strips across all whole plots (fungicides) and that the arrangement of methods of application was systematic in the three replicates. This design is a split block design with one set of whole

TABLE X-10. Example of a split block design with one of the whole plot treatments arranged systematically in the three replicates (Fungicides = $a_0, a_1, a_2, \ldots, a_8$; methods of application = b_0, b_1, b_2)

Replicate I

	a_2	a_5	a_8	a_4	a_7	a_1	a_0	a_6	a_3
b_0									
b_1									
b_2									

Replicate II

	a_1	a_7	a_4	a_8	a_0	a_3	a_2	a_5	a_6
b_0									
b_1									
b_2									

Replicate III

	a_4	a_0	a_2	a_5	a_8	a_3	a_1	a_7	a_6
b_0									
b_1									
b_2									

Analysis of variance

Source of variation		df	ms
Replicate		2	–
A = fungicide		8	–
Replicate x A = error (a)		16	E_a
B = method	confounded	6	–
Replicate x B			
AxB		16	–
AxB x replicate = error (b)		32	E_b
Total		80	

plots arranged systematically in all replicates. Owing to the error in laying out the experiment, the comparison of most interest, methods of applications = B effect, was lost entirely, as there is no suitable error for testing the B effect mean square. The A effect, fungicides = whole plot treatments, is tested with the same precision as it is in any split plot or split block design. The $A \times B$ interaction is tested with the same precision as it is in the ordinary split block design. Thus, the net result of this design is that no information is available on the B effect, the A effect is compared in three replicates in a randomized complete block design with an estimated experimental error equal to E_a, and the $A \times B$ effect is measured most accurately and is tested with the error (b) mean square = E_b.

The split plot design was used effectively by Gowe [138] in a poultry breeding experiment to study the length of fertility of sperm in the oviduct of two strains of chickens, a_0 and a_1. Two pens representing replicates were available. The whole plot treatments were eight cocks, four of the a_1 strain and four of the a_0 strain. The eight cocks were used in both pens, and each cock was mated to four dams, two of which were of the a_0 strain and the other two of the a_1 strain. This resulted in a total of thirty-two dams in each pen. The breakdown of the total degrees of freedom for the design is given in table X-11. The key-out of the total degrees of freedom is different from the

TABLE X-11. Breakdown of degrees of freedom for Gowe's poultry breeding experiment

Source of variation	df		ms
Whole Plot Analysis			
Pens	1		-
Cocks	7		
Between strains		1	-
Within a_0 strain		3	-
Within a_1 strain		3	-
Pens x cocks = error (a)	7		E_a
Split Plot Analysis			
Between strains for dams	1		-
Strains dams x strains cocks	1		-
Error (b)	14		E_b
Within Split Plots Analysis			
Between two a_0 dams on same male in same pen	16		-
Between two a_1 dams on same male in same pen	16		-
Total	63		

ordinary split plot because of the nature of the experimental material. The split plot treatments were not the same for all whole plot treatments. The 16 degrees of freedom in the split plot analysis are associated with the failure of the two samples of two hens of the same strain in the pens mated with the same cock to react the same with regard to length of fertility of the sperm in the oviduct. This results in 8 degrees of freedom for the samples of a_0 dams and 8 for a_1 dams. After removing the effect due to strains of the dams and the interaction of strains dams by strains cocks, there are 14 remaining degrees of freedom for the error (b) sum of squares, which is regarded as the experimental error sum of squares. The within-split plot sums of squares are obtained in the usual manner. The variances among dams and among cocks in the a_0 strain may be compared with the corresponding variances for the a_1 strain.

Another variation of the split plot design is introduced when the comparison among some split or split split plots represents dummy comparisons (i.e., comparisons among plots treated alike). P. G. Homeyer (lecture notes, 1946, Iowa State College) discussed an example involving two methods of

application (c_0 and c_1), three fertilizers (b_0, b_1, and b_2), and four rates of planting (a_0, a_1, a_2, and a_3) in three replicates. The field design was a conventional split split plot design with the rates of planting as the whole plots, the fertilizers (equal to none, 100 lbs., and 200 lbs.) as the split plots, and methods of application as the split split plot. Clearly, it is impossible to make two methods of application of no fertilizer! The comparison between the two split split plots treated alike in each replicate represents intra-split plot variation and should be included in the error (c) sum of squares. The breakdown of the total degrees of freedom for a design of this type is given in table X-12. In obtaining the $B \times C$ interaction sum of squares, only the levels of fertilizer b_1 and b_2 are used, resulting in an interaction sum of squares with 1 degree of freedom. If three levels of b are used, 2 degrees of freedom are available for interaction, but one of the 2 degrees of freedom represents a dummy comparison and is included in the error (c) sum of squares. Likewise, there are 3 degrees of freedom in the 6 $A \times B \times C$ interaction degrees of freedom which represent dummy comparisons. These 3 are included in error (c). The resulting error (c) sum of squares has 28 degrees of freedom instead of the twenty-four in the ordinary split split plot design. The $B \times C$ and $A \times B \times C$ interactions are evaluated on two-thirds of the plots rather than on all of the plots, as they are in the ordinary split split plot design.

TABLE X-12. Breakdown of degrees of freedom for the application-fertilizer-planting experiment

Source of variation	df	ms
Whole Plot Analysis		
Replicate	2	–
Rates of planting = A	3	–
Rates x rep. = error (a)	6	E_a
Split Plot Analysis		
Levels of fertilizers = B	2	–
AxB	6	–
Error (b)	16	E_b
Split Split Plot Analysis		
Methods of application = C	1	–
AxC	3	–
BxC	1	–
AxBxC	3	–
Error (c) 1+3+24 or 12+16 =	28	E_c
Total	71	

X-5 Expectation of Mean Squares

X-5.1 SPLIT PLOT DESIGN

The mathematical model for the split plot design composed of p whole plots in r replicates of a randomized complete block design with the q split plot treatments randomly allotted to the subunits of each whole plot is

$$X_{gij} = \mu + \rho_g + \alpha_i + \delta_{gi} + \beta_j + (\alpha\beta)_{ij} + \epsilon_{gij}, \qquad \text{(X-20)}$$

where μ = the over-all mean effect, ρ_g = the gth replicate effect, α_i = the effect of the ith level of factor a, δ_{gi} = a random component of error associated with the ith whole plot treatment in the gth replicate, β_j = the effect of the jth level of factor b, $(\alpha\beta)_{ij}$ = the interaction effect of the ith level of factor a with the jth level of factor b, and ϵ_{gij} = a random component of error associated with the gijth sub-plot in the gth replicate.

If the effects are assumed to be random independent variables, the coefficients of the various components of variance and of μ^2 are as given in the left half of table X-13. The experimenter may be interested only in the particular levels of a and b under experimentation. For this situation, coefficients in the right half of table X-13 are the appropriate ones for the various components of variance, of μ^2, and of the various sums of squares of effects. The expectation of the various mean squares in the split plot design are given in table X-14 for both models. These are obtained from the coefficients in table X-13 for the appropriate sums of squares. The method used here follows that described in Chapter VIII. For example, the expected value of the sum of squares for the whole plot treatment $\times$ sub-plot treatment interaction for the infinite model is

$$\begin{aligned}
&E[\textstyle\sum\sum X_{.ij}^2/r - \sum X_{.i.}^2/rq - \sum X_{..j}^2/rp + X_{...}^2/rpq] \\
&= \{rpq(\mu^2 + \sigma_\alpha^2 + \sigma_\beta^2 + \sigma_{\alpha\beta}^2) + pq(\sigma_\rho^2 + \sigma_\delta^2 + \sigma_\epsilon^2)\} - \{rpq(\mu^2 + \sigma_\alpha^2) \\
&+ pq(\sigma_\rho^2 + \sigma_\delta^2) + rp(\sigma_\beta^2 + \sigma_{\alpha\beta}^2) + p\sigma_\epsilon^2\} - \{rpq(\mu^2 + \sigma_\beta^2) + pq\sigma_\rho^2 \\
&+ rq(\sigma_\alpha^2 + \sigma_{\alpha\beta}^2) + q(\sigma_\delta^2 + \sigma_\epsilon^2)\} + \{rpq\mu^2 + pq\sigma_\rho^2 + rq\sigma_\alpha^2 + q\sigma_\delta^2 + rp\sigma_\beta^2 \\
&+ r\sigma_{\alpha\beta}^2 + \sigma_\epsilon^2\} = (p-1)(q-1)(\sigma_\epsilon^2 + r\sigma_{\alpha\beta}^2),
\end{aligned} \tag{X-21}$$

with $(p - 1)(q - 1)$ degrees of freedom.

In the finite case the $\sum\alpha_i = \sum\beta_j = \sum_i(\alpha\beta)_{ij} = \sum_j(\alpha\beta)_{ij} = 0$. The expected value of the interaction sum of squares in equation (X-21) now becomes

$$\begin{aligned}
&\{rpq\mu^2 + pq(\sigma_\rho^2 + \sigma_\delta^2 + \sigma_\epsilon^2) + rq\textstyle\sum\alpha_i^2 + rp\sum\beta_j^2 + r\sum\sum(\alpha\beta)_{ij}^2\} - \{rpq\mu^2 \\
&+ pq\sigma_\rho^2 + rq\textstyle\sum\alpha_i^2 + pq\sigma_\delta^2 + p\sigma_\epsilon^2\} - \{rpq\mu^2 + pq\sigma_\rho^2 + q\sigma_\delta^2 + pr\sum\beta_j^2 \\
&+ q\sigma_\epsilon^2\} + \{rpq\mu^2 + pq\sigma_\rho^2 + q\sigma_\delta^2 + \sigma_\epsilon^2\} \\
&= (p-1)(q-1)\sigma_\epsilon^2 + r\textstyle\sum\sum(\alpha\beta)_{ij}^2,
\end{aligned} \tag{X-22}$$

with $(p - 1)(q - 1)$ degrees of freedom.

X-5.2 RANDOMIZED COMPLETE BLOCK EXPERIMENT REPEATED AT SEVERAL LOCATIONS

For a randomized complete block experiment repeated at several places, the yield of a single observation may be expressed as

$$X_{gij} = \mu + \lambda_g + \delta_{gi} + \alpha_j + (\alpha\lambda)_{gj} + \epsilon_{gij}, \tag{X-23}$$

where μ = mean effect, λ_g = effect of gth place or location, δ_{gi} = a random effect associated with the ith replicate at the gth location, α_j = treatment effect, $(\alpha\lambda)_{gj}$ = effect of jth treatment at the gth location, and ϵ_{gij} = a random component associated with the jth treatment in the ith replicate in the

TABLE X-13. Coefficients of μ^2, of the components of variance, and of the sums of squares of effects for the split plot design

	Infinite model							Finite model						
	μ^2	σ_ρ^2	σ_α^2	σ_δ^2	σ_β^2	$\sigma_{\alpha\beta}^2$	σ_ϵ^2	μ^2	σ_ρ^2	$\Sigma\alpha_i^2$	σ_δ^2	$\Sigma\beta_j^2$	$\Sigma\Sigma(\alpha\beta)_{ij}^2$	σ_ϵ^2
$\Sigma\Sigma\Sigma X^2_{gij}$	pqr	pqr	pqr	pqr	pqr	pqr	pqr	pqr	pqr	qr	pqr	pr	r	pqr
$\Sigma X^2_{g..}/pq$	"	"	qr	qr	pr	r	r	"	"	0	qr	0	0	r
$\Sigma X^2_{.i.}/rq$	"	pq	pqr	pq	pr	pr	p	"	pq	qr	pq	0	0	p
$\Sigma\Sigma X^2_{gi.}/q$	"	pqr	pqr	pqr	pr	pr	pr	"	pqr	qr	pqr	0	0	pr
$X^2_{...}/pqr$	"	pq	qr	q	rp	r	1	"	pq	0	q	0	0	1
$\Sigma X^2_{..j}/rp$	"	"	"	"	pqr	rq	q	"	"	0	q	pr	0	q
$\Sigma\Sigma X^2_{.ij}/r$	"	"	pqr	pq	pqr	pqr	pq	"	"	qr	pq	pr	r	pq

TABLE X-14. Expected values of mean squares in the split plot design

Source of variation	df	Expected value of mean square	
		Infinite model	Finite model
Replicate	r - 1	$\sigma_\epsilon^2 + q\sigma_\delta^2 + pq\sigma_\rho^2$	$\sigma_\epsilon^2 + q\sigma_\delta^2 + pq\sigma_\rho^2$
Whole plot treatment (A)	p - 1	$\sigma_\epsilon^2 + q\sigma_\delta^2 + r\sigma_{\alpha\beta}^2 + rq\sigma_\alpha^2$	$\sigma_\epsilon^2 + q\sigma_\delta^2 + \frac{qr}{p-1}\Sigma\alpha_i^2$
Error (a)	(r - 1)(p - 1)	$\sigma_\epsilon^2 + q\sigma_\delta^2$	$\sigma_\epsilon^2 + q\sigma_\delta^2$
Split plot treatment (B)	q - 1	$\sigma_\epsilon^2 + r\sigma_{\alpha\beta}^2 + rp\sigma_\beta^2$	$\sigma_\epsilon^2 + \frac{pr}{q-1}\Sigma\beta_j^2$
A x B	(p - 1)(q - 1)	$\sigma_\epsilon^2 + r\sigma_{\alpha\beta}^2$	$\sigma_\epsilon^2 + \frac{r}{(p-1)(q-1)}\Sigma\Sigma(\alpha\beta)_{ij}^2$
Error (b)	p(q - 1)(r - 1)	σ_ϵ^2	σ_ϵ^2
Total	pqr - 1	-	-

gth location. Assuming that the effects are random and independent variables, the coefficients for the various components of variance are as given in the left-hand portion of table X-15. If it is assumed that these locations are the only ones of interest, then $\sum_g \lambda_g = \sum_g (\alpha\lambda)_{gj} = 0$ and the appropriate coefficients are those in the right half of table X-15. The expected values for the various mean squares are given in table X-16 for both models.

The appropriate error mean square for treatments is different for the two models. For the infinite model the interaction mean square is the appropriate error for testing treatments. (This is also the correct error mean square if the treatments are the only ones of interest.) The error (b) mean square is the appropriate one for comparison with the treatment mean square *for these particular locations*; i.e., treatments are to be recommended for each location and not for the entire area.

The above analysis and models are appropriate for other types of experiments. For example, equation (X-23) is the appropriate linear model for annual experiments repeated through time. Also, equation (X-23) and the finite model are appropriate for artificial insemination and progeny evaluation experiments, where p sires are available and these sires are the only ones of interest; the dams are the treatments and the progeny records are the subunit observations.

X-5.3 EXPECTATION OF MEAN SQUARES FROM AN EXPERIMENT ON PERENNIAL CROPS [197]

The layout, yields, X_{ghij}, and linear model for a forage-crop experiment follow:

	Hay cut			Aftermath			
	Replicates						
Year 1 Varieties	1	$2 \cdots r$		1	$2 \cdots r$		
1	X_{1111}	$X_{1121} \ldots X_{11r1}$	$X_{11\cdot 1}$	X_{1211}	$X_{1221} \ldots X_{12r1}$	$X_{12\cdot 1}$	$X_{1\cdot\cdot 1}$
2	X_{1112}	$X_{1122} \ldots X_{11r2}$	$X_{11\cdot 2}$	X_{1212}	$X_{1222} \ldots X_{12r2}$	$X_{12\cdot 2}$	$X_{1\cdot\cdot 2}$
.							
.							
.							
v	X_{111v}	$X_{112v} \ldots X_{11rv}$	$X_{11\cdot v}$	X_{121v}	$X_{122v} \ldots X_{12rv}$	$X_{12\cdot v}$	$X_{1\cdot\cdot v}$
	$X_{111\cdot}$	$X_{112\cdot} \ldots X_{11r\cdot}$	$X_{11\cdot\cdot}$	$X_{121\cdot}$	$X_{122\cdot} \ldots X_{12r\cdot}$	$X_{12\cdot\cdot}$	$X_{1\cdot\cdot\cdot}$

Year 2,

.

.

.

Year y.

and

$$
\begin{aligned}
X_{ghij} = {} & \mu + \alpha_g + \gamma_h + (\alpha\gamma)_{gh} + \rho_i + (\alpha\rho)_{gi} + (\gamma\rho)_{hi} + (\alpha\gamma\rho)_{ghi} + \tau_j + (\rho\tau)_{ij} \\
& + (\alpha\tau)_{gj} + (\gamma\tau)_{hj} + (\alpha\gamma\tau)_{ghj} + (\gamma\rho\tau)_{hij} + (\alpha\rho\tau)_{gij} + \epsilon_{ghij}, \qquad \text{(X-24)}
\end{aligned}
$$

TABLE X-15. Coefficients of μ^2, of the components of variance, and of the sums of squares of effects for a randomized complete block design repeated at several places

	Infinite model						Finite model for number of locations					
	μ^2	σ_λ^2	σ_δ^2	σ_α^2	$\sigma_{\alpha\lambda}^2$	σ_ϵ^2	μ	$\Sigma\lambda_g^2$	σ_δ^2	σ_α^2	$\sigma_{\alpha\lambda}^2$	σ_ϵ^2
$\Sigma\Sigma\Sigma X^2_{gij}$	pqr	pqr	pqr	pqr	pqr	pqr	pqr	qr	pqr	pqr	pqr	pqr
$\Sigma X^2_{g..}/pq$	"	"	pq	pr	pr	p	"	"	pq	pr	pr	p
$\Sigma\Sigma X^2_{gi.}/q$	"	"	pqr	pr	pr	pr	"	"	pqr	"	"	pr
$X^2_{...}/pqr$	"	qr	q	pr	r	1	"	0	q	pr	0	1
$\Sigma X^2_{..j}/rp$	"	"	"	pqr	rq	q	"	0	"	pqr	0	q
$\Sigma\Sigma X^2_{g.j}/r$	"	pqr	pq	pqr	pqr	pq	"	qr	pq	pqr	pqr	pq

TABLE X-16. Expected values of mean squares for the randomized complete block design repeated at several places

Source of variation	df	Expected value of mean square	
		Infinite model	Finite model for number of locations
Location	p - 1	$\sigma_\epsilon^2 + q\sigma_\delta^2 + r\sigma_{\alpha\lambda}^2 + qr\sigma_\lambda^2$	$\sigma_\epsilon^2 + q\sigma_\delta^2 + \frac{rp}{p-1}\sigma_{\alpha\lambda}^2 + \frac{rq}{p-1}\Sigma\lambda_g^2$
Replicates within a location = error (a)	p(r - 1)	$\sigma_\epsilon^2 + q\sigma_\delta^2$	$\sigma_\epsilon^2 + q\sigma_\delta^2$
Treatment	q - 1	$\sigma_\epsilon^2 + r\sigma_{\alpha\lambda}^2 + rp\sigma_\alpha^2$	$\sigma_\epsilon^2 + rp\sigma_\alpha^2$
Treatment x location	(p - 1)(q - 1)	$\sigma_\epsilon^2 + r\sigma_{\alpha\lambda}^2$	$\sigma_\epsilon^2 + \frac{rp}{p-1}\sigma_{\alpha\lambda}^2$
Error (b)	p(r - 1)(q - 1)	σ_ϵ^2	σ_ϵ^2
Total	pqr - 1	-	-

where μ = mean effect,
α_g = effect due to gth year,
γ_h = effect of hth cut,
$(\alpha\gamma)_{gh}$ = effect peculiar to hth cut in gth year,
ρ_i = effect due to ith replicate,
$(\alpha\rho)_{gi}$ = effect due to ith replicate in gth year,
$(\gamma\rho)_{hi}$ = effect due to hth cut in ith replicate,
$(\alpha\gamma\rho)_{ghi}$ = effect due to hth cut in ith replicate in the gth year,
τ_j = effect due to jth variety,
$(\rho\tau)_{ij}$ = effect due to jth variety in ith replicate,
$(\alpha\tau)_{gj}$ = effect due to jth variety in gth year,
$(\gamma\tau)_{hj}$ = effect due to jth variety in hth cut,
$(\alpha\gamma\tau)_{ghj}$ = effect due to jth variety in hth cut in the gth year,
$(\gamma\rho\tau)_{hij}$ = effect of hth cut on jth variety in the ith replicate,
$(\alpha\rho\tau)_{gij}$ = effect of jth variety in the ith replicate in the gth year,
ϵ_{ghij} = effect common to jth variety in hth cut in the ith replicate in the gth year.

In the above scheme, only two cuttings, hay and aftermath, are considered. The following formulae are developed for c cuttings per year rather than for the two in the example. The mean effect μ is considered to be a constant. The number of cuts in these experiments constitute the whole of the population; i.e., there are only c types of individuals in the population of cuttings. Therefore, $E\mu = \mu$, $\sum_h \gamma_h = 0 = \sum_h (\alpha\gamma)_{gh} = \sum_h (\gamma\rho)_{hi} = \sum_h (\gamma\tau)_{hj}$, $E\gamma_h^2 = \gamma_h^2$, $E(\alpha\gamma)_{gh}^2 = \sigma_{\alpha\gamma}^2$ $E(\gamma\rho)_{hi}^2 = \sigma_{\gamma\rho}^2$, and $E(\gamma\tau)_{hj}^2 = \sigma_{\gamma\tau}^2$. Also, the following summations equal zero:

$$\sum_{h=1}^{c}(\alpha\gamma\rho)_{ghi} = \sum_h(\alpha\gamma\tau)_{ghj} = \sum_h(\gamma\rho\tau)_{hij} = 0. \tag{X-25}$$

The following expectations are assumed:

$$E(\alpha\gamma\rho)_{ghi}^2 = \sigma_{\alpha\gamma\rho}^2,\ E(\alpha\gamma\tau)_{ghj}^2 = \sigma_{\alpha\gamma\tau}^2, \text{ and } E(\gamma\rho\tau)_{hij}^2 = \sigma_{\gamma\rho\tau}^2. \tag{X-26}$$

The remaining effects are considered to be random independent variables from an infinitely large population with zero means and variances peculiar to each effect. The average value or the summed values of all cross products equals zero.

Since the sample of years represents consecutive years, it might be argued that they do not represent independent variates. Since it is doubtful if the dependence is of any appreciable size and since an experiment on perennial forage crops will never be conducted any other way, the sample of years is assumed to be a random sample of years. Cuts, on the other hand, were not regarded as independent. Regardless of the desires of the experimenter, there will only be hay yields and aftermath yields on some types of forage-crop

TABLE X-17. Coefficients of components for expected value of the various sums of squares

Sum of squares	Components															
	μ^2	σ_α^2	$\Sigma\gamma_h^2$	$\sigma_{\alpha\gamma}^2$	σ_ρ^2	$\sigma_{\alpha\rho}^2$	$\sigma_{\gamma\rho}^2$	$\sigma_{\alpha\gamma\rho}^2$	σ_τ^2	$\sigma_{\alpha\tau}^2$	$\sigma_{\gamma\tau}^2$	$\sigma_{\rho\tau}^2$	$\sigma_{\alpha\gamma\tau}^2$	$\sigma_{\alpha\rho\tau}^2$	$\sigma_{\gamma\rho\tau}^2$	σ_ϵ^2
$\Sigma\Sigma\Sigma\Sigma X^2_{ghij}$	crvy	crvy	rvy	crvy	crvy	crvy	crvy	crvy	crvy	crvy	crvy	crvy	crvy	crvy	crvy	crvy
$X^2_{....}/crvy$	crvy	crv	0	0	cvy	cv	0	0	cry	cr	0	cy	0	c	0	1
$\Sigma X^2_{g...}/crv$	crvy	crvy	0	0	cvy	cvy	0	0	cry	cry	0	cy	0	cy	0	y
$\Sigma X^2_{.h..}/rvy$	crvy	crv	rvy	crv	cvy	cv	cvy	cv	cry	cr	cry	cy	cr	c	cy	c
$\Sigma\Sigma X^2_{gh..}/rv$	crvy	crvy	rvy	crvy	cvy	cvy	cvy	cvy	cry	cry	cry	cy	cry	cy	cy	cy
$\Sigma X^2_{..i.}/cvy$	crvy	crv	0	0	crvy	crv	0	0	cry	cr	0	cry	0	cr	0	r
$\Sigma\Sigma X^2_{g.i.}/cv$	crvy	crvy	0	0	crvy	crvy	0	0	cry	cry	0	cry	0	cry	0	ry
$\Sigma\Sigma X^2_{.hi.}/vy$	crvy	crv	rvy	crv	crvy	crv	crvy	crv	cry	cr	cry	cry	cr	cr	cry	cr
$\Sigma\Sigma\Sigma X^2_{ghi.}/v$	crvy	crvy	rvy	crvy	crvy	crvy	crvy	crvy	cry	cry	cry	cry	cry	cry	cry	cry
$\Sigma X^2_{...j}/cry$	crvy	crv	0	0	cvy	cv	0	0	crvy	crv	0	cvy	0	cv	0	v
$\Sigma\Sigma X^2_{g..j}/cr$	crvy	crvy	0	0	cvy	cvy	0	0	crvy	crvy	0	cvy	0	cvy	0	vy
$\Sigma\Sigma X^2_{.h.j}/ry$	crvy	crv	rvy	crv	cvy	cv	cvy	cv	crvy	crv	crvy	cvy	crv	cv	cvy	cv
$\Sigma\Sigma X^2_{..ij}/cy$	crvy	crv	0	0	crvy	crv	0	0	crvy	crv	0	crvy	0	crv	0	rv
$\Sigma\Sigma\Sigma X^2_{gh.j}/r$	crvy	crvy	rvy	crvy	cvy	cvy	cvy	cvy	crvy	crvy	crvy	cvy	crvy	cvy	cvy	cvy
$\Sigma\Sigma\Sigma X^2_{g.ij}/c$	crvy	crvy	0	0	crvy	crvy	0	0	crvy	crvy	0	crvy	0	crvy	0	rvy
$\Sigma\Sigma\Sigma X^2_{.hij}/y$	crvy	crv	rvy	crv	crvy	crv	crvy	crv	crvy	crv	crvy	crvy	crv	crv	crvy	crv

TABLE X-18. Expectations of mean squares in the analysis of variance for the experiment on perennial crops[a]

Source of variation	df	σ_ϵ^2	$\sigma_{\alpha\rho\tau}^2$	$\sigma_{\gamma\rho\tau}^2$	$\sigma_{\alpha\gamma\tau}^2$	$\sigma_{\gamma\tau}^2$	$\sigma_{\alpha\tau}^2$	$\sigma_{\rho\tau}^2$	σ_τ^2	$\sigma_{\alpha\gamma\rho}^2$	$\sigma_{\gamma\rho}^2$	$\sigma_{\alpha\rho}^2$	σ_ρ^2	$\sigma_{\alpha\gamma}^2$	$\Sigma\gamma_h^2$	σ_α^2
Year	y-1	1	c				cr					cv				crv
Cut	c-1	1		$\frac{cy}{c-1}$	$\frac{cr}{c-1}$	$\frac{cry}{c-1}$				$\frac{cv}{c-1}$	$\frac{cvy}{c-1}$			$\frac{crv}{c-1}$	$\frac{rvy}{c-1}$	
Cut x year	(y-1)(c-1)	1			$\frac{cr}{c-1}$					$\frac{cv}{c-1}$				$\frac{crv}{c-1}$		
Replicate	r-1	1	c					cy				cv	cvy			
Replicate x year	(r-1)(y-1)	1	c									cv				
Replicate x cut	(r-1)(c-1)	1		$\frac{cy}{c-1}$						$\frac{cv}{c-1}$	$\frac{cvy}{c-1}$					
Rep. x cut x year	(r-1)(c-1)(y-1)	1								$\frac{cv}{c-1}$						
Variety	v-1	1	c				cr	cy	cry							
Variety x replicate	(v-1)(r-1)	1	c					cy								
Variety x year	(v-1)(y-1)	1	c				cr									
Variety x cut	(v-1)(c-1)	1		$\frac{cy}{c-1}$	$\frac{cr}{c-1}$	$\frac{cry}{c-1}$										
Variety x year x cut	(v-1)(c-1)(y-1)	1			$\frac{cr}{c-1}$											
Variety x cut x rep.	(v-1)(c-1)(r-1)	1		$\frac{cy}{c-1}$												
Variety x year x rep.	(v-1)(y-1)(r-1)	1	c													
Residual	(v-1)(y-1)(c-1)(r-1)	1														

[a]The coefficients of the various components of variance entering into the expectation of a mean square are given in the table. A blank indicates that the particular component of variance has a zero coefficient.

experiments. Therefore, it would be idle to speculate about the relative efficiency of increasing the number of cuttings per year. Two cuts will be available each year. It is possible on perennials, but not on biennials, to increase the number of years on which yields are taken. However, it is doubtful if yields will be obtained for more than four to six years at most.

The coefficients of the various components are summarized in table X-17. The expectations of the mean squares are presented in table X-18. Since cuts represent the whole of the population, the various effects do not affect the variance of a treatment, year, or replicate (or any combination of these) mean; i.e., there is no sampling error due to obtaining only a sample of cuts rather than all of them. Also, as a result of having a finite population of cuts, the coefficient $c/(c - 1)$ appears in the expectations of the mean squares involving cuts.

CHAPTER XI

Incomplete Block Designs: General Considerations and the One-Restrictional Lattices with Treatments in Complete Replicates

XI-1 Introduction

The designs discussed in Chapters IV to VI may be unsuitable for experiments in which a large number of varieties or treatments is used, and those given in Chapters IX and X may be unsuitable for experiments in which the treatments are not of the factorial type. In response to the need for efficient designs for a large number of treatments, Yates [320-8] developed the group of incomplete block designs known as quasi-factorials or lattices. Following Yates' first discussions on the construction of lattices, Bose and Nair [30] and Nair and Rao [226] presented a detailed account of the construction of several incomplete block designs. Numerous other papers [e.g., 48, 60, 71, 74, 103, 105, 114, 129, 136, 148-50, 175, 177, 178, 222, 227, 255, 259, 266, 305; see 113 for other references] have been written on the construction of lattices; the general methods for constructing b incomplete blocks of size k for v treatments in r replicates are: (i) for v of the form p^n or $k \times q^m \times p^n$ the method of confounding in factorial experiments [103, 105, 114, 175, 177, 178, 228, 320, 322-4, 326, 327]; (ii) the use of geometrical configurations [30, 71, 129, 226]; (iii) the use of orthogonal latin squares [74, 222, 305, 322]; and (iv) exhaustive enumeration.

Lattice designs may be subdivided into the three categories described in Chapter IX; i.e., balanced confounding, partially balanced confounding, and unbalanced confounding of the contrasts. In constructing lattice designs of the balanced type, two fundamental relations are involved:

$$\left.\begin{aligned} vr &= kb \\ \text{and} \qquad \lambda(v-1) &= r(k-1), \end{aligned}\right\} \qquad \text{(XI-1)}$$

where $b \geq v$ and λ = the number of times (an integer) a treatment occurs with each of the other treatments within an incomplete block. If λ is equal for all pairs of varieties (treatments), the design is balanced. A large number

of balanced designs has been worked out and tabulated [26, 30, 32, 60, 71, 129, 320] for $r \leq 10$ and for k of various sizes (table XI-1). Since it is not always possible to arrange the incomplete blocks so that all varieties fall within one complete block or replicate, table XI-2 has been prepared to illustrate the range of values of v and k for which the varieties may be arranged in complete blocks. It should be noted that there are other values of v and k not listed in table XI-2 for which complete block arrangements are possible. Also, it would be interesting to know what arrangements are possible for treatments appearing two or three times in one complete block. These arrangements have not been completely investigated to date [60, 327]; such designs would be useful in testing for differential effects and for determining the appropriate scale of measurement. Since the number of replicates required for balance may be large, Yates [320, 322, 324, 327], Bose and Nair [30], and others developed the partially balanced incomplete block designs. This class of designs adds considerably to the range of values of v and k and allows for relatively small values of r (table XI-2). Partially balanced incomplete block (p.b.i.b.) designs are available or may be constructed for most values of v and k, although the analysis may be difficult in some cases.

As first proposed the analysis for the quasi-factorials or lattices recovered only intrablock information; the variance among the incomplete blocks was ignored (see Chapter IX). To make use of the additional information available in the variance among incomplete blocks, Yates [326, 327] and others [48, 136, 177, 178, 220, 255] proposed an analysis recovering interblock information. With the recovery of interblock information the efficiency of the lattice design (with treatments in complete replicates) relative to the randomized complete block design can be only slightly less than 100 per cent, whereas the efficiency can be much less than 100 per cent when interblock information is ignored. The same is true for lattice designs with treatments not in complete replicates relative to the completely randomized design. Hence, the recovery of interblock information as proposed by Yates greatly enhances the value of incomplete block designs relative to complete block designs, since the average efficiency of the former designs to the latter will always be greater than 100 per cent with any meaningful stratification or grouping of the experimental material into homogeneous subgroups.

XI-1.1 ADVANTAGES AND DISADVANTAGES

The chief advantage of lattice designs is that a large number of treatments may be compared within relatively small blocks, the incomplete blocks. Another advantage of lattice designs lies in the fact that they may be analyzed as a randomized complete block design or as a completely randomized design, depending upon whether or not the incomplete blocks are arranged in complete blocks [326]; this procedure introduces a small bias in some designs and in some methods of analysis [175, sec. 23.7]. For practical purposes the amount of information from lattices is never less than for the comparable complete

TABLE XI-1. Balanced incomplete block arrangement with 10 or less replicates[a]

SOLVED

v	b	r	k	v	b	r	k
4	6	3	2	13	26	6	3
4	4	3	3	13	13	4	4
5	10	4	2	13	13	9	9
5	10	6	3	15	35	7	3
5	5	4	4	15	15	7	7
6	15	5	2	15	15	8	8
6	10	5	3	16	20	5	4
6	15	10	4	16	24	9	6
6	6	5	5	16	16	6	6
7	21	6	2	16	16	10	10
7	7	3	3	19	57	9	3
7	7	4	4	19	19	9	9
7	7	6	6	19	19	10	10
8	28	7	2	21	70	10	3
8	14	7	4	21	21	5	5
8	8	7	7	21	30	10	7
9	36	8	2	25	50	8	4
9	12	4	3	25	30	6	5
9	18	8	4	25	25	9	9
9	18	10	5	28	63	9	4
9	12	8	6	28	56	9	7
9	9	8	8	31	31	6	6
10	45	9	2	31	31	10	10
10	30	9	3	37	37	9	9
10	15	6	4	41	82	10	5
10	18	9	5	49	56	8	7
10	15	9	6	57	57	8	8
10	10	9	9	64	72	9	8
11	55	10	2	73	73	9	9
11	11	5	5	81	90	10	9
11	11	6	6	91	91	10	10
11	11	10	10				

UNSOLVED				NO SOLUTION			
36	45	10	8	15	21	7	5
46	46	10	10	21	28	8	6
46	69	9	6	22	22	7	7
51	85	10	6	29	29	8	8

[a]Shrikhande (*Ann. Math. Stat.* **21**:106) has shown that there is no solution for v = b = 46 and r = k = 10; Connor (*Ann. Math. Stat.* **23**:57) has shown that no solution exists for v = 36, b = 45, r = 10, and k = 8.

TABLE XI-2. Incomplete blocks in a randomized complete block design

	No. of treatments v	No. of units per block k	No. of replicates for balance[a] r	No. of replicates others[b] r		No. of treatments v	No. of units per block k	No. of replicates for balance[a] r	No. of replicates others[b] r
	4	2	3	2		243	27		2, 3, ...
	8	2	7	3, ..., 6		243	81	121	2, ..., 121
	8	4	7	2, ..., 6		256	2	255	8, ..., 254
	9	3	4	2, 3		256	4	85	4, ..., 84
c	12	3	-	2, 3		256	8		3, ...
	16	2	15	4, ..., 14		256	16	17	2, ..., 16
	16	4	5	2, 3, 4		256	32		2, ...
	16	8	15	2, ..., 14		256	64		2, ...
c	20	4	-	2, 3		256	128	255	2, ...
	25	5	6	2, ..., 5	c	272	16	-	2, 3
	27	3	13	3, ..., 12		289	17	18	2, ..., 17
	27	9	13	2, ..., 12	c	306	17	-	2, 3
	28	4	9	2, ..., 8		324	18		2, 3
c	30	5	-	2, 3	c	342	18	-	2, 3
	32	2	31	5, ..., 30		361	19	20	2, ..., 19
	32	4	31	3, ...	c	380	19	-	2, 3
	32	8	31	2, ...		400	20	-	2, 3
	32	16	31	2, ..., 30	c	420	20	-	2, 3
	36	6	-	2, 3		441	21	-	2, 3
c	42	6	-	2, 3	c	462	21	-	2, 3
	49	7	8	2, ..., 7		484	22	-	2, 3
c	56	7	-	2, 3	c	506	22	-	2, 3
	64	2	63	6, ..., 62		512	2	511	9, ..., 510
	64	4	21	3, ..., 21		512	4		5, ...
	64	8	9	2, ..., 8		512	8	73	3, ..., 72
	64	16		2, ...		512	16		3, ...
	64	32	63	2, ..., 62		512	32		2, ...
	72	8	-	2, 3		512	64		2, ...
	81	3	40	4, ..., 39		512	128		2, ...
	81	9	10	2, ..., 9		512	256	511	2, ...
	81	27	40	2, ..., 39		529	23	24	2, ..., 23
c	90	9	-	2, 3	c	552	23	-	2, 3
	100	10	-	2, 3		576	24	-	2, 3
c	110	10	-	2, 3	c	600	24	-	2, 3
	121	11	12	2, ..., 11		625	5	156	4, ..., 155
	125	5	31	3, ..., 30		625	25	26	2, ..., 25
	125	25	31	2, ..., 30		625	125	156	2, ..., 155
	128	2	127	7, ..., 126	c	650	25	-	2, 3
	128	4		4, ...		676	26	-	2, 3
	128	8		3, ...	c	702	26	-	2, 3
	128	16		2, ...		729	3	364	6, ..., 363
	128	32		2, ...		729	9	91	3, ..., 90
	128	64	127	2, ...		729	27	28	2, ..., 27
c	132	11	-	2, 3		729	81		2, ...
	144	12	-	2, 3, 4		729	243	364	2, ..., 363
c	156	12	-	2, 3	c	756	27	-	2, 3
	169	13	14	2, ..., 13		784	28	-	2, 3
c	182	13	-	2, 3	c	812	28	-	2, 3
	196	14	-	2, 3		841	29	30	2, ..., 29
c	210	14	-	2, 3	c	870	29	-	2, 3
	216	6	-	3		900	30	-	2, 3
	225	15	-	2, 3	c	930	30	-	2, 3
c	240	15	-	2, 3		961	31	32	2, ..., 31
	243	3	121	5, ..., 120	c	992	31	-	2, 3
	243	9	121	3, ..., 120		1000	10	-	3

[a]Blank signifies unknown; dash (-) means that balance is impossible.
[b]There may be more replicates available for some of the partially balanced designs.
[c]Rectangular lattices for $k(k + 1)$ treatments in blocks of k.

block design; with meaningful stratification considerably more information may be obtained.

The disadvantages of lattice designs are

(i) They are more complex computationally; this is especially true when missing plots occur [60, 65, 68, 153, 255, 259] or when a covariance analysis is used [48, 74, 259].
(ii) Lattice designs are not available for all values of v, r, and k.
(iii) The analysis becomes complex if the treatments are subject to different error variances [175, sec. 23.7].
(iv) The designs are more difficult to construct.

The use of prepared arrangements [42, 112] and the use of punched card equipment to analyze the results from lattice experiments [160] greatly simplify the layout and the analysis of lattice experiments. Missing data or unequal error variances considerably complicate the analysis; if either situation is likely to occur, it is suggested that the experimenter improve the experimental technique and (or) use a randomized complete block design. Also, the selection of more homogeneous material within a given experiment or of a suitable transformation may eliminate the problem of unequal variances. If the differences in the treatment yields are large, it may be inadvisable to use lattice designs, since the variances may be related to the means and would, therefore, be unequal. In such cases the widely divergent groups should be included in separate experiments or in a split plot design with the groups as whole plots [47, 227, 324; Chapter XIII]. The partial confounding of treatment differences with block effects makes it unwise to employ lattices when comparing treatments which have a large range in yields [305].

If a design for the required number of treatments is not available, the addition or subtraction of a few treatments is usually all that is required to obtain a lattice of the desired size. An incomplete block design with the treatments not in complete replicates may fit the experimental situation whenever a design with the treatments in complete replicates is not available (tables XI-1 and XI-2).

XI-1.2 RANDOMIZATION PROCEDURE FOR LATTICE DESIGNS WITH ONE RESTRICTION

The randomization procedure for v treatments arranged in b blocks of k treatments each follows that described in Chapter IX for confounded factorial experiments; i.e., the groups of treatments are allotted at random to the b incomplete blocks, and the k treatments within each incomplete block are randomly allotted to the experimental units. The randomization procedure for other lattice designs is discussed in Chapters XII and XIII.

XI-1.3 ANALYSIS FOR LATTICE DESIGNS WITH ONE RESTRICTION

General methods of analysis for lattice designs are available for v treatments in incomplete blocks of size k with r replicates of the treatments [30,

175, Ch. 18, 26, 27; 177; 178; 255]. The method followed in this text is essentially that put forth by Yates [324] and Kempthorne and Federer [177, 178] for factorial experiments. Rao's [255] analysis is carried through for the simple lattice design and for the cubic lattice design. Little progress has been made in generalizing the analysis for experiments with n-restrictions.

The analysis of variance for v treatments in b incomplete blocks of size k is

Source of variation	df	Average value of ms
Block (ignoring treatment)	$b - 1$	—
Treatment (eliminating block)	$v - 1$	$\sigma_e^2 + f$ (treatment effect)
Intrablock	$(kb - v - b + 1)$	σ_e^2
Total	$kb - 1$	—

Alternatively, the analysis for the above design may be written in the form,

Source of variation	df	Mean square	
		Observed	Average value
Treatment (ignoring block)	$v - 1$	—	—
Block (eliminating treatment)	$b - 1$	E_b	$\sigma_e^2 + \frac{bk - v}{b - 1}\sigma_\beta^2$
Intrablock	$(kb - v - b + 1)$	E_e	σ_e^2
Total	$bk - 1$	—	—

Designs with an analysis of variance like the above are treated in Chapter XIII. If the treatments are in complete blocks, the following form of the analysis of variance is appropriate:

Source of variation	df	Mean square	
		Observed	Expected value
Replicate	$r - 1$	—	—
Treatment (ignoring block)	$v - 1$	—	—
Block (eliminating treatment)	$b - r$	E_b	$\sigma_e^2 + \left(\frac{r-1}{r}\right)k\sigma_\beta^2$
Intrablock	$(r - 1)(v - 1) - (b - r)$	E_e	σ_e^2
Total	$bk - 1$	—	—

Designs with the above analysis of variance are discussed in the present chapter. The designs with two or more restrictions are discussed in Chapter XII.

The following formula [177] represents the average variance of the difference between two adjusted treatment means in an experiment of $v = k^n$ treatments in blocks of k treatments in r complete blocks:

$$\frac{2(k-1)}{v-1}\left\{\frac{n_1}{w+(r-1)w'}+\frac{n_2}{2w+(r-2)w'}+\cdots+\frac{n_r}{rw}\right\}, \quad \text{(XI-2)}$$

where n_1 = the number of effects confounded in $(r-1)$ replicates,

n_2 = " " " " " " $(r-2)$ "

.

.

.

n_r = " " " " " " no replicates,

$w = 1/E_e$, and $w' = (r-1)/(rE_b - E_e)$.

The average standard error of a mean difference is obtained by taking the square root of formula (XI-2), and the average effective error variance (average error variance per experimental unit) is equal to $r/2$ times equation (XI-2).

In most situations the average standard error of a mean difference is suitable for all comparisons even if the comparisons are of different accuracies, since the various standard errors differ little from each other or from the average error. However, the various standard errors should be computed in order to check this point for experiments laid out in a p.b.i.b. design. In this connection it should be noted that there is only one standard error in a balanced incomplete block design (b.i.b.).

In certain small experiments the degrees of freedom associated with E_b are less than 10 to 14, and interblock information is usually ignored. Only intrablock information is recovered because E_b is so inaccurately determined. Thus, in designing lattice experiments, particular attention should be paid to the number of degrees of freedom associated with E_b as well as E_e. Given a number of treatments, v, to be compared, the degrees of freedom associated with E_b may be increased by increasing the number of replicates or by decreasing the number of treatments per incomplete block. However, the decrease in the number of treatments per block decreases the number of degrees of freedom associated with E_e, and a complete block design may have to be used in order to have sufficient degrees of freedom in the error mean square.

The block (eliminating treatment effect) mean square, E_b, may be smaller than the intrablock error mean square, E_e. Whenever this is the case, it is suggested that the experiment be analyzed as a complete block design (as a completely randomized design if the incomplete blocks do not form compact replicates) with no adjustments on the treatment means.

XI-1.4 SIZE AND SHAPE OF INCOMPLETE BLOCK AND LAYOUT OF BLOCKS

The optimum size and shape for an incomplete block depends upon the nature of the experimental area and material. The optimum shape of an in-

complete block may be determined along the lines set forth in Chapter III. The shape is adjusted to obtain experimental units as alike as possible within each incomplete block. The area of the incomplete block is determined by the size of the experimental units, the relative variability, and the number of units per incomplete block. The desirable number of experimental units per incomplete block depends upon the heterogeneity among units and the degree to which this may be reduced by decreasing the size of the incomplete block. The effect of size and shape of the incomplete block on the experimental error has been investigated empirically [168; Chapter III].

To date there is little experimental evidence concerning the relationship between the number of experimental units per incomplete block and the average effective error variance. From the results obtained by Cochran [50] and Cox [73], there is an indication that the average effective error tends to increase as the number of units, k, per incomplete block increases. These authors do not give the actual error mean squares but do give the efficiencies relative to the randomized complete block design. The efficiencies tend to increase as k increases. If the error mean squares were available, it would be possible to check the extent of the relationship between k and the average effective error variance. If the increase in the error per unit increase in k is relatively large, then it will be necessary to use smaller incomplete blocks and to pay more attention to the size and shape of incomplete blocks for relatively large values of v.

The one-restrictional lattice design is suitable for many types of material and for many situations. For example, each incomplete block may constitute a day, a judge, or a litter; in a field trial the incomplete blocks may be laid out end to end, etc. This feature represents an advantage of the lattice design over the randomized complete block design, since the extra variation encountered is controlled in the incomplete block design but is not in the randomized complete block design. In field experiments the calculation of efficiency of the incomplete block design with incomplete blocks laid end to end, relative to the randomized complete block design, may not be informative, since one would not ordinarily use a complete block design with such an arrangement.

XI-2 Classification of Lattice Designs

In general, lattice designs may be classified into two broad groups: group I comprising the lattice designs not forming compact replicates and group II the lattice designs forming complete blocks or replicates. Group I is sometimes called "the incomplete block designs" and group II "the lattice designs" [60, 175]; this distinction does not appear logical, since both groups form incomplete block designs. The term "lattice" is used chiefly to designate the group of incomplete block designs in which the v treatments do not represent a factorial arrangement. It should be remembered that all designs discussed

in Chapters IX to XIII are incomplete block designs; the term lattice design is restricted to the group of incomplete block designs discussed in Chapters XI to XIII. However, the distinction is not always clear cut.

The lattices of group I may be classified into the general categories:

(i) balanced with $r = kb/v = \lambda(v - 1)/(k - 1)$,
(ii) partially balanced incomplete blocks (p.b.i.b.), and
(iii) others including one or more restrictions on the allocation of treatments.

The classification of lattice designs forming complete replicates, group II, is given below.

Design	Size of incomplete block	No. of arrangements
Two-dimensional with $v = k^2$ entries (square lattices)		
One-restrictional		
Simple or double lattice	k*	2
Triple lattice	k*	3
Quadruple lattice	k* except 6,10	4
.		
.		
.		
Balanced lattice	k**	$(k + 1)$
Two-restrictional		
Semi-balanced lattice square	k**	$(k + 1)/2$
Balanced lattice square	k**	$(k + 1)$
Unbalanced lattice square	k**	2^{+}
" " "	$k = 6,10$	3
" " "	$k = 12$	**2,3,4**
Two-dimensional with $v = pk$ entries (rectangular lattices)		
One-restrictional (preferable for $k = p - 1$)		
Simple rectangular lattice	k*	2
Triple " "	k*	3
.		
.		
.		
Near balance rectangular lattice	**k****	**k**
Two-restrictional	—	—
Three-dimensional with $v = k^3$ entries (cubic lattices)		
One-restrictional—k^2 blocks of k		
Triple lattice (commonly known as cubic)	**k***	3
Quadruple lattice	**k****	4
Quintuple lattice	**k****	5
.		
.		
.		
Balanced	**k****	$\mathbf{k^2 + k + 1}$
Two-restrictional		
k of k of k	**k****	$\mathbf{3^{+}}$
k rows × $\mathbf{k^2}$ columns	**k****	$\mathbf{3^{+}}$
Three-restrictional		
k × k lattice square with split plots of k	**k***	$\mathbf{3^{+}}$
k k × k lattice squares	**k***	$\mathbf{3^{+}}$

Three-dimensional with $v = kpq$ entries		
One-restrictional	—	—
Two-restrictional	—	—
Three-restrictional	—	—
Four-dimensional with $v = k^4$ entries (quartic lattices)		
One-restrictional		
k^3 blocks of k	k^*	4^+
k^2 " " k^2	k^*	2^+
k " " k^3	k^*	2^+
Two-restrictional		
k^2 blocks of k of k	k^{**}	—
k " " k^2 " k	k^{**}	—
k " " k " k^2	k^{**}	—
k rows $\times$ k^3 columns	k^{**}	—
Three-restrictional		
k^2 $k \times k$ lattice squares	k^{**}	—
$k \times k$ lattice square with split plots of k^2	k^{**}	—
k blocks of k of k of k	k^{**}	—
Four-restrictional		
$k \times k$ lattice squares with split plots of $k \times k$ lattice squares	k^*	—
.		
.		
.		
n-dimensional with k^n entries		
One-restrictional	—	—
.		
.		
.		
n-restrictional	—	—
n-dimensional with $kpq \cdots t$ entries		
One-restrictional	—	—
.		
.		
.		
n-restrictional	—	—

where k^* = any integer, k^{**} = any prime number or power of a prime number, and $^+$ means that more arrangements are available for some values of k.

The number of arrangements, say n, for a given design represents the number of replicates, r, available for the design. Also, any multiple, say q, of the number of arrangements may be used as the number of replicates in a given experiment; thus, $r = nq$. Although the basic set of arrangements may be repeated q times to obtain the desired number of replicates, this is not recommended. Instead, n should be as nearly equal to r as possible, because this tends to equalize the precision of the various comparisons between means. For example, instead of using two sets, $q = 2$, of a triple lattice design for twenty-five treatments and six replicates, one should use a balanced lattice design.

The above classification for group II lattices is more detailed than those given to date [255, 323] but is still not entirely complete. For example, the design containing the same groups of varieties in the incomplete blocks in the different replicates [47, 227, 255; Chapter XIII] is not included in the

classification. However, the above breakdown does serve to illustrate the diversity of types of lattice designs available to the experimenter.

The term *dimensional* is used to refer to the power of the number k in the k^n lattice design. This term was adopted by Yates [326] in his discussion of the three-dimensional lattice known as the cubic lattice in which all the combinations of levels of three factors were likened to the points of the intersections of the three-dimensional lattice (a geometrical configuration; see figure XI-1). Also, combinations of two factors may be likened to the

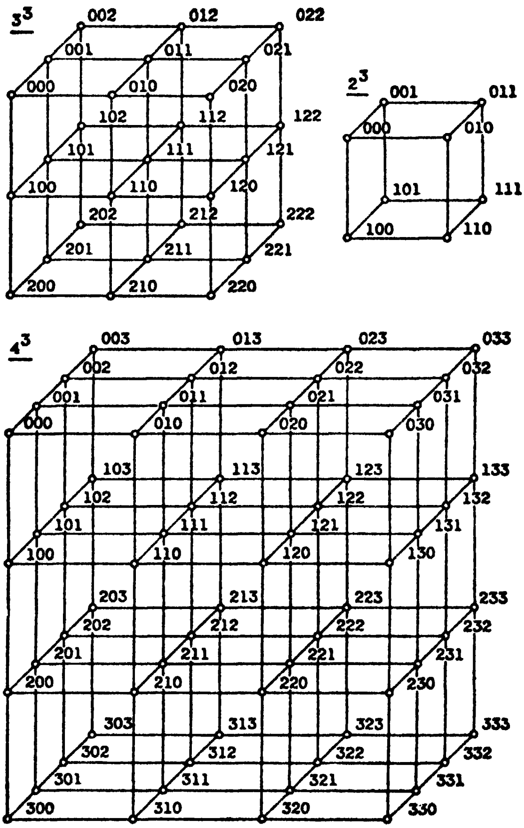


Figure XI-1. Geometrical configuration for cubic lattice designs.

intersections of the lines on ordinary graph paper. An n-dimensional lattice may be thought of as all intersections of the lines in an n-dimensional lattice.

The simplest lattice designs, some designs in group I and the two-dimensional one-restrictional lattices of group II, are subject to a single restriction on the random allocation of varieties; i.e., certain groups of varieties appear together in an incomplete block. Lattice square designs are subject to two restrictions on the random allocation of varieties within the complete block;

the grouping of varieties is by rows and columns (or some similar grouping) within each replicate. The three- and four-restrictional designs are subject to three and four restrictions on the random allocation of the varieties.

Within each of the n-dimensional s-restrictional designs there are *balanced, partially balanced,* and *unbalanced* lattice designs. Balanced lattice designs have the feature that every variety (treatment) is compared with every other variety an equal number of times in the incomplete blocks (or rows and columns). Every pair of varieties has the same value of λ. In order for balance to be attained and for every variety to be compared with every other variety once and only once in the incomplete blocks of size k, it is necessary to have $k + 1$ replicates for k^2 varieties, $k^2 + k + 1$ replicates for k^3 varieties, $k^3 + k^2 + k + 1$ replicates for k^4 varieties, etc., and k must be a prime number or power of a prime number. In designs that are not balanced, the various pairs of varieties have unequal values of λ.

The term *rectangular lattice* has been used by Harshbarger [148-50] when there are $k \times p$ combinations of two factors rather than k^2. Also, he proposed this term to contrast with the notation used by Yates [323] and Goulden [136], who designate a lattice design for k^2 varieties as a *square lattice.* The notation used herein is not inconsistent with that used in the literature, even though it is in a slightly different form.

The two-dimensional one-restrictional partially balanced lattice design in two replicates has been known as the *simple lattice* or the *lattice.* In order to be consistent with the notation for the triple lattice [48, 74] and the quadruple lattice [136], this design should be called a *double lattice.* For the three-dimensional designs the notation triple, quadruple, etc. is used to be consistent with the notation for two-dimensional designs. The prevalent use of the name *cubic lattice* is for the lattice design with k^3 varieties in sets of three replicates, which in the present nomenclature is the three-dimensional triple lattice; the name cubic lattice is retained for this design.

XI-3 Two-Dimensional One-Restrictional Lattice Designs

In constructing lattice designs for k^2 treatments, the entries are designated in the manner described for factorial arrangements. For example, $k^2 = 3^2$ treatments are numbered 00, 01, 02, 10, 11, 12, 20, 21, and 22, and even though the nine treatments are not a factorial arrangement of the factors a and b, they may be likened to a factorial arrangement for purposes of design and analysis.[1] The resulting main effects and interactions are called *pseudo-main*

[1]Yates [324] has denoted the resulting arrangement both as a pseudo-factorial and a quasi-factorial, but states that the latter name appears preferable both descriptively and etymologically. Although the name quasi-factorial describes the arrangement of v variates as a factorial, the terms pseudo-main effect and pseudo-interaction are considered preferable to the terms quasi-main effect and quasi-interaction because the latter terms are inappropriate descriptively.

effects and *pseudo-interactions* to distinguish them from the factorial experiment. In the above example, then, the following pseudo-effects are available: A, B, AB, and AB^2, with the three levels for each effect. In constructing lattice designs for k^2 treatments, various effects are confounded with incomplete block differences in the different replicates with the confounding spread as equally as possible among the pseudo-effects. The analogy between confounding in factorial experiments and lattice designs is illustrated in the examples that follow.

XI-3.1 DOUBLE (SIMPLE) LATTICE

Suppose that it is desired to set up a double lattice design for $k^2 = 3^2$ varieties or treatments. The nine treatments are numbered 00, 01, 02, 10, 11, 12, 20, 21, and 22. The levels of pseudo-effects are obtained by summing the yields for certain treatments. The relationship between the treatments and levels of pseudo-effects are

Level of pseudo-effect	Treatments
$(A)_0$	00 + 01 + 02
$(A)_1$	10 + 11 + 12
$(A)_2$	20 + 21 + 22
$(B)_0$	00 + 10 + 20
$(B)_1$	01 + 11 + 21
$(B)_2$	02 + 12 + 22
$(AB)_0$	00 + 12 + 21
$(AB)_1$	01 + 10 + 22
$(AB)_2$	02 + 11 + 20
$(AB^2)_0$	00 + 11 + 22
$(AB^2)_1$	02 + 10 + 21
$(AB^2)_2$	01 + 12 + 20

Since the pseudo-main effects are no more important than the pseudo-interactions, it matters little which are confounded with incomplete block differences. For simplicity, let the A pseudo-effect be confounded with incomplete block differences in one replicate and let the B pseudo-effect be confounded in the second replicate. The resulting X and Y arrangements (this notation is followed because of its prevalent use in statistical literature) before randomization are

Arrangement X			Level of effect
00	01	02	$(A)_0$
10	11	12	$(A)_1$
20	21	22	$(A)_2$

Arrangement Y			Level of effect
00	10	20	$(B)_0$
01	11	21	$(B)_1$
02	12	22	$(B)_2$

The AB and AB^2 effects are unconfounded with incomplete block differences in both arrangements, and the A and the B effects are completely confounded in the X and in the Y arrangements, respectively. Some information is available on all treatment comparisons even though the comparisons among treatments appearing together in an incomplete block are more accurate than the comparisons of treatments not appearing together in an incomplete block.

Thus, there are $n = 2$ basic arrangements in the double lattice design; the two arrangements are called the basic set. For r replicates $r/2 = q$ of the replicates will have the X arrangement and the remaining q replicates will have the Y arrangement (see section IX-4.2). The general form of the analysis of variance for q sets of a double lattice experiment is

Source of variation	df	Mean square	
		Observed	Expected value
Replicate	$2q - 1$	—	—
Treatment (ignoring block)	$k^2 - 1$	—	—
Block (eliminating treatment)	$2q(k - 1)$	E_b	$\sigma_e^2 + \frac{2q - 1}{2q} k\sigma_\beta^2$
Component (a)	$2(q - 1)(k - 1)$	—	$\sigma_e^2 + k\sigma_\beta^2$
Levels of A × replicate	$(q - 1)(k - 1)$	—	
Levels of B × replicate	$(q - 1)(k - 1)$	—	
Component (b) = confounded vs unconfounded levels	$2(k - 1)$		$\sigma_e^2 + k\sigma_\beta^2/2$
Intrablock	$2qk^2 - k^2 - 2qk + 1$	E_e	σ_e^2
Total	$2qk^2 - 1$	—	—

where σ_e^2 is the population variance associated with the random error deviations in the double lattice and σ_β^2 is the population variance associated with the incomplete block deviations (see section XI-8.2). The expected values are useful in understanding the combination of E_b and E_e used to obtain the estimated amount of information on interblock comparisons. The efficiency factor for the double lattice design is $k/(k + 1)$ (formula (XIII-3)). This result may be obtained by the method given in Chapter IX. Two effects, A and B, are each confounded in one-half of the replicates. The amount of intrablock information on these two effects is $\frac{1}{2}$, and is one on the remaining $k - 1$ effects. Therefore, the average information per effect is $[2(\frac{1}{2}) + (k - 1)(1)] /(k + 1) = k/(k + 1)$, which is the efficiency of this incomplete block design relative to the randomized complete block design when the errors in the two designs are equal.

The method of randomization is as follows:

(i) Assign the numbers $ij = 00, 01, \cdots, k - 1, k - 1$ to the treatments at random.

(ii) Assign the levels of the effects or groups of treatments to the incomplete blocks at random.

(iii) Assign the treatments within each incomplete block to the experimental units at random.

Example XI-1. Double lattice in two replicates

(i) Usual method of analysis

After randomization, a plan such as that presented in table XI-3 might be obtained. The yields are synthetic and are chosen for ease of analysis. The arrangements are allotted to the replicates at random, the treatments making up the levels of the A pseudo-effect in the X arrangement are assigned to the incomplete blocks at random, and the treatments within an incomplete block group are assigned to the plots at random. The same procedure is followed to obtain the experimental randomization for the Y arrangement. The incomplete block totals in replicate I are designated as $(B)_{10}$, $(B)_{11}$, and $(B)_{12}$, corresponding to the $(B)_0$, $(B)_1$, and $(B)_2$ pseudo-effects, respectively, in replicate I; the sum of the yields of treatments 02, 12, and 22 in replicate I is the $(B)_2$ level of pseudo-effect B for replicate I:

$$3 + 2 + 6 = 11 = (B)_{12}.$$

TABLE XI-3. Yields per plot for one set of a double lattice experiment with $k^2 = 3^2$ synthetic treatments in two replicates (Entries or varieties in parentheses)

Replicate I

(Y arrangement)

	(00) 8	(20) 5	(10) 3	(02) 3	(12) 2	(22) 6	(21) 3	(11) 7	(01) 3
Block total	$(B)_{10} = 16$			$(B)_{12} = 11$			$(B)_{11} = 13$		
Replicate total									40

Replicate II

(X arrangement)

	(21) 2	(20) 2	(22) 7	(10) 3	(11) 3	(12) 3	(01) 2	(02) 4	(00) 6
Block total	$(A)_{22} = 11$			$(A)_{21} = 9$			$(A)_{20} = 12$		
Replicate total									32

TABLE XI-4. Total yields and other totals required for the analysis of the double lattice experiment presented in table XI-3

	Treatment numbers and totals						$(A)_{.i}$	$(A)_{2i}$	$(A)_{.i} - 2(A)_{2i}$	c_x'
	(00)	14	(01)	5	(02)	7	26	12	2	.24
	(10)	6	(11)	10	(12)	5	21	9	3	.37
	(20)	7	(21)	5	(22)	13	25	11	3	.37
Totals										
$(B)_{.j}$		27		20		25	72	32	8	
$(B)_{1j}$		16		13		11	40			
$(B)_{.j} - 2(B)_{1j}$		− 5		− 6		3	−8		0	
c_y'		−.61		−.73		.37				

In table XI-4 the treatment totals from replicates I and II are arranged in such a way that the levels of the A effect may be obtained by summing over the rows and the levels of the B effect by summing over the columns. For example,

$$(A)_{.0} = (8 + 6) + (3 + 2) + (3 + 4) = 14 + 5 + 7 = 26$$

and

$$(B)_{.1} = (3 + 2) + (7 + 3) + (3 + 2) = 5 + 10 + 5 = 20.$$

The next step in the analysis is to copy the incomplete block totals, i.e., $(A)_{2i}$ and $(B)_{1j}$, from table XI-3 in the appropriate places. $(A)_{20}$ is placed next to $(A)_{.0}$, $(A)_{21}$ to $(A)_{.1}$, etc. This means that the sum of the group of varieties, which are together in the incomplete block, is placed next to the sum of the totals of the same treatments. The next step is to compute the quantities $(A)_{.i} - 2(A)_{2i}$ and $(B)_{.j} - 2(B)_{1j}$ which correspond directly to the rkc_x and rkc_y values found in various references [48, 74, 160]. The first $(A)_{.i} - 2(A)_{2i}$ value in table XI-4 is $26 - 2(12) = 2$. The computations necessary to obtain the last column and row of table XI-4 are explained later.

The sums of squares for the randomized complete block analysis (top part of table XI-5) should be computed first, since all the sums of squares are required for the analysis of a double lattice experiment. The latter analysis consists of partitioning the randomized complete block error (or residual) sum of squares into two portions:

(i) that due to the variation among incomplete blocks after the treatment effect has been removed = block (eliminating treatment effect)
(ii) and that due to residual variation after removing treatment, complete block or replicate, and incomplete block effects = intrablock.

The block (eliminating treatment effect) sum of squares is computed from the quantities, $(A)_{.i} - 2(A)_{2i}$ and $(B)_{.j} - 2(B)_{1j}$; thus:

$$\begin{aligned}
&\sum_{i=0}^{k-1} \frac{[(A)_{.i} - 2(A)_{2i}]^2}{k(1+1)} - \frac{\{\sum[(A)_{.i} - 2(A)_{2i}]\}^2}{2k^2} + \sum_{j=0}^{k-1} \frac{[(B)_{.j} - 2(B)_{1j}]^2}{k(1+1)} \\
&\quad - \frac{\{\sum[(B)_{.j} - 2(B)_{1j}]\}^2}{2k^2} \qquad \text{(XI-3)} \\
&= \frac{2^2 + 3^2 + 3^2}{2(3)} - \frac{8^2}{18} + \frac{(-5)^2 + (-6)^2 + 3^2}{2(3)} - \frac{(-8)^2}{18} \\
&= 0.111 + 8.111 = 8.222.
\end{aligned}$$

Upon further examination, it will be discovered that the quantities $(A)_{.i} - 2(A)_{2i} = (A)_{1i} - (A)_{2i}$ and $(B)_{.j} - 2(B)_{1j} = (B)_{2j} - (B)_{1j}$ represent the comparison of the total of the treatments which appear together in an incomplete block with the total of the *same* treatments in the replicate in which these treatments do not appear together in an incomplete block. Alternatively, these quantities represent the comparison of the levels of the pseudo-effects in the replicates in which the effect is *unconfounded* with incomplete block differences with the levels in the replicate in which the effect is *confounded* with incomplete block differences. The latter way of viewing these sums of squares explains the divisor for the sums of squares. Each quantity compared is composed of k yields and the sum of squares of coefficients is $1^2 + (-1)^2 = 2$.

The intrablock error sum of squares is obtained by subtracting the block (eliminating treatment) sum of squares from the randomized complete block error sum of

squares, $13.44 - 8.22 = 5.22$ with $(2-1)(k^2-1) - 2(k-1) = (k-1)^2 = 4$ degrees of freedom. The sums of squares and mean squares are summarized in table XI-5.

TABLE XI-5. Analysis of variance for the data of table XI-3

Source of variation	df	ss	ms
Replicate	1	3.56	3.56
Treatment (ignoring block)	8	49.00	6.125
Error (r. c. b.)	8	13.44	1.680 = E_e'
Block (elim. treatment)	4	8.22	2.055 = E_b
Among $(A)_{.i} - 2(A)_{2i}$	2	0.111	
Among $(B)_{.j} - 2(B)_{1j}$	2	8.111	
Intrablock = residual	4	5.22	1.305 = E_e
Total	17	66.00	
Correction for mean	1	268.00	

The average variance of a mean difference between any two treatments for a lattice has been given in general terms by Kempthorne and Federer [177] (formula (XI-2)). Briefly, the average variance of a mean difference is derived in the following manner. The variances of a mean level of an effect in a replicate where it is unconfounded with incomplete block differences, e.g., $(A)_{1i}/k$, and of a mean level of an effect in a replicate where it is confounded with incomplete block differences, e.g., $(B)_{1j}/k$, are σ_ϵ^2/k and $(\sigma_\epsilon^2 + k\sigma_\beta^2)/k$, respectively, where E_e is an estimate of σ_ϵ^2 and $2E_b - E_e$ is an estimate of $\sigma_\epsilon^2 + k\sigma_\beta^2$ in a double lattice with two replicates. Hence, per experimental unit, the estimated variances for intrablock and for interblock comparisons are E_e and $2E_b - E_e$, respectively, and the amount of information on intrablock comparisons is $w = 1/E_e = 1/1.305 = 0.7663$, and on interblock comparisons is $w' = 1/(2E_b - E_e) = 1/[2(2.055) - 1.305] = 0.3565$. The average variance of a difference between two adjusted means is equal to

$$\frac{2}{k+1}\left\{\frac{2}{w+w'} + \frac{k-1}{2w}\right\} = E_e\left\{1 + \frac{2k}{k+1}\mu\right\} \qquad \text{(XI-4)}$$

$$= 1.305\left\{1 + \frac{2(3)}{3+1}0.122\right\} = 1.544,$$

where $\mu = \dfrac{w-w'}{k(w+w')} = \dfrac{E_b - E_e}{kE_b} = 0.122$. The corresponding standard error is 1.24.

For two replicates, formula (XI-4) is the average effective error variance. Since all pairs of treatments do not appear together in an incomplete block, some pairs are subject to different variances than others. The standard error of a difference between two adjusted means for a pair occurring together in an incomplete block is

$$\sqrt{\frac{2}{k}\left\{\frac{1}{w+w'} + \frac{(k-1)}{2w}\right\}} = \sqrt{E_e\{1+\mu\}} = 1.21. \qquad \text{(XI-5)}$$

The standard error of a difference between adjusted means for a pair of treatments not occurring together in an incomplete block is

$$\sqrt{\frac{2}{k}\left\{\frac{2}{w+w'}+\frac{(k-2)}{2w}\right\}} = \sqrt{E_e(1+2\mu)} = 1.27. \qquad \text{(XI-6)}$$

The efficiency of this double lattice relative to the randomized complete block design is the ratio of the two effective error variances in per cent:

$$\frac{1.680}{1.544} = 109 \text{ per cent,}$$

or a gain in efficiency of 9 per cent. If the gain in efficiency is small (less than 10 to 15 per cent), the resulting standard error for a randomized complete block and the unadjusted means are used. If the gain in efficiency is larger than 15 to 20 per cent, the double lattice analysis should be used and the treatment or variety means should be adjusted. In the present example the unadjusted means differ little from the adjusted means, owing to the small gain in efficiency, but the means are adjusted for illustrative purposes.

The first step in obtaining adjusted means is to multiply the $(A)_{.i} - 2(A)_{2i}$ and $(B)_{.j} - 2(B)_{1j}$ values by $\mu = \dfrac{w-w'}{k(w+w')} = 0.122$. The resulting values are entered in the last column and last row, respectively, of table XI-4. For example, the second c_x' value is $\mu\ [(A)_{.1} - 2(A)_{21}] = 0.122(3) = 0.37$, and the last c_y' value is $\mu[(B)_{.2} - 2(B)_{12}] = 0.122(3) = 0.37$. The second step in obtaining the adjusted means is to add the corrections c_x' and c_y' to the corresponding treatment total and divide the resulting sum by two, the number of replicates. The adjusted mean for treatment 01 is $[5 + 0.24 - 0.73]/2 = 2.26$ and for treatment 22 is $[13 + 0.37 + 0.37]/2 = 6.87$. The remainder of the adjusted means (table XI-6) are obtained in a similar manner.

TABLE XI-6. Adjusted totals and means for the experiment in table XI-3

Treatment number	Unadjusted totals	Sum of adjustments	Adjusted totals	Adjusted means
00	14	-.37	13.63	6.82
01	5	-.49	4.51	2.26
02	7	.61	7.61	3.80
10	6	-.24	5.76	2.88
11	10	-.36	9.64	4.82
12	5	.74	5.74	2.87
20	7	-.24	6.76	3.38
21	5	-.36	4.64	2.32
22	13	.74	13.74	6.87
Total	72	.03	72.03	36.02

(ii) Analysis of double lattice as a confounded factorial

In order to understand the various adjustments and sums of squares given in (i), it is helpful to obtain these quantities in the manner described in Chapter IX. For this

method of analysis, it is necessary to obtain the levels of the pseudo-effects in each replicate (table XI-7). These effects are obtained by adding the necessary treatment yields for each pseudo-effect. $(AB)_1$ in replicate I is equal to

$$(AB)_{11} = X_{110} + X_{101} + X_{122} = 3 + 3 + 6 = 12.$$

The block (eliminating treatment effect) sum of squares is the sum of squares of the differences of the levels of the effects confounded in one replicate and the corresponding level of the effect in the replicate in which it is unconfounded; thus:

$$\frac{(14 - 12)^2 + (12 - 9)^2 + (14 - 11)^2}{3(1 + 1)} - \frac{(40 - 32)^2}{9(1 + 1)}$$
$$+ \frac{(11 - 16)^2 + (7 - 13)^2 + (14 - 11)^2}{3(1 + 1)} - \frac{(32 - 40)^2}{9(1 + 1)} = 8.22.$$

The intrablock error sum of squares may be obtained as the interaction of levels of the effects with the replicates in which the effects are unconfounded. The only effects unconfounded in more than one replicate are AB and AB^2. The interaction of levels of these two effects with replicates yields $2 + 2 = 4$ degrees of freedom and a sum of squares of

$$\sum_{g=1}^{2} \sum_{u=0}^{k-1} \frac{(AB)_{gu}^2}{k} - \frac{(X_{1..}^2 + X_{2..}^2)}{k^2} - \frac{\sum (AB)_{\cdot u}^2}{2k} + \frac{X_{...}^2}{2k^2} + \sum_{g} \sum_{u} \frac{(AB^2)_{gu}^2}{k}$$
$$- \frac{(X_{1..}^2 + X_{2..}^2)}{k^2} - \frac{\sum (AB^2)_{\cdot u}^2}{2k} + \frac{X_{...}^2}{2k^2} \qquad \text{(XI-7)}$$

$$= \frac{13^2 + 11^2 + 12^2 + 12^2 + 15^2 + 9^2}{3 = k} - \frac{32^2 + 40^2}{9 = k^2} - \frac{24^2 + 24^2 + 24^2}{2k = 6} + \frac{72^2}{2k^2 = 18}$$
$$+ \frac{21^2 + 16^2 + 9^2 + 9^2 + 10^2 + 7^2}{3 = k} - \frac{32^2 + 40^2}{9 = k^2} - \frac{37^2 + 18^2 + 17^2}{2k = 6} + \frac{72^2}{2k^2 = 18}$$
$$= \frac{(13 - 11)^2 + (12 - 12)^2 + (15 - 9)^2 + (21 - 16)^2 + (9 - 9)^2 + (10 - 7)^2}{2k = 6}$$
$$- \frac{2(40 - 32)^2}{2k^2 = 18} = 5.22.$$

The intrablock error sum of squares need not be obtained by subtraction but may be computed as above. The analysis of variance is the same as that presented in table XI-5. The weights w and w' and the standard errors are computed as before.

Before computing the adjusted means, it is necessary to obtain the weighted levels of the pseudo-effects. This weighting is necessary because the partially confounded effects are estimated with different variances in the two replicates. In the replicates where the effect is confounded with incomplete block differences, the levels of the effect are estimated with a variance equal to $\hat{\sigma}_\epsilon^2 + k\hat{\sigma}_\beta^2 = 1/w'$. The unconfounded effects are estimated with a variance equal to $\hat{\sigma}_\epsilon^2 = 1/w$. The weighted levels of the effects are obtained by weighting the level of an effect inversely to the variance with which it is estimated. The weighted level of the $(A)_0$ effect is

$$\frac{w'(A)_{20} + w(A)_{10}}{w' + w} = \frac{0.3565(12) + 0.7663(14)}{0.3565 + 0.7663} = 13.3650.$$

The remaining levels of effects are computed similarly and are given in the last column of table XI-7.

TABLE XI-7. Weighted and unweighted effects on a total mean per k = 3 plot basis

Level of effect	Replicate I[a]	Replicate II[a]	Unweighted for both reps.	Weighted for both reps.
$(A)_0$	14	$\underline{12}$	13.0	13.3650
$(A)_1$	12	$\underline{9}$	10.5	11.0475
$(A)_2$	14	$\underline{11}$	12.5	13.0475
$(B)_0$	$\underline{16}$	11	13.5	12.5875
$(B)_1$	$\underline{13}$	7	10.0	8.9051
$(B)_2$	$\underline{11}$	14	12.5	13.0475
$(AB)_0$	13	11	12.0	12.0000
$(AB)_1$	12	12	12.0	12.0000
$(AB)_2$	15	9	12.0	12.0000
$(AB^2)_0$	21	16	18.5	18.5000
$(AB^2)_1$	9	9	9.0	9.0000
$(AB^2)_2$	10	7	8.5	8.5000
Total	$160 = (3+1)40$	$128 = (3+1)32$	$144.0 = \frac{(3+1)72}{2}$	144.0001

[a]Underline indicates that level of the effect confounded with incomplete block differences in the specified replicate.

The adjusted means are obtained by using the weighted effects in the last column of table XI-7; thus:

$$\bar{x}_{\cdot ij}' = \frac{[(A)_i + (B)_j]_{wt\cdot d}}{k} + \frac{(AB)_{i+j} + (AB^2)_{i+2j}}{k} - \frac{(X\ldots)}{2k}, \qquad \text{(XI-8)}$$

which for treatment 00 is

$$\frac{13.3650 + 12.5875 + 12.0000 + 18.5000}{3} - \frac{72}{6} = 6.82,$$

and for treatment 02 is

$$\frac{13.3650 + 13.0475 + 12.0000 + 9.0000}{3} - \frac{72}{6} = 3.80.$$

The remaining adjusted means are computed similarly and agree with those in the last column of table XI-6 within rounding errors.

(iii) Application of Rao's general analysis to a simple (double) lattice

In 1947, C. R. Rao [255] described a general method of analysis for incomplete block designs [30, 107, 221, 226]. The incomplete block designs are of the type that

have v varieties or treatments in b incomplete blocks of k treatments with each treatment repeated r times. In the double lattice design, $v = k^2$, $r = 2$, and $b = 2k$.

The double lattice design represents two different groupings of the k^2 treatments in the following manner:

Y grouping of treatments

	00	01	02	...	0, k − 1
	10	11	12	...	1, k − 1
	20	21	22	...	2, k − 1
X grouping of treatments	. . .			. ... .	. . .
	k − 1, 0	k − 1, 1	k − 1, 2	...	k − 1, k − 1

Thus, in the X grouping, treatment 00 appears with the $k - 1$ treatments (01,02, $\cdots$, 0,$k - 1$) in an incomplete block, while in the Y grouping 00 appears with treatments 10, 20, $\cdots$, $k - 1$,0. Every one of the k^2 treatments occurs with $2(k - 1)$ treatments in an incomplete block in the two groupings. Likewise, there are $(k - 1)^2$ treatments with which a given variety is not associated in an incomplete block. On the basis of "association" of treatments in the incomplete block, Rao sets up a system of associates. First associates (or logically zeroth associates) are treatments which do not appear together in an incomplete block. Rao's second associates refer to pairs of treatments appearing together once in an incomplete block.

The terminology zeroth, first, second, etc. associates is adopted herein and is equivalent to Rao's first, second, third, etc. associates. The reason for this change is to have the degree of the associate refer to the number of times, λ_i, that treatment pairs appear together in the incomplete blocks.

In the double lattice there are $n_0 = (k - 1)^2$ zeroth associates and $n_1 = 2(k - 1)$ first associates for any treatment. $\lambda_0 = 0$ refers to a pair of treatments which are not compared in any incomplete block. $\lambda_1 = 1$ refers to a pair of treatments which appear together in one of the incomplete blocks. If a pair of treatments appears together twice in the b incomplete blocks, λ_2 equals 2. However, the association of treatments in the incomplete blocks for the double lattice design is either none or once; therefore, the two parameters are $\lambda_0 = 0$ and $\lambda_1 = 1$.

One further type of parameter peculiar to a given incomplete block design is the number of associates in common among the various pairs of treatments with varying degrees of association. For a pair of treatments which are zeroth associates there are four parameters, $p_{00}{}^0 = (k - 2)^2$ = number of treatments in common among their zeroth associates, $p_{01}{}^0 = p_{10}{}^0 = 2(k - 2)$ = number of treatments in common among their zeroth and first associates, and $p_{11}{}^0 = 2$ = number of treatments in common among the first associates of the two treatments in question.

Likewise, there are four parameters for a pair of treatments which are first associates. Thus, $p_{00}{}^1 = (k - 1)(k - 2)$ = number of treatments in common between zeroth associates of the two in question, $p_{01}{}^1 = p_{10}{}^1 = k - 1$ = number of treatments in common between zeroth and first associates of the two treatments, and $p_{11}{}^1 = k - 2$ = number of treatments in common among their first associates. The parameters for the simple lattice are summarized in table XI-8 along with Rao's general notation.

TABLE XI-8. Parameters for a simple lattice design with the corresponding notation from Rao's paper

Present terminology	Rao's general terminology	Present terminology	Rao's general terminology	Present terminology	Rao's general terminology
k^2 = number of varieties	v	$n_0 = (k-1)^2$	n_1	$p_{ij}^{0} = \begin{Vmatrix} (k-2)^2 & 2(k-2) \\ 2(k-2) & 2 \end{Vmatrix}$	p_{ij}^{1}
2 = " " replicates	r	$\lambda_0 = 0$	λ_1		
2k = " " blocks	b	$n_1 = 2(k-1)$	n_2	$p_{ij}^{1} = \begin{Vmatrix} (k-1)(k-2) & (k-1) \\ (k-1) & (k-2) \end{Vmatrix}$	p_{ij}^{2}
k = " per block	k	$\lambda_1 = 1$	λ_2		

With the parameters in table XI-8 for the simple lattice design the following constants are computed (*the letters* A *and* B *in this section do not refer to pseudo-effects; they are used to retain Rao's symbolism*):

$$\left.\begin{aligned} A_{01} &= 2k-1;\\ A_{11} &= p_{01}^{1} = k-1;\\ B_{01} &= 1;\\ B_{11} &= 2k-1+(p_{00}^{0}-p_{00}^{1}) = k+1. \end{aligned}\right\} \Delta = A_{01}B_{11} - A_{11}B_{01} = 2k^2. \tag{XI-9}$$

check

$$\left.\begin{aligned} A_{00} &= 2(k-1);\\ A_{10} &= -p_{01}^{0} = -2(k-2);\\ B_{00} &= -1;\\ B_{10} &= 2(k-1)-(p_{11}^{1}-p_{11}^{0}). \end{aligned}\right\} \Delta = A_{00}B_{10} - A_{10}B_{00} = 2k^2. \tag{XI-10}$$

$$R = 2w + \frac{2w'}{k-1}. \tag{XI-11}$$

$$\left.\begin{aligned} \Lambda_1 &= w - w'.\\ \Lambda_0 &= 0. \end{aligned}\right\} \tag{XI-12}$$

$$\left.\begin{aligned} A_{01}' &= (2k-1)w + w'.\\ A_{11}' &= \Lambda_1 p_{01}^{1} = (k-1)(w-w').\\ B_{01}' &= w - w'.\\ B_{11}' &= A_{01}' + B_{01}'(p_{00}^{0}-p_{00}^{1})\\ &= (k+1)w + (k-1)w'. \end{aligned}\right\} \Delta' = A_{01}'B_{11}' - A_{11}'B_{01}' = 2k^2w(w+w'). \tag{XI-13}$$

check

$$\left.\begin{aligned} A_{00}' &= 2(k-1)w + 2w'.\\ A_{10}' &= -(w-w')p_{01}^{0}\\ &= -2(k-2)(w-w').\\ B_{00}' &= -(w-w').\\ B_{10}' &= A_{00}' + B_{00}'(p_{11}^{1}-p_{11}^{0})\\ &= (k+2)w + (k-2)w'. \end{aligned}\right\} \Delta' = A_{00}'B_{10}' - A_{10}'B_{00}' = 2k^2w(w+w'). \tag{XI-14}$$

The variance of a mean difference for a pair of treatments which are zeroth associates is

$$\frac{2kB_{10}'}{\Delta'} = \frac{2}{k}\left\{\frac{2}{w+w'} + \frac{k-2}{2w}\right\} \tag{XI-15}$$

and for a pair of treatments which are first associates is

$$\frac{2kB_{11}'}{\Delta'} = \frac{2}{k}\left\{\frac{1}{w+w'} + \frac{k-1}{2w}\right\}. \tag{XI-16}$$

The average variance of a mean difference is

$$\frac{\frac{k^2(k-1)^2}{2}\left(\frac{2}{k}\right)\left(\frac{2}{w+w'} + \frac{k-2}{2w}\right) + \frac{k^2(2k-2)}{2}\left(\frac{2}{k}\right)\left(\frac{1}{w+w'} + \frac{k-1}{2w}\right)}{\frac{k^2\,(k-1)^2}{2} + \frac{k^2(2k-2)}{2}}$$

$$= \frac{2}{k+1}\left(\frac{2}{w+w'} + \frac{k-1}{2w}\right) \tag{XI-17}$$

since there are $\frac{k^2}{2}(k-1)^2$ comparisons among zeroth associates and $2(k-1)\left(\frac{k^2}{2}\right)$ comparisons among first associates.

For Rao's general method of analysis the parameters for the data in table XI-3 are

$$\left.\begin{aligned} &r = 2;\ k^2 = 9;\ b = 6;\ k = 3; \\ &\lambda_0 = 0;\ n_0 = 4;\ \lambda_1 = 1;\ n_1 = 4. \end{aligned}\right\} \tag{XI-18}$$

$$\left.p_{ij}^{0} = \begin{pmatrix} 1 & 2 \\ 2 & 2 \end{pmatrix}; \qquad p_{ij}^{1} = \begin{pmatrix} 2 & 2 \\ 2 & 1 \end{pmatrix}.\right\} \tag{XI-19}$$

From table XI-5, $w = \dfrac{1}{E_e} = 0.7663$ and $w' = \dfrac{1}{2E_b - E_e} = 0.3565$. The constants for the simple lattice are now computed as

$A_{01} = 5$, $A_{11} = 2$, $B_{01} = 1$, $B_{11} = 4$, and $\Delta = 18$.
$A_{00} = 4$, $A_{10} = -2$, $B_{00} = -1$, $B_{10} = 5$, and $\Delta = 18$.
$R = 2w + w' = 1.8891$, $\Lambda_1 = w - w' = .4098$, $\Lambda_0 = 0$,
$A_{01}' = 4.1880$, $A_{11}' = .8196$, $B_{01}' = .4098$, $B_{11}' = 3.7782$,
$A_{00}' = 3.7782$, $A_{10}' = -.8196$, $B_{00}' = -.4098$, $B_{10}' = 4.1880$, and
$\Delta' = A_{01}'B_{11}' - A_{11}'B_{01}' = A_{00}'B_{10}' - A_{01}'B_{00}' = 2k^2w(w + w') = 15.487230$.

Equipped with the A_{ij} and B_{ij}, it is now possible to complete table XI-9. The first column contains the treatment number, the second column contains the treatment totals α (table XI-6), and the third column contains the sum of the block totals containing a particular treatment; for example, treatment 01 appears in the block totals $(B)_{11}$ and $(A)_{20}$, and the β value for treatment 01 is $13 + 12 = 25$.

The fourth column is computed from columns 2 and 3. For example, the second γ value is

$$\gamma = k\alpha - \beta = 3(5) - 25 = -10.$$

The fifth column contains the $(k - 1)^2$ treatments which are zeroth associates of treatment α. The sixth column contains the sum of the γ values for treatment α and its zeroth associates. The second ϵ value is computed as

$$-10 - 7 - 5 - 6 + 17 = -11.$$

The η values in the seventh column are computed from the formula

$$\eta = \frac{(B_{01} + B_{11})\gamma - B_{01}\epsilon}{\Delta} = \frac{(k + 2)\gamma - \epsilon}{2k^2}. \tag{XI-20}$$

For example, the first η value is

$$\frac{(3 + 2)(14) - 25}{18} = 2.5000.$$

With the above values the analysis of variance may now be completed. The replicate and treatment (ignoring block) sums of squares are computed as before. The blocks within replicate (ignoring treatment) sum of squares is

$$\frac{16^2 + 11^2 + 13^2 + 11^2 + 9^2 + 12^2}{3} - \frac{(40^2 + 32^2)}{9} = 5.78.$$

TABLE XI-9. Computations for the simple lattice design by Rao's method

Treatment no.	α	β	$\gamma = k\alpha - \beta$	δ	ϵ	η	A	B	C	D
00	14	28	14	11, 12, 21, 22	25	2.5000	20.7102	60.5115	3.99921	6.8175
01	5	25	-10	10, 12, 20, 22	-11	-2.1667	1.2495	33.9942	-.56162	2.2567
02	7	23	-2	10, 11, 20, 21	-16	0.3333	6.6669	30.8757	.98585	3.8042
10	6	25	-7	01, 02, 21, 22	-11	-1.3333	3.5484	33.9942	.06004	2.8783
11	10	22	8	00, 02, 20, 22	31	.5000	13.9734	67.2483	1.99921	4.8175
12	5	20	-5	00, 01, 20, 21	-16	-.5000	3.2985	31.9452	.04668	2.8650
20	7	27	-6	01, 02, 11, 12	-15	-.8333	5.0277	30.2160	.56004	3.3783
21	5	24	-9	00, 02, 10, 12	-9	-2.0000	1.6593	35.8833	-.50079	2.3175
22	13	22	17	00, 01, 10, 11	22	3.5000	20.8701	60.3516	4.04668	6.8650
Total	72 = G	216 = kG	0	———	0	0.0000	77.0040 = kw'G	385.0200	10.63530	36.0000 = G/2

α = variety or treatment total

β = sum of block totals in which treatment α appears

$\gamma = k\alpha - \beta$

δ = zeroth associates of treatment α

ϵ = sum of the γ values for treatment α and its zeroth associates

$A = kw\alpha - (w - w')\beta$

B = sum of A for treatment α and its zeroth associates

$$C = \frac{(B_{01}' + B_{11}')A - B_{01}'(B)}{\Delta'} = \frac{[(k+2)w + (k-2)w']A - (w - w')B}{2k^2 w(w + w')}$$

$$D = C + \bar{x} - \frac{\Sigma C}{k^2}$$

$$\eta = \frac{(B_{01}' + B_{11})\gamma - B_{01}\epsilon}{\Delta} = \frac{(k+2)\gamma - \epsilon}{2k^2}$$

The treatment (eliminating block) sum of squares is

$$\frac{1}{k}\sum \gamma\eta = 51.44. \tag{XI-21}$$

Therefore, the block (eliminating treatment) sum of squares is

$$5.78 - (49.00 - 51.44) = 8.22,$$

which is the same as obtained previously (table XI-5).

With the analysis of variance the values of $w = .7663$ and $w' = .3565$ are computed. The values of A_{ij}', B_{ij}', and Δ' (see above) and the last four columns of table XI-9 may now be calculated.

The first A value is computed from the formula $A = kw\alpha - (w - w')\beta = 3(.7663)(14) - (.7663 - .3565)28 = 20.7102$. The B value, the sum of the A values for a treatment and its zeroth associates, for treatment 00 is calculated as

$$20.7102 + 13.9734 + 3.2985 + 1.6593 + 20.8701 = 60.5115.$$

The C values are obtained from the formula,

$$\frac{[(k+2)w + (k-2)w']A - (w - w')B}{2k^2w(w+w')} \tag{XI-22}$$

$$= \frac{[5(.7663) + .3565]A - .4098B}{15.487230} = .27041634A - .02646051B,$$

the first C value being 3.99921.

The D values are the adjusted treatment means and should correspond (within rounding errors) to those given in the last column of table XI-6. The adjusted mean for treatment 00 is

$$D = C + \left(\bar{x} - \frac{\sum C}{k^2}\right) = 3.99921 + 2.81830 = 6.8175. \tag{XI-23}$$

Several partial checks are available in the construction of table XI-9; these are:

$$\sum \alpha = G = \text{grand total} = X\ldots. \tag{XI-24}$$

$$\sum \beta = kG. \tag{XI-25}$$

$$\sum \gamma = \sum \epsilon = \sum \eta = \text{zero}. \tag{XI-26}$$

$$\sum A = kw'G. \tag{XI-27}$$

$$\sum B = [(k-1)^2 + 1]\sum A. \tag{XI-28}$$

$$\sum C = \frac{1}{2k^2w(w+w')}\{[(k+2)w + (k-2)w']\sum A - (w - w')\sum B\}$$

$$= w'G\left\{\frac{w'(k-1) - w(k-3)}{2w(w+w')}\right\}. \tag{XI-29}$$

$$\sum D = G/2. \tag{XI-30}$$

Example XI-2. Double lattice with four replicates ($q = 2$ sets). In the event that two or more sets of the double lattice experiment are used, only one additional sum of squares is required for the analysis. The numerical example in table XI-10 contains four replicates or $q = 2$ sets of a double lattice. The first set is X_1 and Y_1, and the second set is X_2 and Y_2. The method of analysis is general for 4, 6, 8, $\cdots$, $2q$ replicates. The double lattice experiment with 2, 4, 6, etc. replicates is partially balanced; if one

replicate is omitted, the design becomes unbalanced, resulting in complications in the analysis. In designing a double lattice experiment with four replicates, different randomizations are required (see table XI-10 for the experimental arrangement) for the two sets. The X and Y arrangements of any set are kept together, with the arrangements being allotted to the replicate at random.

The yield data for $k^2 = 3^2$ treatments in two sets (four replicates) of a double lattice design are presented in table XI-10. The incomplete block totals, $(A)_{2i} = (A)_i$ effect in replicate II, $(B)_{1j} = (B)_j$ effect in replicate I, $(B)_{3j} = (B)_j$ effect in replicate III, and $(A)_{4i} = (A)_i$ effect in replicate IV, and the replicate totals are given in table XI-10. Table XI-11 is similar to table XI-4 except that the totals are from four repli-

TABLE XI-10. Yields from four replicates of a double lattice experiment (Entry or treatment numbers in parentheses)

	Block 1	Block 2	Block 3	Replicate total
Replicate I (Y_1 arrangement)				
	(00) 8, (20) 5, (10) 3	(02) 3, (12) 2, (22) 6	(21) 3, (11) 7, (01) 3	
Block total	$(B)_{10} = 16$	$(B)_{12} = 11$	$(B)_{11} = 13$	$40 = X_{1..}$
Replicate II (X_1 arrangement)				
	(21) 2, (20) 2, (22) 7	(10) 3, (11) 3, (12) 3	(01) 2, (02) 4, (00) 6	
Block total	$(A)_{22} = 11$	$(A)_{21} = 9$	$(A)_{20} = 12$	$32 = X_{2..}$
Replicate III (Y_2 arrangement)				
	(22) 5, (02) 4, (12) 4	(01) 2, (21) 2, (11) 5	(10) 3, (00) 7, (20) 4	
Block total	$(B)_{32} = 13$	$(B)_{31} = 9$	$(B)_{30} = 14$	$36 = X_{3..}$
Replicate IV (X_2 arrangement)				
	(00) 8, (01) 3, (02) 4	(21) 2, (22) 7, (20) 3	(12) 3, (10) 3, (11) 5	
Block total	$(A)_{40} = 15$	$(A)_{42} = 12$	$(A)_{41} = 11$	$38 = X_{4..}$

TABLE XI-11. Table of total yields and other totals required for the analysis of the double lattice experiment in table XI-10

	Treatment numbers and totals			$(A)_{.i}$	$(A)_{21} + (A)_{41} = (X)_i$	$(A)_{.i} - 2(X)_i$	c_x'
	(00) 29	(01) 10	(02) 15	54	12 + 15 = 27	0	.00
	(10) 12	(11) 20	(12) 12	44	9 + 11 = 20	4	.40
	(20) 14	(21) 9	(22) 25	48	11 + 12 = 23	2	.20
$(B)_{.j}$	55	39	52	146	70	6	
$(B)_{1j} + (B)_{3j} = (Y)_j$	16 + 14 = 30	13 + 9 = 22	11 + 13 = 24	76			
$(B)_{.j} - 2(Y)_j$	− 5	− 5	4	−6		0	
c_y'	−.50	−.50	.40				

cates instead of two. The $(A)._i$ and $(B)._j$ totals are obtained by summing the row and column yields, respectively. The second from the last column is the sum of the incomplete block totals from replicates II and IV or the replicates in which the A effect is confounded with incomplete block differences. The second from the last row is obtained from the incomplete block totals of replicates I and III. The next to the last column and row are obtained as in table XI-4, while the last column and row are computed after completing the analysis of variance table (table XI-12).

TABLE XI-12. Analysis of variance for the data of table XI-10

Source of variation	df	ss	ms
Replicate	3	3.89	1.30
Variety (ignoring block)	8	96.89	12.11
Error (r. c. b.)	24	19.11	0.796
Block (eliminating variety)	8	8.62	1.078 = E_b
Component (a)	4	3.45	
Component (b)	4	5.17	
Intrablock	16	10.49	0.656 = E_e
Total	35	119.89	-
Correction for mean	1	592.11	-

The component (b) sum of squares is obtained from the comparison of the level of the effect in the confounded replicates and in the unconfounded replicates; it is exactly analogous to the component (b) sum of squares with two replicates of the double lattice except that $2q$ instead of 2 appears in the divisors; thus:

$$\sum_{i=0}^{k-1}\frac{[(A)._i - 2(X)_i]^2}{2qk} - \frac{[\sum[(A)._i - 2(X)_i]]^2}{2qk^2} + \frac{\sum[(B)._j - 2(Y)_j]^2}{2qk} - \frac{[\sum[(B)._j - 2(Y)_j]]^2}{2qk^2} \tag{XI-31}$$

$$= \frac{0^2 + 4^2 + 2^2}{12} - \frac{6^2}{36} + \frac{(-5)^2 + (-5)^2 + 4^2}{12} - \frac{(-6)^2}{36} = 5.17.$$

The component (a) sum of squares is the interaction of the levels of the effects with the replicates in which the effects are confounded. Thus, the sum of the interaction sums of squares for levels of effect A with replicates II and IV and for levels of effect B with replicates I and III is

$$\frac{12^2 + 15^2 + 9^2 + 11^2 + 11^2 + 12^2}{k = 3} - \frac{32^2 + 38^2}{k^2 = 9} - \frac{27^2 + 20^2 + 23^2}{2k = 6} + \frac{70^2}{2k^2 = 18}$$

$$+ \frac{16^2 + 14^2 + 13^2 + 9^2 + 11^2 + 13^2}{k = 3} - \frac{40^2 + 36^2}{k^2 = 9} - \frac{30^2 + 22^2 + 24^2}{2k = 6} + \frac{76^2}{2k^2 = 18}$$

$= 0.34 + 3.11 = 3.45$, with $2(r/2 - 1)(k - 1) = 4$ degrees of freedom.

The intrablock error is obtained by subtracting the total of the component (*a*) and component (*b*) sums of squares (block (eliminating treatment) sum of squares) from the randomized complete block error sum of squares:

$$19.11 - 3.45 - 5.17 = 10.49.$$

The weights and the weighting factor for adjusting the treatment totals are obtained as

$$w' = \frac{r-1}{rE_b - E_e} = \frac{3}{4(1.078) - 0.656} = 0.821, \tag{XI-32}$$

$$w = \frac{1}{0.656} = 1.524, \tag{XI-33}$$

and

$$\mu = \frac{w - w'}{k(w + w')} = \frac{q(E_b - E_e)}{k(qE_b + (q-1)E_e)} = \frac{0.844}{8.436} = 0.100. \tag{XI-34}$$

The average effective error variance is

$$E_e\left[1 + \frac{2(k)}{k+1}\mu\right] = 0.656\left[1 + \frac{2(3)}{4}(.100)\right] = 0.754. \tag{XI-35}$$

The efficiency of the double lattice relative to the randomized complete block design is $0.796/0.754 = 106$ per cent.

The average standard error of a mean difference between two adjusted means is

$$\sqrt{\frac{2E_e}{r}\left[1 + \frac{2k}{k+1}\mu\right]} = \sqrt{\frac{.754}{2}} = .61. \tag{XI-36}$$

The standard error of a mean difference for two treatments appearing in the same incomplete block is

$$\sqrt{\frac{2E_e(1+\mu)}{r}} = \sqrt{\frac{0.656(1 + .100)}{2}} = .60. \tag{XI-37}$$

The standard error of a mean difference for two treatments not appearing together in an incomplete block is

$$\sqrt{\frac{2E_e(1+2\mu)}{r}} = \sqrt{\frac{0.656(1 + .200)}{2}} = .63. \tag{XI-38}$$

The adjustments for the totals are obtained by multiplying the $(A)_{.i} - 2(X)_i$ and the $(B)_{.j} - 2(Y)_j$ values by μ to obtain the c_x' and c_y' values. These values are then added to the corresponding totals in the same manner as for the previous example to obtain the adjusted totals (table XI-13).

The intrablock error sum of squares may be computed directly as the interaction of levels of unconfounded effects with the replicates in which the effects are unconfounded. Effects AB and AB^2 are unconfounded in all four replicates. Hence, the interaction sum of squares for levels of effects AB and AB^2 with replicates yields $2(4-1)(k-1) = 12$ degrees of freedom. The effect A is unconfounded in replicates I and III and B in replicates II and IV. The interaction sum of squares from the levels of effects A and B with replicates yields $2(k-1)(2-1) = 4$ degrees of freedom. The total of the interaction sums of squares yields 16 degrees of freedom for the intrablock error sum of squares.

TABLE XI-13. Adjusted totals and means for the double lattice experiment in table XI-10

Treatment number	Unadjusted totals	Sum of adjustments	Adjusted totals	Adjusted means
00	29	-.50	28.50	7.12
01	10	-.50	9.50	2.38
02	15	.40	15.40	3.85
10	12	-.10	11.90	2.98
11	20	-.10	19.90	4.98
12	12	.80	12.80	3.20
20	14	-.30	13.70	3.42
21	9	-.30	8.70	2.18
22	25	.60	25.60	6.40
Total	146	0.00	146.00	36.51

XI-3.2 TRIPLE LATTICE

The triple lattice design is constructed similarly to the double lattice. The X and Y arrangements are constructed in the same manner, but another arrangement, Z, is added. The triple lattice experiment may be used with 3, 6, 9, ⋯, $3q$ replicates. The AB (or AB^2, AB^3, or any other) effect is confounded with incomplete block differences in the Z arrangement. For the $k^2 = 3^2$ triple lattice the effects A, B, and AB^2 (or AB) are each confounded with incomplete block differences in one arrangement and unconfounded in the other two, while AB (or AB^2) is unconfounded in all three arrangements. The randomization procedure for the triple lattice follows that for the double lattice; thus:

(i) assign the treatment numbers to the treatments at random,
(ii) assign the arrangements to the replicates and the level of the effects to the incomplete blocks at random, and
(iii) assign the treatments within the incomplete blocks to the plots at random.

For more than one set of the triple lattice, proceed in the same manner as described for the double lattice.

The general form of the analysis of variance for a triple lattice is

Source of variation	df	Mean square	
		Observed	Expected value
Replicate	$3q - 1 = r - 1$	—	—
Treatment (ignoring block)	$k^2 - 1$	—	—
Block (eliminating treatment)	$3q(k - 1)$	E_b	$\sigma_e^2 + \frac{3q-1}{3q}k\sigma_\beta^2$
Component (a)	$3(q - 1)(k - 1)$	—	$\sigma_e^2 + k\sigma_\beta^2$
Component (b)	$3(k - 1)$	—	$\sigma_e^2 + 2k\sigma_\beta^2/3$
Intrablock	$(3q - 1)(k^2 - 1) - 3q(k - 1)$	E_e	σ_e^2
Total	$3qk^2 - 1$	—	—

The component (*a*) sum of squares represents the interaction of levels of the *A* and *B* effects with the replicates in which these effects are confounded. Component (*b*) represents the contrasts of the levels of the effects in the replicates in which the effects are confounded with the same effects in the remaining replicates.

Example XI-3. The field arrangement and yields (synthetic) for a $k^2 = 3^2$ triple lattice with $q = 1$ set of three replicates are given in table XI-14. The incomplete block totals in replicates I, II, and III are $(B)_{1j} = (B)_j$ effect in replicate I, $(AB^2)_{2h} = (AB^2)_h$ effect in replicate II, and $(A)_{3i} = (A)_i$ effect in replicate III.

TABLE XI-14. Yields and their arrangements for a triple lattice design with 3^2 treatments in three replicates (Treatment numbers in parentheses)

Replicate I (Y arrangement)

	Block 1			Block 2			Block 3			Replicate total
	(00) 8	(20) 5	(10) 3	(02) 3	(12) 2	(22) 6	(21) 3	(11) 7	(01) 3	
Block total	$(B)_{10} = 16$			$(B)_{12} = 11$			$(B)_{11} = 13$			$40 = X_{1..}$

Replicate II (Z arrangement)

	Block 1			Block 2			Block 3			Replicate total
	(01) 3	(20) 3	(12) 3	(11) 5	(22) 7	(00) 8	(10) 3	(21) 2	(02) 4	
Block total	$(AB^2)_{22} = 9$			$(AB^2)_{20} = 20$			$(AB^2)_{21} = 9$			$38 = X_{2..}$

Replicate III (X arrangement)

	Block 1			Block 2			Block 3			Replicate total
	(21) 2	(20) 2	(22) 7	(10) 3	(11) 3	(12) 3	(01) 2	(02) 4	(00) 6	
Block total	$(A)_{32} = 11$			$(A)_{31} = 9$			$(A)_{30} = 12$			$32 = X_{3..}$

TABLE XI-15. Total yields and other totals required for the analysis of the triple lattice experiment in table XI-14

	Treatment numbers and totals			$(A)_{.i}$	$(A)_{3i}$	$(A)_{.i} - 3(A)_{3i}$	c_x'
	(00) 22	(01) 8	(02) 11	41	12	5	.36
	(10) 9	(11) 15	(12) 8	32	9	5	.36
	(20) 10	(21) 7	(22) 20	37	11	4	.29
$(B)_{.j}$	41	30	39	110	32	14	
$(B)_{1j}$	16	13	11	40			
$(B)_{.j} - 3(B)_{1j}$	- 7	- 9	6	-10			
c_y'	-.51	-.66	.44				

	Treatment numbers and totals			$(A)_{.i}$
	(00) 22	(01) 8	(02) 11	
	(11) 15	(12) 8	(10) 9	
	(22) 20	(20) 10	(21) 7	
$(AB^2)_{.h}$	57	26	27	110
$(AB^2)_{2h}$	20	9	9	38
$(AB^2)_{.h} - 3(AB^2)_{2h}$	- 3	- 1	0	- 4
c_z'	-.22	-.07	.00	

The next step in the analysis is to obtain the treatment totals and arrange them in the X, Y, and Z groupings. Summing over the rows in the first part of table XI-15 results in the $(A)._i$ levels of effect A. The column totals are the levels of the B effect. The last part of table XI-15 is required to obtain the totals for the levels of the AB^2 effect, i.e., the row totals yield the $k = 3$ levels of effect AB^2.

The $(A)._i - 3(A)_{3i}$, $(B)._j - 3(B)_{1j}$, and $(AB^2)._h - 3(AB^2)_{2h}$ quantities represent the comparisons of the levels of the confounded effect from one replicate with the unconfounded effect from the other two replicates. The sum of the sums of squares of these quantities is equal to the component (b) sum of squares:

$$\frac{\sum[(A)._i - 3(A)_{3i}]^2}{qk(1+1+4)} - \frac{\{\sum[(A)._i - 3(A)_{3i}]\}^2}{6qk^2} + \frac{\sum[(B)._j - 3(B)_{1j}]^2}{6qk}$$
$$- \frac{\{\sum[(B)._j - 3(B)_{1j}]\}^2}{6qk^2} + \frac{\sum[(AB^2)._h - 3(AB^2)_{2h}]^2}{6qk} - \frac{\{\sum[(AB^2)._h - 3(AB^2)_{2h}]\}^2}{6qk^2}$$
$$= \frac{5^2 + 5^2 + 4^2}{3(6)} - \frac{14^2}{54} + \frac{(-7)^2 + (-9)^2 + 6^2}{18} - \frac{(-10)^2}{54} + \frac{(-3)^2 + (-1)^2 + 0^2}{18}$$
$$- \frac{(-4)^2}{54} = 7.67. \tag{XI-39}$$

The component (a) sum of squares is the total of the interaction sums of squares for levels of the effect and the replicates in which the effect is confounded. For a single set, $q = 1$, the component (a) sum of squares does not exist. For two or more sets the component (a) sum of squares may be computed similarly to that for the double lattice in example XI-2. The sum of the component (a) and (b) sums of squares yields the block (eliminating treatment) sum of squares:
$0.00 + 7.67 = 7.67$ with $3q(k-1) = r(k-1) = 3(3-1) = 6$ degrees of freedom.

The intrablock error sum of squares is obtained by subtracting the block (eliminating treatment) sum of squares from the randomized complete block error sum of squares:

$$14.82 - 7.67 = 7.15,$$

with $(r-1)(k^2-1) - r(k-1) = rk^2 - k^2 - rk + 1 = 10$ degrees of freedom. The sums of squares and mean squares are presented in table XI-16.

TABLE XI-16. Analysis of variance

Source of variation	df	ss	ms
Replicate	2	3.85	1.92
Treatment (ignoring block)	8	81.18	10.15
Error (r. c. b.)	16	14.82	0.926
Block (eliminating treatment)	6	7.67	$1.278 = E_b$
Component (a)	0	0.00	
Component (b)	6	7.67	
Intrablock	10	7.15	$0.715 = E_e$
Total	26	99.85	-
Correction for mean	1	448.15	-

The weights, w and w', for the triple lattice design are obtained from the intra-block error and block (eliminating treatment) variances:

$$w = 1/E_e = 1/0.715 = 1.399, \tag{XI-40}$$

$$w' = (r-1)/(rE_b - E_e) = 2/[3(1.278) - 0.715] = 0.641. \tag{XI-41}$$

The weighting factor, μ, is obtained as follows:

$$\mu = \frac{(w - w')}{k(2w + w')} = \frac{q[E_b - E_e]}{k[2qE_b + (q-1)E_e]} = \frac{[1.278 - 0\ 715]}{3[2(1.278)]} = 0.073. \tag{XI-42}$$

The product of μ and each of the quantities $(A)_{\cdot i} - 3(A)_{3i}$, $(B)_{\cdot j} - 3(B)_{1j}$, and $(AB^2)_{\cdot k} - 3(AB^2)_{2k}$ yields the corrections for adjusting the treatment totals. These are entered in the last row of both parts of table XI-15 and in the last column of the first part of the table. The adjusted total for any treatment is obtained by adding three correction terms to the treatment total, one for each of the three arrangements. The adjusted mean for treatment 12 is

$$\frac{1}{3}[8 + 0.36 + 0.44 - 0.07] = 2.91.$$

The remaining adjusted means are obtained similarly and are given, together with the sum of the adjustments, in table XI-17. The sum of the adjustments equals zero within rounding errors.

TABLE XI-17. Adjusted totals and means for the triple lattice experiment in table XI-14

Treatment number	Unadjusted totals	Sum of adjustments	Adjusted totals	Adjusted means
00	22	-.37	21.63	7.21
01	8	-.37	7.63	2.54
02	11	.80	11.80	3.93
10	9	-.15	8.85	2.95
11	15	-.52	14.48	4.83
12	8	.73	8.73	2.91
20	10	-.29	9.71	3.24
21	7	-.37	6.63	2.21
22	20	.51	20.51	6.84
Total	110	-.03	109.97	36.66

The average effective error variance for the triple lattice design is

$$\frac{3}{k+1}\left\{\frac{3}{2w+w'} + \frac{k-2}{3w}\right\} = E_e\left\{1 + \frac{3k}{k+1}\mu\right\} = 0.715\left\{1 + \frac{9}{4}(.073)\right\}$$

$$= 0.832. \tag{XI-43}$$

The efficiency of the triple lattice relative to a randomized complete block design is

$$\frac{0.926}{0.832} = 111 \text{ per cent.}$$

The average standard error of a difference between any two adjusted treatment means is

$$\sqrt{\frac{2}{k+1}\left\{\frac{3}{2w+w'}+\frac{k-2}{3w}\right\}} = \sqrt{\frac{2E_e}{r}\left\{1+\frac{3k\mu}{k+1}\right\}} = \sqrt{\frac{2}{3}(.715)\left[1+\frac{9}{4}(0.073)\right]}$$
$$= 0.745. \qquad \text{(XI-44)}$$

The standard error of a mean difference between two treatments appearing together in an incomplete block is

$$\sqrt{\frac{2E_e}{r}(1+2\mu)} = \sqrt{\frac{2}{3}(.715)[1+2(.073)]} = 0.74. \qquad \text{(XI-45)}$$

The standard error of a mean difference between two treatments not appearing together in an incomplete block is

$$\sqrt{\frac{2E_e}{r}(1+3\mu)} = \sqrt{\frac{2(.715)}{3}[1+3(.073)]} = 0.76. \qquad \text{(XI-46)}$$

For more than one set of the triple lattice, the analytical procedure follows that outlined for the double lattice with slight alterations.

XI-3.3 BALANCED LATTICE

The balanced lattice design requires that all pseudo-effects be confounded equally in the set of replicates chosen. If one pseudo-effect or interaction is confounded in each replicate, it requires $k+1$ replicates for the balanced lattice. Balanced lattices are available for all prime numbers or powers of prime numbers (for example, $k = 3, 4, 5, 7, 8, 9, 11$, etc.) as well as for some other integers.

The general form of the analysis of variance for q sets of a balanced lattice with k^2 treatments is

Source of variation	df	Mean square	
		Observed	Expected value
Replicate	$q(k+1)-1$	—	—
Treatment (ignoring block)	k^2-1	—	—
Block (eliminating treatment)	$q(k^2-1)$	E_b	$\sigma_e^2 + \frac{q(k+1)-1}{q(k+1)} k\sigma_\beta^2$
Component (a)	$(q-1)(k^2-1)$	—	$\sigma_e^2 + k\sigma_\beta^2$
Component (b)	(k^2-1)	—	$\sigma_e^2 + \frac{k^2}{k+1}\sigma_\beta^2$
Intrablock	$(k^2-1)(qk-1)$	E_e	σ_e^2
Total	$qk^2(k+1)-1$	—	—

Example XI-4. The illustrative example chosen is for $k^2 = 3^2$ treatments in $k + 1 = 4$ replicates. Effect A is confounded with incomplete block differences in replicate III, B in replicate I, AB^2 in replicate II, and AB in replicate IV. The first three replicates of table XI-14 and one additional replicate represent the example for a balanced lattice (table XI-18). The 3^2 balanced lattice with four replicates is also a quadruple lattice, but the analysis may be somewhat shortened owing to the balanced feature of the design. The randomization plan follows that for the triple lattice.

TABLE XI-18. Yields and field arrangement for a balanced lattice with 3^2 varieties in four replicates (Treatment numbers in parentheses)

				Replicate total
	Replicate I (Y arrangement)			
	(00) (20) (10)	(02) (12) (22)	(21) (11) (01)	
	8 5 3	3 2 6	3 7 3	
Block total	$(B)_{10} = 16$	$(B)_{12} = 11$	$(B)_{11} = 13$	$40 = X_{1..}$
	Replicate II (Z arrangement)			
	(01) (20) (12)	(11) (22) (00)	(10) (21) (02)	
	3 3 3	5 7 8	3 2 4	
Block total	$(AB^2)_{22} = 9$	$(AB^2)_{20} = 20$	$(AB^2)_{21} = 9$	$38 = X_{2..}$
	Replicate III (X arrangement)			
	(21) (20) (22)	(10) (11) (12)	(01) (02) (00)	
	2 2 7	3 3 3	2 4 6	
Block total	$(A)_{32} = 11$	$(A)_{31} = 9$	$(A)_{30} = 12$	$32 = X_{3..}$
	Replicate IV (W arrangement)			
	(11) (20) (02)	(22) (10) (01)	(00) (12) (21)	
	5 4 4	5 3 2	7 4 2	
Block total	$(AB)_{42} = 13$	$(AB)_{41} = 10$	$(AB)_{40} = 13$	$36 = X_{4..}$

TABLE XI-19. Total yields and other totals required for the analysis of the balanced lattice in table XI-18, adjusted totals and adjusted means

Treatment number	Treatment total = V	Sum of block totals containing treatment = SB	kV + G − (k + 1)SB = W	μW	Adjusted treatment total	Adjusted treatment mean
00	29	61	−11	−.60	28.40	7.10
01	10	44	0	.00	10.00	2.50
02	15	45	11	.60	15.60	3.90
10	12	44	6	.33	12.33	3.08
11	20	55	−14	−.77	19.23	4.81
12	12	42	14	.77	12.77	3.19
20	14	49	− 8	−.44	13.56	3.39
21	9	46	−11	−.60	8.40	2.10
22	25	52	13	.71	25.71	6.43
Total	146	438	0	.00	146.00	36.50

As with other lattices a table of totals (table XI-19) is required. This table for the balanced lattice is different from those (tables XI-4 and XI-15) for the double and triple lattice designs, although a comparable table could be used if desired. The second column of table XI-19 contains the treatment totals. The third column contains the

block totals in which a treatment appears in the $k + 1 = 4$ replicates. Thus, treatment 20 appears, by chance, in the first incomplete block in replicates I, II, III, and IV, and the corresponding total of the incomplete blocks is $16 + 9 + 11 + 13 = 49$. The fourth column is obtained by adding k times the treatment total ($=kV$) to the

TABLE XI-20. Analysis of variance

Source of variation	df	ss	ms
Replicate	3	3.89	1.30
Treatment (ignoring block)	8	96.89	12.11
Error (r. c. b.)	24	19.11	0.796
Block (eliminating treatment)	8	9.48	1.185 = E_b
Intrablock	16	9.63	.602 = E_e
Total	35	119.89	-
Correction for mean	1	590.11	-

grand total ($=G$) and subtracting $k + 1$ times the sum of the block totals in which the treatment appears ($=(k + 1)SB$). For treatment 01 this quantity is

$$3(10) + 146 - (3 + 1)44 = 0.$$

After performing the computations in columns 2, 3, and 4 of table XI-19, the analysis of variance for the balanced lattice may be computed (table XI-20). The replicate, treatment, and total sums of squares are computed in the usual manner. The sum of squares of the $kV + G - (k + 1)SB = W$ quantities, divided by $k^3(k + 1)$,

$$\frac{\sum W^2}{k^3(k + 1)} = \frac{1024}{108} = 9.48, \tag{XI-47}$$

yields the sum of squares for block (eliminating treatment) with $r(k - 1) = (k + 1)(k - 1) = k^2 - 1 = 8$ degrees of freedom. The intrablock error sum of squares is obtained by subtracting the block (eliminating treatment) sum of squares from the randomized complete block error sum of squares. $19.11 - 9.48 = 9.63$, with $(k^2 - 1)(r - 1) - r(k - 1) = k^3 - k^2 - k + 1 = 16$ degrees of freedom.

The weighting factor μ is

$$\mu = \frac{w - w'}{k(kw + w')} = \frac{E_b - E_e}{k^2E_b} = \frac{1.185 - .602}{9(1.185)} = .0547. \tag{XI-48}$$

The adjusted totals are obtained by adding μW to the unadjusted total, or by the method described in Chapter IX. The adjusted total for treatment 21 is $9 - .60 = 8.40$, and the adjusted mean is $(9 - .60)/4 = 2.10$.

The average effective error mean square is

$$E_e(1 + k\mu) = 0.602[1 + 3(.0547)] = .701. \tag{XI-49}$$

The efficiency of the balanced lattice relative to the randomized complete block design is the ratio in per cent of the two average effective error variances, $.796/.701 = 114$ per cent.

The standard error of a mean difference between any two adjusted treatment neans is

$$\sqrt{\frac{2}{k+1}\left(\frac{k+1}{kw+w'}\right)} = \sqrt{\frac{2E_e}{k+1}[1+k\mu]} = \sqrt{\frac{2(.701)}{4}} = .59. \qquad \text{(XI-50)}$$

All treatment comparisons are of equal accuracy, since each treatment is compared once and only once with every other treatment in an incomplete block. Hence, only a single standard error of a mean difference is required. This is a feature common to balanced lattice designs.

The coefficient of variation is computed similarly to that for all lattices; i.e., the square root of the average effective error variance is divided by the mean of the experiment:

$$\frac{\sqrt{.701}}{146/36} = 21 \text{ per cent.}$$

As with other lattice designs, several checks are available in computing the totals of table XI-19. The sum of column 2 equals the grand total. The sum of column 3 equals k times the grand total, $3(146) = 438$. The sum in both columns 4 and 5 is equal to zero. The sum of column 6 is equal to the grand total, within rounding errors. The sum of column 7 is equal to the grand total divided by the number of replicates, within rounding errors.

XI-3.4 RECTANGULAR LATTICES

The two-dimensional one-restrictional lattice design composed of kp treatments is known as the rectangular lattice. This class of designs was first proposed by Yates [323, 324], and some of these designs are included in the general class of designs discussed by Bose and Nair [30]. The designs proposed by Yates had incomplete blocks of size k in one arrangement and of size p in the second arrangement. The analysis and discussion of the particular designs for $k(k+1)$ treatments in incomplete blocks of k treatments in all arrangements are given by Harshbarger [148-50]. The analytical procedure for the designs discussed by Harshbarger is only slightly more complex than that for lattice designs with k^2 treatments in incomplete blocks of k treatments. Since this is true and since $k(k+1)$ treatments as well as k^2 treatments may be put into incomplete blocks of size k, this class of designs is an important one.

There are several methods for constructing $k(k+1)$ rectangular lattices [60, 148-50, 222, 266]; the method of orthogonal latin squares is the one illustrated below. The first step is to select the $p-1$ orthogonal $p \times p$ latin squares of side $p = k+1$ [129]. From these squares, select the $k-1$ orthogonal squares for which the leading diagonal can be ordered numerically. For example, these squares for the 3×3 and 4×4 latin squares are

3×3 square

c_0	c_2	c_1
c_2	c_1	c_0
c_1	c_0	c_2

4×4 squares

c_0	c_2	c_3	c_1
c_3	c_1	c_0	c_2
c_1	c_3	c_2	c_0
c_2	c_0	c_1	c_3

d_0	d_3	d_1	d_2
d_2	d_1	d_3	d_0
d_3	d_0	d_2	d_1
d_1	d_2	d_0	d_3

If the letter *a* is used to denote the column number and the letter *b* to denote the row number, if the squares are combined, and if the leading diagonal is omitted, the following arrangements for the 3 × 3 and 4 × 4 squares are obtained:

—	(1) $a_1b_0c_2$	(2) $a_2b_0c_1$
(3) $a_0b_1c_2$	—	(4) $a_2b_1c_0$
(5) $a_0b_2c_1$	(6) $a_1b_2c_0$	—

—	(1) $a_1b_0c_2d_3$	(2) $a_2b_0c_3d_1$	(3) $a_3b_0c_1d_2$
(4) $a_0b_1c_3d_2$	—	(5) $a_2b_1c_0d_3$	(6) $a_3b_1c_2d_0$
(7) $a_0b_2c_1d_3$	(8) $a_1b_2c_3d_0$	—	(9) $a_3b_2c_0d_1$
(10) $a_0b_3c_2d_1$	(11) $a_1b_3c_0d_2$	(12) $a_2b_3c_1d_0$	—

The next step is to select a letter and to assign identical subscripts to an incomplete block. Thus, for the 3 × 2 rectangular lattice the following three arrangements are obtained:

X

a_1	(1)	(6)
a_2	(2)	(4)
a_0	(3)	(5)

Y

b_0	(1)	(2)
b_1	(3)	(4)
b_2	(5)	(6)

Z

c_2	(1)	(3)
c_1	(2)	(5)
c_0	(4)	(6)

Each group is assigned to a replicate. For the 3 × 4 rectangular lattice the following four arrangements are obtained:

X

a_1	(1)	(8)	(11)
a_2	(2)	(5)	(12)
a_3	(3)	(6)	(9)
a_0	(4)	(7)	(10)

Y

b_0	(1)	(2)	(3)
b_1	(4)	(5)	(6)
b_2	(7)	(8)	(9)
b_3	(10)	(11)	(12)

W

c_2	(1)	(6)	(10)
c_3	(2)	(4)	(8)
c_1	(3)	(7)	(12)
c_0	(5)	(9)	(11)

Z

d_3	(1)	(5)	(7)
d_1	(2)	(9)	(10)
d_2	(3)	(4)	(11)
d_0	(6)	(8)	(12)

The same procedure may be followed for constructing other rectangular lattices. A number of these arrangements have been tabulated by Robinson and Watson [259].

To obtain a *simple rectangular lattice*, any two (usually the first two) of the $k + 1$ arrangements may be selected. Likewise, for a *triple rectangular lattice* any three arrangements may be used; the first three are ordinarily selected. If $k + 1$ groups are used, the design is called a *near balance rectangular lattice* [150].

The general form of the analysis of variance for the $k(k + 1)$ rectangular lattice is

Source of variation	df	Expected value of ms
Replicate	$nq - 1 = r - 1$	—
Treatment (ignoring block)	$k(k + 1) - 1$	—
Block (eliminating treatment)	rk	$\sigma_e^2 + \frac{r-1}{r}k\sigma_\beta^2$
Component (a)	$nk(q - 1)$	—
" (b)	nk	—
Intrablock error	$rk^2 - r - k^2 - k + 1$	σ_e^2
Total	$rk(k + 1) - 1$	—

where q = number of repetitions of the basic plan of n arrangements, component (a) represents the sum of squares due to the interaction of confounded effects and replicates in which effects are confounded, and component (b) represents the comparison between the groups of treatments in replicates in which they are unconfounded with the same treatments in the replicates in which the group comparisons are confounded with incomplete block differences.

The weighting factors for rectangular lattices with $k(k + 1)$ treatments are of the form,

$$\lambda = \frac{(E_b - E_e)}{[k(n-1) - 1]E_b + [(k+1) - k/q]E_e} \tag{XI-51}$$

and

$$\mu = \frac{\lambda^2}{1 + n\lambda}. \tag{XI-52}$$

The approximate average standard error of a difference between two adjusted means is [60, 259]:

$$\sqrt{\frac{2E_e}{r}\left\{\left(\frac{n(k-1)}{k^2+k-1}\right)(1+(n-1)\lambda-\mu)+\left(\frac{k^2-(n-1)(k-1)}{k^2+k-1}\right)\left(1+n\lambda-\frac{n\mu}{2}\right)\right\}}. \tag{XI-53}$$

The symbols used above are the same as for other designs (E_b = block (eliminating treatment effect) mean square; E_e = intrablock error mean

square; n = number of arrangements in the basic plan; q = number of repetitions of the basic plan, $qn = r$ = total number of replicates; and $k(k+1)$ = the number of treatments). Use is made of the weighting factors to obtain weights for adjusting the treatment means. The analysis and the adjustment of treatment totals are illustrated below with an example of a triple rectangular lattice design. The randomization procedure follows that for the simple, triple, etc. lattices described above.

Example XI-5. The 2×3 triple rectangular lattice example in table XI-21 was prepared by omitting treatments 02, 11, and 20 from the experiment described in example XI-3. The notation a_j, b_j, and c_j follows the method of constructing the three arrangements for the 2×3 triple rectangular lattice described above.

Robinson and Watson [259] describe the analysis in detail for the simple and triple rectangular lattices. Their procedure and notation are followed to a considerable extent in setting up table XI-21 and in obtaining the analysis of variance table. The construction of the various totals and differences and the randomized complete block analysis are straightforward. The sum of squares for the component (b) equals the block (eliminating treatment effect) sum of squares, since there is no component (a) sum of squares; this is equal to

$$\sum_{j=0}^{2} \frac{(Q_{xj}^2 + Q_{yj}^2 + Q_{zj}^2)}{r(nk-k-1)} - \frac{Q_{x\cdot}^2 + Q_{y\cdot}^2 + Q_{z\cdot}^2}{r(k+1)(nk-k-1)} - \frac{\sum Q_{\cdot j}^2}{r(n-1)(k+1)(nk-k-1)} \tag{XI-54}$$

$$= \frac{(-2)^2 + (-3)^2 + \cdots + 6^2}{9} - \frac{(-4)^2 + (-1)^2 + 5^2}{27} - \frac{(-4)^2 + (-7)^2 + 11^2}{54} = 3.89.$$

The intrablock error sum of squares is equal to $4.55 - 3.89 = 0.66$, with $10 - 3(2) = 4$ degrees of freedom.

The weighting factors are found to be

$$\lambda = \frac{[.648 - .165]}{3(.648) + .165} = \frac{.483}{2.109} = 0.229$$

and

$$\mu = \frac{\lambda^2}{1 + 3\lambda} = .031.$$

The average standard error as obtained from formula (XI-53) is equal to

$$\sqrt{\frac{2(.165)}{3}\left\{\frac{3}{5}[1 + 2(.229) - .031] + \frac{2}{5}[1 + 3(.229) - \frac{3}{2}(.031)]\right\}} = .41.$$

The average effective error variance is obtained by squaring formula (XI-53) and multiplying the result by $r/2$. For this example the average effective error variance equals $(.165)(1.5124) = .2495$. The coefficient of variation in per cent is equal to the square root of the average effective error variance divided by the mean of the experiment: $\frac{\sqrt{.2495}}{74/18} \times 100 = 12$ per cent. The efficiency of this rectangular lattice design relative to the corresponding randomized complete block design is the ratio of the two average effective error variances expressed in per cent; thus, $.455 \times 100/.2495 = 182$ per cent, or a gain in efficiency of 82 per cent. The above efficiency has not been

TABLE XI-21. Numerical example of a 2 × 3 triple rectangular lattice

Replicate I (Y arrangement)	Block no.	Yields	Block total = B_{yj}	$Q_{yj} = B_{.j} - nB_{yj}$	$\lambda Q_{yj} - \mu Q_{.j}$
	b_0	(1) 8 (2) 3	11	-2	-.334
	b_2	(5) 2 (6) 6	8	4	.575
	b_1	(4) 3 (3) 3	6	-3	-.470
Total			25	-1	-.229
Replicate II (X arrangement)	**Block no.**	**Yields**	**Block total = B_{xj}**	**$Q_{xj} = B_{.j} - rB_{xj}$**	**$\lambda Q_{xj} - \mu Q_{.j}$**
	a_0	(3) 3 (5) 3	6	-2	-.334
	a_1	(6) 7 (1) 8	15	-3	-.470
	a_2	(2) 3 (4) 2	5	1	-.112
Total			26	-4	-.916
Replicate III (Z arrangement)	**Block no.**	**Yields**	**Block total = B_{zj}**	**$Q_{zj} = B_{.j} - rB_{zj}$**	**$\lambda Q_{zj} - \mu Q_{.j}$**
	c_2	(3) 2 (1) 6	8	6	1.033
	c_1	(2) 3 (5) 3	6	-1	-.012
	c_0	(4) 2 (6) 7	9	0	.124
Total			23	5	1.145

	Unadjusted treatment totals			$B_{.j}$ for Y arrangement	$B_{.j}$ for X arrangement
	(1) 22	(2) 9	———	31	$B_{.0}$ = 16
	(3) 8	———	(4) 7	15	$B_{.1}$ = 42
	———	(5) 8	(6) 20	28	$B_{.2}$ = 16
$B_{.j}$ for Z arrangement	30	17	27	74	74

Table of Q values

Block subscript	Q_{xj}	Q_{yj}	Q_{zj}	Total $Q_{.j}$	λQ_{xj}	λQ_{yj}	λQ_{zj}	$\mu Q_{.j}$
0	-2	-2	0	- 4	-.458	-.458	0	-.124
1	-3	-3	-1	- 7	-.687	-.687	-.229	-.217
2	1	4	6	11	.229	.916	1.374	.341
Total $Q_{i.}$	-4	-1	5	0	-.916	-.229	1.145	.000

Analysis of variance

Source of variation	df	ss	ms
Replicate	2	0.78	0.39
Treatment (ignoring block)	5	76.45	15.29
Error (r. c. b.)	10	4.55	0.455
Block (eliminating treatment)	6	3.89	0.648
Intrablock	4	0.66	0.165
Total	17	81.78	-
Correction for mean	1	304.22	-

Adjusted treatment totals

	(1)	(2)	(3)	(4)	(5)	(6)	Total
Total	22.229	8.542	8.229	6.542	8.229	20.229	74.000
Mean	7.4	2.8	2.7	2.2	2.7	6.7	-

corrected for the difference in the number of degrees of freedom associated with the two variances.

Making use of the supplemental values in the right-hand portion of the table of Q values the adjustments for each incomplete block are computed. Thus, the correction for the b_2 incomplete block is $4(.229) - (11)(.031) = .916 - .341 = .575$. The other adjustments are computed similarly. As a partial check the sum of all the adjustments is equal to zero within rounding errors. To obtain an adjusted treatment total add the $n = 3$ adjustments for the three incomplete blocks in which the treatment occurs to the unadjusted treatment total. For example, the adjusted total for treatment 5 is equal to $8 + .575 - .334 - .012 = 8.229$.

The computational procedure for other rectangular lattice designs follows that for the triple rectangular lattice design. Robinson and Watson [259] give the procedure for covariance analyses in the simple and triple rectangular lattices and the method of analysis when missing data occur. Nair [221] presents the general method of analysis for a simple rectangular lattice design.

XI-4 Three-Dimensional One-Restrictional Lattices

The three-dimensional one-restrictional lattice design with the k^3 treatments in complete replicates may be related to the k^3 factorial experiment with confounding (Chapter IX). The k^3 treatments may be designed in incomplete blocks of size k or of size k^2 depending upon the experimental conditions. The number of possible arrangements in blocks of size k depends upon the value of k. If k is equal to a prime number or power of a prime number, the number of arrangements is $k^2 + k + 1$, and the number of replicates used may be 3, 4, $\cdots$, $k^2 + k + 1$, or any multiple of these numbers. If $k = 6$ or 10, three arrangements are possible. Yates [326], in developing the design for k^3 treatments in three groupings, denoted these arrangements as the X, Y, and Z arrangements. If we designate the k^3 treatments in the same manner as for a factorial arrangement (i.e., combination $a_ib_jc_h$ = treatment ijh, where $i, j, h = 0, 1, \cdots, k - 1$) the X arrangement results in confounding pseudo-effects B, C, and their interaction with incomplete block differences. The Y arrangement is equivalent to confounding A, C, and their interaction, and the Z arrangement confounds A, B, and their interaction. If more replicates are desired, other pseudo-effects may be confounded with incomplete block differences in the remaining replicates. Also, one may repeat the X, Y, and Z arrangements an equal number or an unequal number of times; unequal repetition of the X, Y, and Z arrangements results in an unbalanced arrangement. The construction and analyses for more than three arrangements of a k^3 lattice in blocks of k have been dealt with in general terms by Kempthorne and Federer [177] and in specific terms by Federer [103]. As suggested earlier, a three-dimensional lattice in blocks of k with four replicates may be called a quadruple lattice, with five replicates a quintuple lattice, etc. Another terminology for these designs is to state specifically the block size and number of arrangements for the k^3 lattice in question. The latter scheme is probably the preferred one from an analytical point of view.

XI-4.1 THE THREE-DIMENSIONAL TRIPLE LATTICE (CUBIC LATTICE)

In the cubic lattice design, there are three groupings of the k^3 treatments into incomplete blocks of size k. Three, 6, $\cdots$, $3q$ replicates may be used for the design. The construction and analysis of the design are fully discussed by Yates [326]. Homeyer *et al.* [160] discuss the computational procedure for the cubic lattice with calculating and with punched card machines. Their procedure and notation are utilized in describing the analysis for example XI-6. In the example described by Yates [326], the treatments do not occur in compact replicates even though the theoretical discussion pertains to such a design. The analysis described by Homeyer *et al.* does pertain to a cubic lattice with the treatments in compact replicates.

The randomization procedure follows that for all one-restrictional lattices with complete replicates. Random numbers are assigned to the treatments unless there is a particular reason for not doing so. The groups of treatments are assigned to the incomplete blocks at random, and the treatments within each incomplete block are randomly allotted to the experimental units.

The general form of the analysis of variance for q sets of a cubic lattice design is

Source of variation	df	Mean square	
		Observed	Expected
Replicate	$3q - 1$	—	—
Treatment (ignoring block)	$k^3 - 1$	—	—
Block (eliminating treatment)	$3q(k^2 - 1)$	E_b	$\sigma_\epsilon^2 + \frac{3q-1}{3q}k\sigma_\beta^2$
Block total × replicate	$(3q - 3)(k^2 - 1)$	—	$\sigma_\epsilon^2 + k\sigma_\beta^2$
Component (a)	$3(k - 1)$	—	$\sigma_\epsilon^2 + k\sigma_\beta^2$
" (b)	$3(k - 1)$	—	$\sigma_\epsilon^2 + k\sigma_\beta^2/3$
" (c)	$3(k - 1)^2$	—	$\sigma_\epsilon^2 + 2k\sigma_\beta^2/3$
Intrablock	$(3q - 1)(k^3 - 1) - 3q(k^2 - 1)$	E_e	σ_ϵ^2
Total	$3qk^3 - 1$	—	—

The incomplete block total × replicate sum of squares and the component (*a*) sum of squares are similar to the component (*a*) sum of squares in a two-dimensional triple lattice. The remaining two sums of squares are similar to the component (*b*) sum of squares in a triple lattice experiment.

Example XI-6. The data chosen to illustrate the analytical procedure for a cubic lattice design are the first three replicates of the data in table 1 of the paper by Federer [103]. The experimental layout, individual plot yields, and the block and replicate totals are presented in table XI-22.

(i) Analytical procedure described by Homeyer *et al.* [160] (*k* any integer)

The treatment totals over all replicates are obtained and arranged in the *X*, *Y*, and *Z* arrangements (table XI-23). The corresponding sums of treatment totals in the

TABLE XI-23. Computational sheet for 3^3 treatments in blocks of 3 with 3 replicates

				X and Y arrangements (d)	(a)	(J)	(c_x')
	(000)	(100)	(200)				
	90.7	93.0	95.6	279.3	92.1	3.0	1.185
	(010)	(110)	(210)				
	93.2	92.8	88.3	274.3	88.7	8.2	1.795
	(020)	(120)	(220)				
	90.0	86.1	90.9	267.0	80.5	25.5	3.824
(e)	273.9	271.9	274.8	820.6 (i)	261.3 (A)	36.7 (J)	
(b)	88.3	90.8	93.6	272.7 (B')	-11.4 (A - B')		
(m)	9.0	- 0.5	- 6.0	2.5 (M')	39.2 (J + M')		
(c_y')	- 0.248	- 0.575	- 1.144		0.833 (Y)		
	(001)	(101)	(201)				
	89.9	95.1	88.3	273.3	77.1	42.0	5.475
	(011)	(111)	(211)				
	93.4	94.5	91.9	279.8	91.7	4.7	1.099
	(021)	(121)	(221)				
	94.5	91.8	91.9	278.2	85.6	21.4	3.058
(e)	277.8	281.4	272.1	831.3 (i)	254.4 (A)	68.1 (J)	
(b)	100.5	100.8	89.9	291.2 (B')	-36.8 (A - B')		
(m)	-23.7	-21.0	2.4	- 42.3 (M')	25.8 (J + M')		
(c_y')	- 4.084	- 2.980	- 0.158		0.548 (Y)		
	(002)	(102)	(202)				
	88.0	89.2	90.5	267.7	82.9	19.0	2.883
	(012)	(112)	(212)				
	93.7	88.2	89.8	271.7	90.9	- 1.0	0.537
	(022)	(122)	(222)				
	98.7	87.7	93.0	279.4	98.6	-16.4	-1.270
(e)	280.4	265.1	273.3	818.8 (i)	272.4 (A)	1.6 (J)	
(b)	96.0	84.2	83.0	263.2 (B')	9.2 (A - B')		
(m)	- 7.6	12.5	24.3	29.2 (M')	30.8 (J + M')		
(c_y')	- 2.196	0.950	2.411		0.654 (Y)		
(g)	832.1	818.4	820.2	2470.7	788.1 (A")	106.4 (J")	
(B)	284.8	275.8	266.5	827.1 (B")	-39.0 (A" - B")		
(M)	-22.3	- 9.0	20.7	- 10.6 (M")	95.8 (J" + M")		
(A')	252.1	271.3	264.7	788.1 (A")			
(J')	64.0	11.9	30.5	106.4 (J")			
(B - C')	- 5.6	- 2.1	-20.7	- 28.4 (B" - C")			
(M + L')	-61.4	-24.3	-20.7	-106.4 (M" + L")			
(α)	- 1.304	- 0.516	- 0.440				

TABLE XI-23 (*continued*)

Z arrangement

	(000)	(100)	(200)		
	90.7	93.0	95.6		
	(001)	(101)	(201)		
	89.9	95.1	88.3		
	(002)	(102)	(202)		
	88.0	89.2	90.5		
(f)	268.6	277.3	274.4	820.3 (h)	
(c)	94.0	96.1	96.4	286.5 (C)	34.4 (C - A')
(l)	-13.4	-11.0	-14.8	-39.2 (L)	24.8 (L + J')
(c_z')	- 1.045	- 0.763	- 1.209		0.527 (β)
	(010)	(110)	(210)		
	93.2	92.8	88.3		
	(011)	(111)	(211)		
	93.4	94.5	91.9		
	(012)	(112)	(212)		
	93.7	88.2	89.8		
(f)	280.3	275.5	270.0	825.8 (h)	
(c)	96.4	92.9	92.6	281.9 (C)	10.6 (C - A')
(l)	- 8.9	- 3.2	- 7.8	-19.9 (L)	-8.0 (L + J')
(c_z')	- 1.214	- 0.545	- 1.085		-0.170 (β)
	(020)	(120)	(220)		
	90.0	86.1	90.9		
	(021)	(121)	(221)		
	94.5	91.8	91.9		
	(022)	(122)	(222)		
	98.7	87.7	93.0		
(f)	283.2	265.6	275.8	824.6 (h)	
(c)	100.0	88.9	98.2	287.1 (C)	22.4 (C - A')
(l)	-16.8	- 1.1	-18.8	-36.7 (L)	-6.2 (L + J')
(c_z')	- 2.103	- 0.261	- 2.337		-0.132 (β)
(g)	832.1	818.4	820.2	2470.7	
(C')	290.4	277.9	287.2	855.5 (C")	67.4 (C" - A")
(L')	-39.1	-15.3	-41.4	-95.8 (L")	10.6 (L" + J")

The intrablock error sum of squares is obtained by subtracting the sums of squares for components (*a*), (*b*), and (*c*) from the randomized complete block error sum of squares; thus:

$$304.67 - 85.582 - 33.093 - 107.768 = 78.227,$$

with 52 − 24 = 28 degrees of freedom. The sums of squares are summarized in table XI-24.

TABLE XI-24. Analysis of variance for 3^2 treatments in 3 replicates

Source of variation	df	ss	ms
Replicate	2	84.82	42.41
Treatment (ignoring block)	26	73.25	2.82
Error (r. c. b.)	52	304.67	5.859
Block (eliminating treatment)	24	226.443	9.435
Component (a)	6	85.582	
Component (b)	6	33.093	
Component (c)	12	107.768	
Intrablock	28	78.227	2.794
Total	80	462.74	-
Correction for mean	1	75362.45	-

The estimated weights are

$$w = \frac{1}{E_e} = 0.357910. \tag{XI-61}$$

$$w' = \frac{3q-1}{3qE_b - E_e} = 0.078398. \tag{XI-62}$$

The weighting factors are

$$\lambda = \frac{w - w'}{w + 2w'} = \frac{E_b - E_e}{E_b + E_e}^{1} = 0.54305. \tag{XI-63}$$

$$\mu = \frac{w - w'}{2w + w'} = \frac{E_b - E_e}{2E_b}^{1} = 0.35193. \tag{XI-64}$$

$$\frac{\lambda - \mu}{k^2} = 0.02124. \tag{XI-65}$$

The average standard error of a difference between two adjusted means is equal to

$$\sqrt{\frac{2E_e}{r}\left\{1 + \frac{6\lambda + 3(k-1)\mu}{k^2 + k + 1}\right\}} = \sqrt{\frac{2}{3}(2.794)(1.41307)} = 1.62. \tag{XI-67}$$

The average effective error variance is

$$E_e\left\{1 + \frac{6\lambda + 3(k-1)\mu}{k + k + 1}\right\} = 3.948. \tag{XI-68}$$

[1]Formula for three replicates; $q = 1$.

The efficiency relative to the corresponding randomized complete block is 5.859/3.948 = 148 per cent, or a gain of 48 per cent. The coefficient of variation is $\sqrt{3.948}/2470.7/3(27) = 1.987/30.5 = 6.5$ per cent.

There are three other standard errors for specific comparisons in a cubic lattice; these are

For a pair of treatments with two subscripts in common (e.g. 001 and 002):

$$\sqrt{\frac{2E_e}{r}\left\{1 + \frac{2\lambda}{k^2} + 2(k-1)\frac{\mu}{k^2}\right\}} = 1.54. \tag{XI-69}$$

For a pair of treatments with one subscript in common (e.g. 111 and 001):

$$\sqrt{\frac{2E_e}{r}\left\{1 + \frac{4\lambda}{k^2} + (3k-4)\frac{\mu}{k^2}\right\}} = 1.64. \tag{XI-70}$$

For a pair of treatments with no subscripts in common (e.g. 111 and 222):

$$\sqrt{\frac{2E_e}{r}\left\{1 + \frac{6\lambda}{k^2} + (3k-2)\frac{\mu}{k^2}\right\}} = 1.75. \tag{XI-71}$$

As a rule, these standard errors will not differ materially from the average standard error. Ordinarily then, the average standard error is used for all comparisons.

Since considerable gains over a randomized complete block design were made with this cubic lattice design (two replicates of a cubic lattice are approximately equivalent to three replicates of a randomized complete block design for these data) the treatment means are adjusted for the incomplete block effects. The following quantities are required to obtain the adjustments for the treatment totals:

$$\alpha = \frac{\lambda - \mu}{k^2}(M + L'). \tag{XI-72}$$

$$\beta = \frac{\lambda - \mu}{k^2}(L + J'). \tag{XI-73}$$

$$\gamma = \frac{\lambda - \mu}{k^2}(J + M') \tag{XI-74}$$

$$c_x' = \frac{\mu}{k}j + \gamma. \tag{XI-75}$$

$$c_y' = \frac{\mu}{k}m + \alpha. \tag{XI-76}$$

$$c_z' = \frac{\mu}{k}l + \beta. \tag{XI-77}$$

In table XI-23, one c_x' value is computed for each j value, one c_y' value for each m value, and one c_z' value for each l value. The sum of the c_x', c_y', and c_z' values equals zero within rounding errors. To obtain the adjusted treatment total, add the corresponding c_x', c_y', and c_z' value from table XI-23 to the unadjusted total; thus, for treatment 101 the adjusted total is equal to $95.1 + 5.475 - 2.980 - 0.763 = 96.832$;

the adjusted mean is 96.832/3 = 32.28. The other adjusted totals and means (table XI-25) are similarly obtained.

TABLE XI-25. Unadjusted and adjusted treatment totals and adjusted means for 3^3 treatments in 3 replicates

Treatment number	Totals Unadj.	Totals Adj.	Mean adj.	Treatment number	Totals Unadj.	Totals Adj.	Mean adj.
000	90.7	90.592	30.20	112	88.2	89.142	29.71
001	89.9	90.246	30.08	120	86.1	89.088	29.70
002	88.0	87.642	29.21	121	91.8	91.617	30.54
010	93.2	93.533	31.18	122	87.7	87.119	29.04
011	93.4	89.201	29.73	200	95.6	94.432	31.48
012	93.7	90.827	30.28	201	88.3	92.408	30.80
020	90.0	91.473	30.49	202	90.5	94.585	31.53
021	94.5	91.371	30.46	210	88.3	87.866	29.29
022	98.7	93.131	31.04	211	91.9	91.756	30.59
100	93.0	92.847	30.95	212	89.8	91.663	30.55
101	95.1	96.832	32.28	220	90.9	91.243	30.41
102	89.2	92.270	30.76	221	91.9	92.463	30.82
110	92.8	93.475	31.16	222	93.0	91.804	30.60
111	94.5	92.074	30.69	Total	2470.7	2470.700	-

(ii) Analysis as a factorial (k = a prime number or power of a prime number)

The analysis of a three-dimensional lattice design in blocks of k as a factorial experiment follows immediately from the results in Chapters VII and IX. The various steps are described in detail by Federer [103] and are sketched below. From the data of table XI-22 the levels of each pseudo-effect are obtained in each replicate (table XI-26).[1] The next step is to compute the sum of squares among levels of effects in each replicate (table XI-27). The sum of the sums of squares for effects within replicates, 377.96, is equal to the within replicate sum of squares, 462.74 − 84.82 = 377.92, within rounding errors.

The component (a) sum of squares is obtained by subtracting the sum of squares for levels of effects in the replicates in which the effect is confounded from the within-replicate sum of squares for these replicates. For example, the interaction sum of squares of levels of effect A with replicates I and II is

$$9.37 + 18.61 - \left\{\frac{(290.4 + 284.8)^2 + (277.9 + 275.8)^2 + (287.2 + 266.5)^2}{2k = 18}\right.$$
$$\left. - \frac{(855.5 + 827.1)^2}{2k^3 = 54}\right\} = 9.37 + 18.61 - 17.12 = 10.86.$$

The interaction sums of squares for B and C are computed similarly.

The component (b) sum of squares represents the contrast of the level of the effect

[1]It is not necessary to compute all effects unless one wishes to compute the treatment (eliminating block) sum of squares.

TABLE XI-26. Totals per level of effect in each replicate and weighted levels

Effect	Replicate I[a] Level of effect 0	1	2	Sum	Replicate II[a] Level of effect 0	1	2	Sum	Replicate III[a] Level of effect 0	1	2	Sum	Weighted levels of effects 0	1	2
A	(290.4)	(277.9)	(287.2)	855.5	(284.8)	(275.8)	(266.5)	827.1	256.9	264.7	266.5	788.1	266.252	268.401	269.653
B	(286.5)	(281.9)	(287.1)	855.5	281.7	272.6	272.8	827.1	(252.1)	(271.3)	(264.7)	788.1	277.923	273.819	273.744
AB	(275.5)	(290.7)	(289.3)	855.5	274.1	276.7	276.3	827.1	254.6	266.0	267.5	788.1	265.451	273.260	273.618
AB^2	(285.1)	(288.7)	(281.7)	855.5	272.7	277.3	277.1	827.1	262.1	264.5	261.5	788.1	269.147	272.657	270.524
C	286.6	285.7	283.2	855.5	(272.7)	(291.2)	(263.2)	827.1	(261.3)	(254.4)	(272.4)	788.1	280.629	281.770	278.509
AC	283.0	285.1	287.4	855.5	(262.4)	(274.3)	(290.4)	827.1	265.7	263.6	258.8	788.1	273.170	274.345	274.808
AC^2	289.2	287.6	278.7	855.5	(272.1)	(276.7)	(278.3)	827.1	267.3	260.1	260.7	788.1	277.643	274.131	270.549
BC	284.8	289.7	281.0	855.5	275.8	272.9	278.4	827.1	(268.6)	(264.4)	(255.1)	788.1	279.145	279.632	277.272
BC^2	283.8	287.7	284.0	855.5	272.3	275.3	279.5	827.1	(282.4)	(257.2)	(248.5)	788.1	278.479	279.101	278.468
ABC	287.5	290.3	277.7	855.5	271.0	272.3	283.8	827.1	259.8	265.5	262.8	788.1	272.767	276.033	274.767
ABC^2	291.2	281.0	283.3	855.5	272.4	276.0	278.7	827.1	259.3	262.9	265.9	788.1	274.300	273.300	275.967
AB^2C	279.3	290.3	285.9	855.5	278.1	270.6	278.4	827.1	268.2	258.6	261.3	788.1	275.200	273.167	275.200
AB^2C^2	283.5	282.1	289.9	855.5	274.8	278.0	274.3	827.1	269.5	253.9	264.7	788.1	275.933	271.333	276.300

[a]Parentheses signify the effect confounded with incomplete block differences

TABLE XI-27. Sums of squares for the effects within each of the 3 replicates and for components (a), (b), and (c)

Replicate	Effect: A	B	AB	AB^2	C	AC	AC^2	BC	BC^2	ABC	ABC^2	AB^2C	AB^2C^2	Total
I	9.37	1.80	15.68	2.72	0.69	1.08	7.11	4.23	1.07	9.73	6.36	6.81	3.84	
II	18.61	6.00	0.44	1.50	45.06	43.88	2.30	1.68	2.91	11.03	2.22	4.34	0.90	
III	5.79	21.15	11.06	0.56	18.33	2.78	3.55	10.61	68.89	1.81	2.43	5.45	14.19	377.96

Component (a) sum of squares

(Interaction of levels of an effect with replicates in which it is confounded)

	Sum of squares	df
A	9.37 + 18.61 - 17.12 = 10.86	2
B	1.80 + 21.15 - 7.21 = 15.74	2
C	45.06 + 18.33 - 4.39 = 59.00	2
Sum	85.60	6

Component (b) sum of squares

(Comparison by levels for mean unconfounded effect in 2 replicates and mean confounded effect in other replicates)

	Sum of squares	df
A	69.814 + 10.935 + 7.935 - 69.882 = 18.802	2
B	11.390 + 1.185 + 0.712 - 0.694 = 12.593	2
C	28.456 + 12.327 + 17.567 - 56.652 = 1.698	2
Sum	33.093	6

Component (c) sum of squares

(Comparison by levels of mean unconfounded effect in 2 replicates with effect confounded in only one replicate)

	Sum of squares	df
AB	9.209 + 27.735 + 22.427 - 56.652 = 2.719	2
AB^2	23.207 + 23.470 + 11.390 - 56.652 = 1.415	2
AC	10.578 + 0.000 + 22.170 - 0.694 = 32.054	2
AC^2	2.802 + 0.602 + 5.479 - 0.694 = 8.189	2
BC	10.140 + 21.156 + 44.827 - 69.882 = 6.241	2
BC^2	1.402 + 43.740 + 81.894 - 69.882 = 57.154	2
Sum	107.772	12

in the replicate in which the effect is unconfounded with the level of the effect in the other two replicates. For example, the sum of squares for this contrast for pseudo-effect A is

$$\frac{[290.4 + 284.8 - 2(256.9)]^2 + [277.9 + 275.8 - 2(264.7)]^2}{k\,(1 + 1 + 4) = 6k^2 = 54}$$

$$+ \frac{[287.2 + 266.5 - 2(266.5)]^2}{54} - \frac{[855.5 + 827.1 - 2(788.1)]^2}{6k^3 = 162}$$

$$= 69.814 + 10.935 + 7.935 - 69.882 = 18.802.$$

The component (c) sum of squares is obtained from similar computations. The various sums of squares agree, within rounding errors, with those obtained previously (table XI-24). The standard errors are computed as before.

To obtain the adjusted totals, it is first necessary to complete table XI-26. The weighted levels of the effects are obtained by weighting the level of an effect inversely to the variance with which it is estimated. The weight for a level of an effect in a replicate in which the effect is unconfounded with block differences is w; the weight for the effect in the replicate in which the effect is confounded is w'. The weighted value for effect $(A)_0$ is

$$\frac{[(A)_{10} + (A)_{20}]w' + w(A)_{30}}{2w' + w}$$

$$= \frac{(290.4 + 284.8)(.078398) + .357910(256.9)}{.514706} = 266.252. \qquad \text{(XI-78)}$$

The other weighted levels of the pseudo-effects are obtained in the same manner. The adjusted totals are obtained in the manner described in Chapter IX. The formula for obtaining the adjusted treatment total for k^3 treatments is

$$\begin{aligned} X_{\cdot ijh}' = \frac{r}{k^2}\Big\{&(A)_i + (B)_j + (AB)_{i+j} + (AB^2)_{i+2j} + (C)_h + (AC)_{i+h} + (AC^2)_{i+2h} \\ &+ (BC)_{j+h} + (BC^2)_{j+2h} + (ABC)_{i+j+h} + (ABC^2)_{i+j+2h} + (AB^2C)_{i+2j+h} \\ &+ (AB^2C^2)_{i+2j+2h}\Big\} - rk(k+1)\bar{x}, \end{aligned} \qquad \text{(XI-79)}$$

where $\bar{x}$ = the experimental mean. The levels of the effects are those in the right-hand portion of table XI-26. For example, the adjusted total for treatment 101 is equal to $\frac{3}{9}(268.401 + 277.923 + 273.260 + 272.657 + 281.770 + 274.808 + 277.643 + 279.632 + 278.468 + 274.767 + 274.300 + 275.200 + 275.933) - 36(2470.7)/3(27) = 96.832$. The remaining adjusted totals are obtained in the same manner. To facilitate the calculation of adjusted totals, table XI-28 has been prepared. The levels of the effects are indicated for each treatment. (Table XI-28 is useful for all 3^3 lattices regardless of the scheme of confounding.) The figure $36\bar{x}$ $\left(= \dfrac{36(2470.7)}{3(27)} = 1098.0889\right)$ is subtracted from r/k^2 = one-third of the sum of the appropriate levels of the effects to obtain the last column, the adjusted totals, in table XI-28.

The sum of squares for treatment (eliminating block effect) may be obtained as described in Chapter IX; i.e., the effects are computed only from the replicates in which they are unconfounded with incomplete block differences. The blocks-within-

TABLE XI-28. Levels of weighted effects from table XI-26 associated with any treatment; adjusted totals ($36\bar{x} = 1098.0889$)

Treat-ment	$(A)_i$	$(B)_j$	$(AB)_{i+j}$	$(AB^2)_{i+2j}$	$(C)_h$	$(AC)_{i+h}$	$(AC^2)_{i+2h}$	$(BC)_{j+h}$	$(BC^2)_{j+2h}$	$(ABC)_{i+j+h}$	$(ABC^2)_{i+j+2h}$	$(AB^2C)_{i+2j+h}$	$(AB^2C^2)_{i+2j+2h}$	Adjusted total
000	0	0	0	0	0	0	0	0	0	0	0	0	0	90.591
001	0	0	0	0	1	1	2	1	2	1	2	1	2	90.246
002	0	0	0	0	2	2	1	2	1	2	1	2	1	87.642
010	0	1	1	2	0	0	0	1	1	1	1	2	2	93.532
011	0	1	1	2	1	1	2	2	0	2	0	0	1	89.201
012	0	1	1	2	2	2	1	0	2	0	2	1	0	90.828
020	0	2	2	1	0	0	0	2	2	2	2	1	1	91.473
021	0	2	2	1	1	1	2	0	1	0	1	2	0	91.371
022	0	2	2	1	2	2	1	1	0	1	0	0	2	93.132
100	1	0	1	1	0	1	1	0	0	1	1	1	1	92.845
101	1	0	1	1	1	2	0	1	2	2	0	2	0	96.832
102	1	0	1	1	2	0	2	2	1	0	2	0	2	92.270
110	1	1	2	0	0	1	1	1	1	2	2	0	0	93.474
111	1	1	2	0	1	2	0	2	0	0	1	1	2	92.075
112	1	1	2	0	2	0	2	0	2	1	0	2	1	89.142
120	1	2	0	2	0	1	1	2	2	0	0	2	2	89.088
121	1	2	0	2	1	2	0	0	1	1	2	0	1	91.618
122	1	2	0	2	2	0	2	1	0	2	1	1	0	87.120
200	2	0	2	2	0	2	2	0	0	2	2	2	2	94.432
201	2	0	2	2	1	0	1	1	2	0	1	0	1	92.407
202	2	0	2	2	2	1	0	2	1	1	0	1	0	94.585
210	2	1	0	1	0	2	2	1	1	0	0	1	1	87.866
211	2	1	0	1	1	0	1	2	0	1	2	2	0	91.756
212	2	1	0	1	2	1	0	0	2	2	1	0	2	91.663
220	2	2	1	0	0	2	2	2	2	1	1	0	0	91.243
221	2	2	1	0	1	0	1	0	1	2	0	1	2	92.463
222	2	2	1	0	2	1	0	1	0	0	2	2	1	91.804

replicates = block (ignoring treatment effect) sum of squares is obtained as the sum of squares among incomplete block totals within replicates. Other methods for computing the treatment (eliminating block effect) are discussed by Kempthorne [175], Nair [221], and Rao [255].

The intrablock error sum of squares may be obtained directly as the sum of the interaction sums of squares of levels of the pseudo-effects with the replicates in which they are unconfounded with incomplete block differences. For the 3^3 cubic lattice with three replicates the sum of the degrees of freedom for the interaction sums of squares for levels of main pseudo-effects with replicates is equal to zero, for levels of the two-factor interactions and replicates = 6(2), and for levels of the three-factor interactions and replicates = $4(3 - 1)(3 - 1) = 16$. Therefore, the total number of degrees of freedom associated with the intrablock error sum of squares is

$$0 + 12 + 16 = 28.$$

(iii) Rao's general method of analysis

Some additional concepts of association among varieties in the incomplete blocks of a cubic lattice design are necessary in order to set up the various systems of parameters in accordance with Rao's [255] general method of analysis. The purpose of this section is to set forth these concepts and to give the parameters peculiar to the cubic lattice. There are two major kinds of association among treatments in the incomplete blocks; viz., those treatments associated together in an incomplete block, or first associates, and those not associated in an incomplete block, or zeroth associates. In three-dimensional designs, there may be two or more kinds of first associates as well as two or more kinds of zeroth associates. In the cubic lattice design, there are two kinds of zeroth associates and one kind of first associates. Any specific treatment will have $3(k - 1)$ first associates; i.e., any treatment appears with $n_1 = 3(k - 1)$ other treatments in the three incomplete blocks in which it appears (figure XI-1). Consequently then, there are $k^3 - 1 - 3(k - 1) = k^3 - 3k + 2$ zeroth associates or $k^3 - 3k + 2$ treatments with which the particular treatment is not associated in one or another of the incomplete blocks. One set of the zeroth associates is connected to a particular treatment through association in incomplete blocks with the first associates of the treatment. There are $n_{01} = 3(k - 1)^2$ zeroth associates of this nature. The remainder of the zeroth associates are not associated with the treatment in any way other than that the different incomplete blocks in which they occur are in the same replicate. There are $n_{00} = k^3 - 3k + 2 - n_{01} = (k - 1)^3$ zeroth associates of this kind.

For the cubic lattice design the first system of parameters is

$$\left.\begin{array}{ll}\text{zeroth associates (first type):} & \lambda_{00} = 0;\ n_{00} = (k - 1)^3;\\ \text{zeroth associates (second type):} & \lambda_{01} = 0;\ n_{01} = 3(k - 1)^2;\\ \text{first associates (only one type):} & \lambda_1 = 1;\ \ n_1 = 3(k - 1);\\ \multicolumn{2}{l}{r = 3;\ v = k^3;\ b = 3k^2;\ k = \text{number of units per incomplete block.}}\end{array}\right\} \quad \text{(XI-80)}$$

The second system of parameters for a cubic lattice design has to do with the number of treatments in common for the various types of association. For example, a given pair of treatments may have no subscript in a given position in common and consequently no common section on the cube, as pictured in figure XI-1. The second

system of parameters describing the types of association among a pair of treatments with no subscript in a given position in common, say 000 and 111, is

$$p_{ij}^{00} = \begin{pmatrix} (k-2)^3 & 3(k-2)^2 & 3(k-2) \\ 3(k-2)^2 & 6(k-2) & 3 \\ 3(k-2) & 3 & 0 \end{pmatrix}. \tag{XI-81}$$

The second system of parameters describing the association among a pair of treatments with one subscript in a given position in common, say 000 and 011, is

$$p_{ij}^{01} = \begin{pmatrix} (k-1)(k-2)^2 & 2(k-1)(k-2) & (k-1) \\ 2(k-1)(k-2) & (k-2)^2 + 2(k-1) & 2(k-2) \\ (k-1) & 2(k-2) & 2 \end{pmatrix}. \tag{XI-82}$$

The types of association among associates of a pair of treatments which are first associates (i.e., compared together in one of the incomplete blocks, or which have two subscripts in common and consequently two common sections on a cube, say treatment 000 and treatment 001) are

$$p_{ij}^{1} = \begin{pmatrix} (k-2)(k-1)^2 & (k-1)^2 & 0 \\ (k-1)^2 & 2(k-2)(k-1) & 2(k-1) \\ 0 & 2(k-1) & (k-2) \end{pmatrix}. \tag{XI-83}$$

From the first and second system of parameters described above, it is possible to compute the various constants required for the analysis of a cubic lattice design without recovery of interblock information. Because of the misprints associated with B_{23} and B_{33} in Rao's original article the formulae for these two quantities are given below. Rao's subscripts on the A's, B's, and C's are retained. The constants in reduced form are

$$\left.\begin{array}{lll} A_{13} = 3k-2; & B_{13} = 1; & C_{13} = 1; \\ A_{23} = 0; & A_{33} = 2(k-1); & \\ \multicolumn{3}{l}{B_{23} = 3(k-1) + \lambda_1 + (\lambda_1 - \lambda_{00})(p_{11}^{00} - p_{11}^{1}) + (\lambda_1 - \lambda_{01})(p_{12}^{00} - p_{12}^{1}) = 3;} \\ \multicolumn{3}{l}{B_{33} = (\lambda_1 - \lambda_{00})(p_{12}^{00} - p_{12}^{1}) + (\lambda_1 - \lambda_{01})(p_{22}^{00} - p_{22}^{1}) = 2k-5;} \\ C_{23} = -(k-1); & C_{33} = 3k-1; & \Delta = 6k^3; \\ F = 2(k^2+k+1); & G = -(k+4); & H = -(k+2). \end{array}\right\} \tag{XI-84}$$

The variances (without recovery of interblock information) of the estimated differences of the different types are

$$V(000\text{-}111) = \frac{\sigma^2(2k^2+3k+6)}{3k} \text{ (zeroth associates of the first type).} \tag{XI-85}$$

$$V(000\text{-}011) = \frac{\sigma^2(2k^2+3k+4)}{3k^2} \text{ (zeroth associates of the second type).} \tag{XI-86}$$

$$V(000\text{-}001) = \frac{2\sigma^2(k^2+k+1)}{3k^2} = \frac{2kF\sigma^2}{\Delta} \text{ (first associates).} \tag{XI-87}$$

The average variance of a mean difference is equal to

$$\frac{\sigma^2}{3}\left\{\frac{2k^2 + 5k + 11}{k^2 + k + 1}\right\}. \tag{XI-88}$$

In an experiment, σ^2 is replaced by E_e, the estimated intrablock error variance. For q repetitions of the basic set of three groups, each of the above variances is multiplied by the factor $1/q$.

The constants required for the analysis of a cubic lattice design with recovery of interblock information are

$$\left.\begin{aligned}
R &= 3w + \frac{3w'}{k-1};\ \Lambda_{00} = 0; \quad \Lambda_{01} = 0; \quad \Lambda_1 = w - w';\\
A_{13}' &= (3k-2)w + 2w'; \qquad A_{23}' = 0;\\
A_{33}' &= 2(k-1)(w-w'); \qquad B_{23}' = 3[w + w'(k-1)];\\
B_{33}' &= (w-w')(2k-5); \qquad C_{23}' = -(k-1)(w-w');\\
C_{33}' &= (3k-1)w + w';\\
F' &= 9k^2ww' + 2(w-w')[w(k^2+k+1) - w'(k-1)^2];\\
G' &= -(w-w')[(k+4)w + 2w'(k-2)];\\
H' &= -(w-w')[w(k+2) + 2w'(k-1)];\\
\Delta' &= 3k^3w[2w^2 + 5ww' + 2w'^2].
\end{aligned}\right\} \tag{XI-89}$$

The relative intrablock and interblock weights are w and w', respectively, and are obtained from the cubic lattice experiment. The values R and Λ_{ij} replace r and λ_{ij} in equation (XI-84) to obtain the A_{ij}', B_{ij}', C_{ij}', F', G', and H' values. The variances of the differences of adjusted means when interblock information is utilized are equal to

$$V(000\text{-}111) = 2k\sigma^2(F' - G')/\Delta', \tag{XI-90}$$

$$V(000\text{-}011) = 2k\sigma^2(F' - H')/\Delta', \tag{XI-91}$$

and

$$V(000\text{-}001) = 2k\sigma^2F'/\Delta'. \tag{XI-92}$$

Nair [221] describes the adjustments of the treatment means both with and without recovery of interblock information.

XI-4.2 THREE-DIMENSIONAL LATTICES WITH MORE THAN THREE ARRANGEMENTS (k = A PRIME NUMBER OR POWER OF A PRIME NUMBER)

The design and analysis for four or more arrangements of k^3 treatments in incomplete blocks of k treatments has been described by Kempthorne and Federer [177] and by Federer [103]. Since k must be a prime number or power of a prime number, the method described in examples XI-1-(ii) and XI-6-(ii), analysis as a factorial, may be used. The k^3 treatments are likened to the k^3 combinations in a factorial experiment, and groups of treatments are set equal to levels of the various effects. Various effects are confounded with incomplete blocks in various arrangements. In order to obtain balanced confounding on all $k^2 + k + 1$ effects, it is necessary to have $k^2 + k + 1$ arrangements (table XI-2 and section XI-2). A partially balanced incomplete block design is obtained by selecting a subset of the $k^2 + k + 1$ arrangements. For three arrangements, any three of the $k^2 + k + 1$ arrangements may be selected. For four or more arrangements, it is necessary to set up a criterion

for the best selection of arrangements. The criterion used here is that all effects are confounded in as nearly an equal number of arrangements as is possible. There is no unique order for the arrangements, but the following order for 3^3 treatments is best for most conditions and criteria:

Arrangement	Effects confounded			
1	A,	B,	AB,	AB^2
2	A,	C,	AC,	AC^2
3	B,	C,	BC,	BC^2
4	AB,	AC,	BC^2,	AB^2C^2
5	AB^2,	AC^2,	BC^2,	ABC
6	AB^2,	AC,	BC,	ABC^2
7	AB,	AC^2,	BC,	AB^2C
8	AB^2,	C,	AB^2C,	AB^2C^2
9	AB,	C,	ABC,	ABC^2
10	A,	BC,	ABC,	AB^2C^2
11	B,	AC,	ABC,	AB^2C
12	B,	AC^2,	ABC^2,	AB^2C^2
13	A,	BC^2,	ABC^2,	AB^2C

If three arrangements are desired, the first three are selected; if four arrangements are desired, the first four are selected; if five arrangements are desired, the first five are selected; etc. This allows the use of any number of replicates, whereas the design in section XI-4.1 requires $3q$ replicates.

A detailed example is given by Federer [103] for $k^3 = 27$ treatments in incomplete blocks of $k = 3$ and for $r = 4$ replicates. The procedure is generalized and may be extended for any number of arrangements.

XI-5 *n*-Dimensional One-Restrictional Designs

The general theory for prime-power lattice designs for k^n treatments in incomplete blocks of size k^s ($s < n$) is given by Kempthorne and Federer [177, 178]. A few of these designs are discussed below. The randomization procedure follows that for the preceding designs. The treatments are randomly assigned to the treatment numbers. The groups of k^s treatments are randomly allotted to the k^{n-s} incomplete blocks. Then, the k^s treatments in each incomplete block are randomly assigned to the experimental units.

XI-5.1 k^4 TREATMENTS IN BLOCKS OF k^s ($s < 4$)

Four-dimensional one-restrictional lattices may be designed in incomplete blocks of size k^3, k^2, or k with the incomplete blocks forming a complete replicate. The number of arrangements possible depends upon the value of k. If incomplete blocks of size k^2 are used, the design falls into the class of two-dimensional one-restrictional lattices. If incomplete blocks of size k or of size k^3 are used, $k^3 + k^2 + k + 1$ arrangements are possible for k a prime number. For $k = 6$ and 10, at least four arrangements are possible.

XI-5.1.1 k^4 treatments in blocks of k. The general treatment for k^4 treatments in incomplete blocks of size k with the treatments forming a

complete block with four or more replicates has been discussed by Federer [105]. Numerical examples for $k = 3$ and $r = 4$ and 6 are presented. The analysis follows that described in Chapter IX for confounded factorial experiments. If four replicates are to be used, one system of confounding of the pseudo-effects is

Replicate I —A, B, C, and their interactions,
Replicate II —A, B, D, " " " ,
Replicate III—A, C, D, " " " ,
and Replicate IV—B, C, D, " " " .

The key-out of the degrees of freedom in the analysis of variance is

Source of variation	df	ms	Expected value of ms
Replicate	3	—	—
Treatment (ignoring block effect)	$(k^4 - 1)$	—	—
Block (elim. treatment effect)	$4(k^3 - 1)$	E_b	$\sigma_\epsilon^2 + 3k\sigma_\beta^2/4$
Component (a)	$6k^2 - 4k - 2$	—	$\sigma_\epsilon^2 + k\sigma_\beta^2$
" (b)	$4(k - 1)$	—	$\sigma_\epsilon^2 + k\sigma_\beta^2/4$
" (c)	$6(k - 1)^2$	—	$\sigma_\epsilon^2 + k\sigma_\beta^2/2$
" (d)	$4(k - 1)^3$	—	$\sigma_\epsilon^2 + 3k\sigma_\beta^2/4$
Intrablock error	$3(k^4 - 1) - 4(k^3 - 1)$	E_e	σ_ϵ^2
Total	$4k^4 - 1$		

The estimated weights are $w = 1/E_e$ and $w' = 3/(4E_b - E_e)$. Four standard errors [105] are available for comparisons of various pairs of treatments. For $k = 3$ and $r = 4$ the average standard error of the difference between two adjusted means (see formula XI-2) is

$$\sqrt{\frac{1}{20}\left\{\frac{4}{w + 3w'} + \frac{12}{2w + 2w'} + \frac{16}{3w + w'} + \frac{8}{4w}\right\}}. \qquad \text{(XI-93)}$$

Four effects are confounded in three replicates; twelve effects are confounded in two of the four replicates; sixteen effects are confounded in one of the four replicates; and eight effects are unconfounded with incomplete block differences in any of the four replicates. The latter eight effects are subject to a variance equal to $1/4w = E_e/4$. The adjusted totals are obtained in the same manner as for confounded factorials with recovery of interblock information [Chapter IX; 105].

XI-5.1.2 k^4 treatments in blocks of k^3. The class of four-dimensional one-restrictional lattices in incomplete blocks of size k^3 is not very important from a practical standpoint, since the block size becomes large for $k > 2$. For $k = 2$, reference is made to Chapter IX for the design and analysis.

XI-5.2 k^5 TREATMENTS IN BLOCKS OF k^s ($s < 5$)

XI-5.2.1 k^5 treatments in blocks of k. The design and analysis of five-dimensional one-restrictional lattices with an incomplete block of size k follow that outlined in section XI-5.1. The designs of practical importance are for $k = 2$ and 3. An illustration for $k = 3$ is provided by Kempthorne and Federer [177]. The minimum number of replicates necessary to obtain intrablock information on all comparisons is the integer equal to or next larger than the ratio n/s; thus, $n/s = 5/1 = 5$ for the present lattice.

XI-5.2.2 k^5 treatments in blocks of k^2. The lattices of this group are discussed in detail by Federer and Robson [114]. A numerical example with $k = 2$, systems of confounding for $k = 2$ and 3, experimental arrangements for $k = 2$ and 3, and the general analyses are discussed in their paper. For $k = 2$ and 3, confounding systems required for balanced arrangements are investigated along with the types of confounding leading to designs approaching equalization of confounding on the various treatment contrasts. The minimum number of replicates necessary to obtain intrablock information on all treatment contrasts is $n/s = 5/2 = 2.5$ or 3 replicates.

XI-5.2.3 k^5 treatments in blocks of k^3. A specific example has been chosen to illustrate the design and analysis for a five-dimensional one-restrictional lattice design in incomplete blocks of size k^3. It was desired to compare thirty-two treatments in a greenhouse experiment. The available number of greenhouse pots was $4(32) = 128$. The experimenter (R. E. Clark, Cornell University) suspected that the variation among complete blocks would be larger than the within-block variation for some of the characters under study. Furthermore, for one of the important characteristics it was impractical to observe more than eight experimental units per day. He suspected that the day-to-day variation might be rather large in comparison with the within-day variation. For these particular conditions an incomplete block design of eight experimental units with the treatments in complete replicates was indicated. Since all treatment contrasts were of equal interest, the thirty-two treatments were randomly assigned to the treatment numbers for a 2^5 factorial; i.e., $ijhgf$ for $i, j, h, g, f = 0$ or 1. The four arrangements given in table XI-29 were then constructed. The $2^2 = 4$ groups of eight treatments each in each replicate were assigned to the four incomplete blocks (position and day) at random, and then the $8 = 2^3$ treatments within a group were randomly assigned to the eight greenhouse pots.

The key-out of the degrees of freedom and the expectation of the mean squares are given in table XI-29. The total, replicate, and treatment (ignoring block) sums of squares are computed in the usual manner for a randomized complete block analysis. The randomized complete block error sum of squares is partitioned into the two parts: block (eliminating treatment effect) and intrablock. Both sums of squares may be computed directly, although the

intrablock error sum of squares usually is obtained by subtraction. The block (eliminating treatment) sum of squares is the sum of the twelve sums of squares, each with a single degree of freedom, for the contrast of the mean level of an effect in the replicate in which the effect is confounded with the mean level of the effect in the replicates in which the effect is unconfounded. For example, A is confounded with incomplete block differences in group 1 but

TABLE XI-29. Four arrangements for 2^5 treatments in blocks of 2^3

block no.	Treatments in the incomplete block							
	Group 1 (A, B, AB confounded)							
1	00000	00001	00010	00011	00100	00101	00110	00111
2	01000	01001	01010	01011	01100	01101	01110	01111
3	10000	10001	10010	10011	10100	10101	10110	10111
4	11000	11001	11010	11011	11100	11101	11110	11111
	Group 2 (C, D, CD confounded)							
5	00000	00001	01000	01001	10000	10001	11000	11001
6	00010	00011	01010	01011	10010	10011	11010	11011
7	00100	00101	01100	01101	10100	10101	11100	11101
8	00110	00111	01110	01111	10110	10111	11110	11111
	Group 3 (E, AC, ACE confounded)							
9	00000	00010	01000	01010	10100	10110	11100	11110
10	00001	00011	01001	01011	10101	10111	11101	11111
11	00101	00111	01101	01111	10001	10011	11001	11011
12	00100	00110	01100	01110	10000	10010	11000	11010
	Group 4 (BD, AE, ABDE confounded)							
13	00000	00100	01010	01110	10001	10101	11011	11111
14	00001	00101	01011	01111	10000	10100	11010	11110
15	00010	00110	01000	01100	10011	10111	11001	11101
16	00011	00111	01001	01101	10010	10110	11000	11100

Analysis of variance

Source of variation	df	ms Observed	ms Expected
Replicate	3	-	-
Treatment (ignoring block)	31	-	-
Block (eliminating treatment)	12	E_b	$\sigma_\epsilon^2 + 3(8)\sigma_\beta^2/4$
Intrablock	81	E_e	σ_ϵ^2
Total	127		

is unconfounded in the other three groups; the sum of squares for this contrast is equal to

$$\frac{[3(A)_{01} - (A)_{02} - (A)_{03} - (A)_{04}]^2 + [3(A)_{11} - (A)_{12} - (A)_{13} - (A)_{14}]^2}{16(9+1+1+1) = 192}$$
$$- \frac{[3((A)_{01} + (A)_{11}) - ((A)_{02} + (A)_{12}) - ((A)_{03} + (A)_{13}) - ((A)_{04} + (A)_{14})]^2}{32(12) = 384}, \tag{XI-94}$$

with a single degree of freedom. Similar formulae are used to compute the sums of squares for this contrast for levels of B, AB, C, D, CD, E, AC, ACE, BD, AE, and $ABDE$, each with a single degree of freedom. These twelve individual sums of squares are summed to obtain the block (eliminating treatment) sum of squares. Also, if one computes the treatment (eliminating block) sum of squares and if one obtains the difference between this sum of squares and the treatment (ignoring block) sum of squares, the resulting difference may be subtracted from the blocks-within-replicate sum of squares to obtain the block (eliminating treatment) sum of squares.

Another method of analysis for this design involves setting up a table of totals in the four arrangements given in table XI-29. The comparisons of the total of treatment totals with $r = 4$ times the block total for the same eight treatments for all sixteen groupings corrected for the mean of the replicate yields the block (eliminating treatment) sum of squares with $3 + 3 + 3 + 3 = 12$ degrees of freedom.

The average standard error of a difference for a pair of adjusted treatment means is equal to

$$\sqrt{\frac{2}{31}\left\{\frac{12}{3w + w'} + \frac{19}{4w}\right\}}, \tag{XI-95}$$

where $w = 1/E_e$ and $w' = 3/(4E_b - E_e)$.

The adjusted treatment means are obtained from the weighted pseudo-effects in much the same manner as described for previous lattices.

XI-6 Missing Data

Analyses for lattice designs with one or more missing experimental units, blocks, or treatments have not been worked out for the majority of the designs except in very general terms. Three methods of analyses have been developed for handling lattice designs with incomplete data; these are

(i) Estimate the missing plot formula by least squares [60, 65, 259].
(ii) Insert a one for the missing experimental unit and zeros elsewhere, and conduct a covariance analysis [255].
(iii) Use the general method of analysis suitable for unequal numbers [175].

The above methods minimize the intrablock error sum of squares but do not consider the interblock sum of squares. The missing plot formula in (i) is

similar to that for the designs with partial confounding of the effects (Chapter IX).

For estimating the value of a missing experimental unit in certain lattice designs, Cornish [65, 68] developed a formula which minimizes the weighted sum of the intrablock and the interblock error sums of squares (formula (XI-104)). The formula for a missing experimental unit is complex but may be simplified in two special cases. If $E_b \leq E_e$ the formula suitable for a randomized complete block may be used. If E_b is considerably larger than E_e, then the formula utilizing only intrablock information is suitable, since w' will be small compared to w. For values of E_b in between these extremes the formulae developed by Cornish or similar ones should be used in the analysis of lattice experiments with incomplete data.

Formulae for a missing experimental unit are given for the simple, triple, and balanced lattices by Cochran and Cox [60, p. 279, 290, 292] and for the simple and triple rectangular lattices by Robinson and Watson [259]. These formulae estimate values which minimize the intrablock error sum of squares. Illustrative examples of the use of these formulae are given in the above references. Healy [153] gives the analysis for lattice designs when one variety is missing.

If formulae for missing data are unavailable, an approximate procedure is to compute the estimated value in the same manner as for a randomized complete block design. Then make an adjustment to this value, taking into account the treatments in the block and the block deviation from the replicate mean. With a little practice an experimenter is able to guess-estimate values which result in little deviation of E_b and E_e from the correct values [322, p. 446].

XI-7 Tests of Significance

Tests of significance for lattice designs are in a less advanced state than are analyses for missing data. Although it usually is easy to write down the average effective error variance for a lattice design, the number of degrees of freedom associated with this variance is unknown. Little work has been done on this problem to date [56, 59, 209, 287, 301]. A preliminary investigation indicated that the number of degrees of freedom associated with the average effective error variance,

$$E = E_e\left\{1 + \frac{r}{(r-1)(k+1)} - \frac{rE_e}{(r-1)(k+1)E_b}\right\}, \qquad \text{(XI-96)}$$

for a lattice design of k^2 treatments in incomplete blocks of k in r arrangements is approximately the number of degrees of freedom associated with E_e [109]. Meier [209] reached the same conclusion with regard to the number of degrees of freedom for E in a double lattice design. As long as the number of degrees

of freedom for E and the probability distribution for E remain unknown, exact tests of significance are not possible.

Several approximate F tests are available for testing the equality of treatment means. For example, one could run a randomized complete block analysis and compute the following F test:

$$F_r[v-1 \text{ and } (r-1)(v-1)df] = \frac{\text{treatments (ignoring block) ms}}{\text{randomized complete block error ms}}. \quad \text{(XI-97)}$$

The degrees of freedom for both mean squares are known, but no use is made of the intrablock and interblock information. As a second F test, one could use the following:

$$F_i[v-1 \text{ and } (v-1)(r-1)-b+r\,df] = \frac{\text{treatments (eliminating block) ms}}{E_e}. \quad \text{(XI-98)}$$

Assuming additivity and normality of effects, F_i is an exact test with known degrees of freedom. However, this test makes no use of the interblock information and is difficult to compute for some comparisons among the treatments. A third F test is

$$F_a = \frac{\text{(sum of squares among adjusted treatment totals)}}{r(k^n-1)(\text{average effective error variance} = E)}. \quad \text{(XI-99)}$$

The last test is useful in practice, but the number of degrees of freedom associated with E is unknown. Preliminary investigations indicate that the number of degrees of freedom for the average effective error variance is approximately the same as for E_e. The test utilizes both intrablock and interblock information, and the total treatment degrees of freedom and sum of squares may be partitioned into subsets. Cochran [48] provides an F test for double and triple lattice designs without stating precisely the number of degrees of freedom associated with the mean squares. It is implied from the application of the formulae that the numerator of F has $v-1$ degrees of freedom and the denominator has the same number of degrees of freedom as E_e [48, 60]. Cochran [48] proposes the following F test for lattice experiments with r arrangements of the k^2 treatments in blocks of k units:[1]

$$F_c = \frac{V_u - \dfrac{(r-1)(w-w')}{(r-1)w+w'}\left\{\left(1+\dfrac{w'}{w(r-1)}\right)B_u-(b-r)E_b\right\}}{(k^2-1)E_e}, \quad \text{(XI-100)}$$

where V_u = treatment (ignoring block) sum of squares, B_u = blocks within replicate sum of squares, b = number of incomplete blocks, r = number of replicates, and w and w' are the weights. For the balanced lattice, F_c is equivalent to F_a, but this is not true for all other lattices.

[1]Cochran presents the formulae for the simple and triple lattice designs; the results are easily generalized to obtain formula (XI-100).

A fifth test has been proposed by Rao [255]. He makes the assumption that the weights are known and uses a chi-square test. The use of chi-square is equivalent to using F with infinite degrees of freedom in the denominator. Since only estimates of the population variances σ_ϵ^2 and σ_β^2 are obtained, the use of chi-square tends to give too many significant results. If Rao's chi-square is divided by $(k^2 - 1)$, it is equivalent to F_c and F_a for the balanced lattice.

For practical purposes it is suggested that F_a be used by the experimenter because of its ease of application. Some adjustment to the degrees of freedom associated with the average effective error variance may be necessary. With large lattices such a correction is of small consequence and may be omitted.

XI-8 Least Squares Estimates for a Double Lattice and Expectation of Mean Squares

Assume that the ijth treatment yield in the gth replicate is represented by one or the other of the following linear equations:

$$\left.\begin{aligned} X_{1ij} &= \mu + \rho_1 + \beta_{1i} + \tau_{ij} + \epsilon_{1ij} \\ X_{2ij} &= \mu + \rho_2 + \beta_{2j} + \tau_{ij} + \epsilon_{2ij} \end{aligned}\right\}, \tag{XI-101}$$

where μ = mean effect, β_{1i} = incomplete block effect in the X arrangement, β_{2j} = incomplete block effect in the Y arrangement, ρ_g = replicate effect in the gth replicate, τ_{ij} = treatment effect, ϵ_{gij} = a random effect, and $i, j = 1, 2, \cdots, k$. Two equations are used instead of one in order to simplify the notation.[1]

XI-8.1 LEAST SQUARES ESTIMATES OF EFFECTS WITH RECOVERY OF INTERBLOCK INFORMATION [COCHRAN, LECTURE NOTES, 1945; 48]

The sum of squares due to intrablock variations is equal to

$$R_1 = \sum_i \sum_j \left\{ [X_{1ij} - \hat{\mu} - \hat{\rho}_1 - \hat{\beta}_{1i} - \hat{\tau}_{ij}]^2 + [X_{2ij} - \hat{\mu} - \hat{\rho}_2 - \hat{\beta}_{2j} - \hat{\tau}_{ij}]^2 \right\}, \tag{XI-102}$$

and that due to the block constants or interblock variations is equal to

$$R_2 = \frac{1}{k}\left\{ \sum_i (X_{1i.} - k\hat{\mu} - k\hat{\rho}_1 - \sum_j \hat{\tau}_{ij})^2 + \sum_j (X_{2.j} - k\hat{\mu} - k\hat{\rho}_2 - \sum_i \hat{\tau}_{ij})^2 \right\}. \tag{XI-103}$$

[1] In setting up the linear equations for a triple lattice, a third equation may be used for the Z arrangement; if AB is confounded, then the block effect is $\beta_{3,i+j}$, in order to denote the particular block and treatment combinations appearing in the incomplete block. This notation may be used for a quadruple, $\cdots$, balanced lattice design.

The deviations in R_1 and R_2 are weighted inversely to their true standard deviations, $\sqrt{1/\omega}$ and $\sqrt{1/\omega'}$. This procedure results in minimum variance unbiased estimates. The weighted sum of squares is

$$\omega R_1 + \omega' R_2 = R, \tag{XI-104}$$

where $\omega = 1/\sigma_\epsilon^2$ and $\omega' = 1/(\sigma_\epsilon^2 + k\sigma_\beta^2)$. Following the procedure of earlier chapters the partial derivatives of R are obtained, the resulting partial derivatives are set equal to zero, and the resulting equations are solved to obtain the least squares estimates, $\hat{\mu}$, $\hat{\rho}_g$, $\hat{\beta}_{1i}$, $\hat{\beta}_{2j}$, and $\hat{\tau}_{ij}$, of the true effects, μ, ρ_g, β_{1i}, β_{2j}, and τ_{ij}, respectively.

$$\begin{aligned}\frac{\partial R}{\partial \hat{\mu}} &= -2\omega\sum_i\sum_j\left\{(X_{1ij} - \hat{\mu} - \hat{\rho}_1 - \hat{\beta}_{1i} - \hat{\tau}_{ij}) + (X_{2ij} - \hat{\mu} - \hat{\rho}_2 - \hat{\beta}_{2j} - \hat{\tau}_{ij})\right\} \\ &\quad -2\omega'\left\{\sum_i(X_{1i\cdot} - k\hat{\mu} - k\hat{\rho}_1 - \sum_j\hat{\tau}_{ij}) + \sum_j(X_{2\cdot j} - k\hat{\mu} - k\hat{\rho}_2 - \sum_i\hat{\tau}_{ij})\right\} \\ &= 0.\end{aligned} \tag{XI-105}$$

If $\sum\hat{\rho}_g = \sum_i\hat{\beta}_{1i} = \sum_j\hat{\beta}_{2j} = \sum\sum\hat{\tau}_{ij} = 0$, then

$$\hat{\mu} = \frac{1}{2k^2}(X_{1\cdot\cdot} + X_{2\cdot\cdot}) = \frac{1}{2}(\bar{x}_{1\cdot\cdot} + \bar{x}_{2\cdot\cdot}) = \bar{x}. \tag{XI-106}$$

$$\frac{\partial R}{\partial \hat{\beta}_{1i}} = -2\omega\sum_j(X_{1ij} - \hat{\mu} - \hat{\rho}_1 - \hat{\beta}_{1i} - \hat{\tau}_{ij}) = 0. \tag{XI-107}$$

$$\hat{\beta}_{1i} = \frac{1}{k}(X_{1i\cdot} - \sum_j\hat{\tau}_{ij}) - \hat{\mu} - \hat{\rho}_1. \tag{XI-108}$$

$$\frac{\partial R}{\partial \hat{\beta}_{2j}} = -2\omega\sum_i(X_{2ij} - \hat{\mu} - \hat{\rho}_2 - \hat{\beta}_{2j} - \hat{\tau}_{ij}) = 0. \tag{XI-109}$$

$$\hat{\beta}_{2j} = \frac{1}{k}(X_{2\cdot j} - \sum_i\hat{\tau}_{ij}) - \hat{\mu} - \hat{\rho}_2. \tag{XI-110}$$

The sum of equations (XI-108) and (XI-110) is equal to

$$\hat{\beta}_{1i} + \hat{\beta}_{2j} = \frac{1}{k}(X_{1i\cdot} + X_{2\cdot j} - \sum_j\hat{\tau}_{ij} - \sum_i\hat{\tau}_{ij}) - 2\hat{\mu}. \tag{XI-111}$$

$$\begin{aligned}\frac{\partial R}{\partial \hat{\tau}_{ij}} &= -2\omega(X_{1ij} - \hat{\mu} - \hat{\rho}_1 - \hat{\beta}_{1i} - \hat{\tau}_{ij} + X_{2ij} - \hat{\mu} - \hat{\rho}_2 - \hat{\beta}_{2j} - \hat{\tau}_{ij}) \\ &\quad - \frac{2\omega'}{k}(X_{1i\cdot} - k\hat{\mu} - k\hat{\rho}_1 - \sum_j\hat{\tau}_{ij} + X_{2\cdot j} - k\hat{\mu} - k\hat{\rho}_2 - \sum_i\hat{\tau}_{ij}) \\ &= 4\omega\hat{\tau}_{ij} - 2\omega(X_{1ij} + X_{2ij}) + 2\omega(\hat{\beta}_{1i} + \hat{\beta}_{2j}) + 4(\omega + \omega')\mu \\ &\quad - \frac{2\omega'}{k}(X_{1i\cdot} + X_{2\cdot j} - \sum_j\hat{\tau}_{ij} - \sum_i\hat{\tau}_{ij}) = 0.\end{aligned} \tag{XI-112}$$

Rearrangement of formula (XI-112) and substitution of the right-hand side of equation (XI-111) for the quantity $\hat{\beta}_{1i} + \hat{\beta}_{2j}$ result in the following:

$$\begin{aligned}&2\omega\hat{\tau}_{ij} + 2\omega'\hat{\mu} - \frac{(\omega - \omega')}{k}(\sum_i\hat{\tau}_{ij} + \sum_j\hat{\tau}_{ij}) \\ &= \omega(X_{1ij} + X_{2ij}) - \frac{(\omega - \omega')}{k}(X_{1i\cdot} + X_{2\cdot j}).\end{aligned} \tag{XI-113}$$

If the above equation is summed over the subscript i and is divided by k, we obtain

$$\frac{2\omega}{k}\sum_i \hat{\tau}_{ij} + 2\omega'\hat{\mu} - \frac{(\omega - \omega')}{k}\left(\sum_i \hat{\tau}_{ij}\right)$$
$$= \frac{\omega}{k}(X_{1\cdot j} + X_{2\cdot j}) - \frac{(\omega - \omega')}{k^2}(X_{1\cdot\cdot} + kX_{2\cdot j}). \qquad \text{(XI-114)}$$

Therefore,

$$\frac{(\omega + \omega')}{k}\sum_i \hat{\tau}_{ij} + 2\omega'\hat{\mu} = \frac{\omega}{k}(X_{1\cdot j} + X_{2\cdot j}) - \frac{(\omega - \omega')}{k^2}(X_{1\cdot\cdot} + kX_{2\cdot j})$$
$$= \frac{\omega}{k}X_{1\cdot j} + \frac{\omega'}{k}X_{2\cdot j} - \frac{(\omega - \omega')}{k^2}X_{1\cdot\cdot}. \qquad \text{(XI-115)}$$

Likewise,

$$\frac{(\omega + \omega')}{k}\sum_j \hat{\tau}_{ij} + 2\omega'\hat{\mu} = \frac{\omega'}{k}X_{1i\cdot} + \frac{\omega}{k}X_{2i\cdot} - \frac{(\omega - \omega')}{k^2}X_{2\cdot\cdot}. \qquad \text{(XI-116)}$$

Adding the above two equations, we obtain

$$\frac{(\omega + \omega')}{k}\left(\sum_i \hat{\tau}_{ij} + \sum_j \hat{\tau}_{ij}\right) = \frac{\omega}{k}(X_{1\cdot j} + X_{2i\cdot}) + \frac{\omega'}{k}(X_{1i\cdot} + X_{2\cdot j})$$
$$- \frac{(\omega - \omega')}{k^2}(X_{1\cdot\cdot} + X_{2\cdot\cdot}) - 4\omega'\hat{\mu}$$
$$= \omega(\bar{x}_{1\cdot j} + \bar{x}_{2i\cdot}) + \omega'(\bar{x}_{1i\cdot} + \bar{x}_{2\cdot j}) - (\omega + \omega')(\bar{x}_{1\cdot\cdot} + \bar{x}_{2\cdot\cdot}). \qquad \text{(XI-117)}$$

Substituting the above result for $\frac{1}{k}\left(\sum_i \hat{\tau}_{ij} + \sum_j \hat{\tau}_{ij}\right)$ in equation (XI-113) and multiplying the result by $(\omega + \omega')$ we obtain

$$2\omega(\omega + \omega')\hat{\tau}_{ij} + 2\omega'(\omega + \omega')\hat{\mu} - (\omega - \omega')\{\omega(\bar{x}_{1\cdot j} + \bar{x}_{2i\cdot}) + \omega'(\bar{x}_{1i\cdot} + \bar{x}_{2\cdot j})$$
$$- (\omega + \omega')(\bar{x}_{1\cdot\cdot} + \bar{x}_{2\cdot\cdot})\}$$
$$= \omega(\omega + \omega')(X_{1ij} + X_{2ij}) - (\omega^2 - \omega'^2)(\bar{x}_{1i\cdot} + \bar{x}_{2\cdot j}). \qquad \text{(XI-118)}$$

After collecting and rearranging the terms, we obtain the following:

$$\hat{\tau}_{ij} + \hat{\mu} = \frac{1}{2}(X_{1ij} + X_{2ij}) + \frac{(\omega - \omega')}{2(\omega + \omega')}\{\bar{x}_{2i\cdot} - \bar{x}_{1i\cdot} + \bar{x}_{1\cdot j} - \bar{x}_{2\cdot j}\}. \qquad \text{(XI-119)}$$

The above least squares estimate of the adjusted treatment mean is the one used in example XI-1. The unadjusted mean, $\frac{1}{2}(X_{1ij} + X_{2ij})$, is adjusted for the correction for block means, $\frac{1}{2}(\bar{x}_{2i\cdot} - \bar{x}_{1i\cdot} + \bar{x}_{1\cdot j} - \bar{x}_{2\cdot j})$, multiplied by a weighting factor equal to $(\omega - \omega')/(\omega + \omega')$.

If interblock information is ignored, the least squares estimate is equal to

$$\tfrac{1}{2}(X_{1ij} + X_{2ij} + \bar{x}_{2i\cdot} - \bar{x}_{1i\cdot} + \bar{x}_{1\cdot j} - \bar{x}_{2\cdot j}). \qquad \text{(XI-120)}$$

The block correction is not dampened down by the weighting factor as it is when interblock information is recovered [322].

If more than one set of a double lattice is used, the procedure follows through in the same manner as for one set. The observations in the X arrange-

ment are denoted by X_{f1ij} and in the Y arrangement by X_{f2ij}, where $f = 1, 2, \cdots, q$ = number of repetitions of the simple lattice.

XI-8.2 EXPECTATION OF MEAN SQUARES

The expectation of the treatment (ignoring block) sum of squares in one set of a double lattice is

$$E\left[\frac{1}{2}\sum_i\sum_j X_{\cdot ij}^2 - \frac{X_{\cdots}^2}{2k^2}\right] = \frac{1}{2}\sum\sum E\Big[(2\mu + \sum \rho_g + \beta_{1i} + \beta_{2j} + 2\tau_{ij} + \epsilon_{1ij}$$

$$+ \epsilon_{2ij})^2 - \frac{1}{2k^2}(2k^2\mu + k^2\sum\rho_g + k\sum\beta_{1i} + k\sum\beta_{2j} + 2\sum\sum\tau_{ij} + \sum\sum\sum\epsilon_{gij})^2\Big]$$

$$= 2k^2\mu^2 + k^2\sigma_\rho^2 + k^2\sigma_\beta^2 + 2k^2\sigma_\tau^2 + k^2\sigma_\epsilon^2 - (2k^2\mu^2 + k^2\sigma_\rho^2 + k\sigma_\beta^2 + 2\sigma_\tau^2 + \sigma_\epsilon^2)$$

$$= (k^2 - 1)\left\{\sigma_\epsilon^2 + \frac{k}{k+1}\sigma_\beta^2 + 2\sigma_\tau^2\right\}. \tag{XI-121}$$

In obtaining the above expectations, it is assumed that the effects are independent and additive, that $E\beta_{1i}^2 = E\beta_{2j}^2 = \sigma_\beta^2$, and that the remaining variables squared have expectations μ^2, σ_ρ^2, σ_τ^2, and σ_ϵ^2.

The replicate sum of squares has the expectation:

$$E\left[\frac{1}{k^2}\sum_g X_{g\cdot\cdot}^2 - \frac{X_{\cdots}^2}{2k^2}\right] = 2k^2(\mu^2 + \sigma_\rho^2) + 2k\sigma_\beta^2 + 2\sigma_\tau^2 + 2\sigma_\epsilon^2 - E[X_{\cdots}^2/2k^2]$$

$$= \sigma_\epsilon^2 + k\sigma_\beta^2 + k^2\sigma_\rho^2. \tag{XI-122}$$

The expectation of the block (eliminating treatment effect) sum of squares is equal to

$$E\left[\sum_i \frac{(X_{2i\cdot} - X_{1i\cdot})^2}{2k} + \sum_j \frac{(X_{1\cdot j} - X_{2\cdot j})^2}{2k} - 2\frac{(X_{1\cdot\cdot} - X_{2\cdot\cdot})^2}{2k^2}\right]$$

$$= 2(k-1)(\sigma_\epsilon^2 + k\sigma_\beta^2/2), \tag{XI-123}$$

with $2(k - 1)$ degrees of freedom.

The expectation of the total sum of squares is

$$E\left[\sum\sum\sum X_{gij}^2 - \frac{X_{\cdots}^2}{2k^2}\right]$$

$$= (2k^2 - 1)\sigma_\epsilon^2 + (2k^2 - k)\sigma_\beta^2 + 2(k^2 - 1)\sigma_\tau^2 + k^2\sigma_\rho^2, \tag{XI-124}$$

with $2k^2 - 1$ degrees of freedom. The intrablock error sum of squares is obtained by subtraction. The above results are summarized below.

Source of variation	df	Expected value of ms
Replicate	1	$\sigma_\epsilon^2 + k\sigma_\beta^2 + k^2\sigma_\rho^2$
Treatment (ignoring block)	$k^2 - 1$	$\sigma_\epsilon^2 + \frac{k}{k+1}\sigma_\beta^2 + 2\sigma_\tau^2$
Block (eliminating treatment)	$2(k - 1)$	$\sigma_\epsilon^2 + k\sigma_\beta^2/2$
Intrablock	$(k - 1)^2$	σ_ϵ^2
Total	$2k^2 - 1$	—

In the above analysis, it will be observed that no appropriate error exists for comparing the treatment (ignoring block) mean square. The expectation of the treatment (eliminating block) mean square is $\sigma_\epsilon^2 + 2\sigma_\tau^2$. Hence, the intrablock error mean square is appropriate for testing this mean square (formula XI-98), but no use is made of the interblock information in this test. The expectation of the component (*a*) sum of squares in more than one set of a double lattice is equal to $\sigma_\epsilon^2 + k\sigma_\beta^2$ [see 302].

XI-9 Derivation of Standard Errors

XI-9.1 THE DOUBLE LATTICE

The variance of the difference between two adjusted means from an experiment designed as a double lattice may be developed in the following manner. Consider the adjusted means for treatments 00 and 01. Then,

$$\bar{x}_{00}' = \frac{[(A)_0 + (B)_0]_{wt\cdot d}}{k} + \frac{(AB)_0 + \cdots + (AB^{k-1})_0}{k} - \frac{X\ldots}{2k}$$

and

$$\bar{x}_{01}' = \frac{[(A)_0 + (B)_1]_{wt\cdot d}}{k} + \frac{(AB)_1 + \cdots + (AB^{k-1})_{k-1}}{k} - \frac{X\ldots}{2k}.$$

$$V(\bar{x}_{00}' - \bar{x}_{01}') = \frac{1}{k^2}V\left\{(B_0 - B_1)_{wt\cdot d} + (AB)_0 - (AB)_1 + \cdots + (AB^{k-1})_0 - (AB^{k-1})_{k-1}\right\} = \frac{1}{k^2}V\left\{\frac{\omega[(B)_{10} - (B)_{11}] + \omega'[(B)_{20} - (B)_{21}]}{\omega + \omega'} + (AB)_0 - (AB)_1 + \cdots + (AB^{k-1})_0 - (AB^{k-1})_{k-1}\right\} = \frac{\omega^2(2k\sigma_\epsilon^2) + (\omega')^2(2k)(\sigma_\epsilon^2 + k\sigma_\beta^2)}{k^2(\omega + \omega')^2}$$

$$+ \frac{k(k-1)\sigma_\epsilon^2}{k^2} = \frac{2}{k(\omega + \omega')} + \frac{2(k-1)}{2k\omega} = \frac{2}{k}\left\{\frac{1}{\omega + \omega'} + \frac{k-1}{2\omega}\right\}, \quad \text{(XI-125)}$$

where $\sigma_\epsilon^2 = 1/\omega$ and $\sigma_\epsilon^2 + k\sigma_\beta^2 = 1/\omega'$ (see formula (XI-5)).

The variance of a difference between a pair of adjusted means of treatments not appearing together in an incomplete block, say 00 and 11, is

$$V(\bar{x}_{00}' - \bar{x}_{11}') = V\left\{\frac{[(A)_0 - (A)_1]_{wt\cdot d}}{k} + \frac{[(B)_0 - (B)_1]_{wt\cdot d}}{k} + \frac{[(AB)_0 - (AB)_2] + \cdots + (AB)_0 - (AB^{k-2})_{k-1}}{k}\right\}$$

$$= \frac{4k(\omega + \omega')}{k^2(\omega + \omega')^2} + \frac{2k(k-2)}{2k^2\omega} = \frac{2}{k}\left\{\frac{2}{\omega + \omega'} + \frac{k-2}{2\omega}\right\} \quad \text{(XI-126)}$$

(see formula (XI-6).)

XI-9.2 THE THREE-DIMENSIONAL TRIPLE LATTICE (THE CUBIC LATTICE)

There are three types of comparisons among pairs of varieties in the

three-dimensional triple lattice. These are exemplified by the following comparisons among adjusted treatment means:

$$\bar{x}_{000}' - \bar{x}_{001}',\ \bar{x}_{000}' - \bar{x}_{011}',\ \text{and}\ \bar{x}_{000}' - \bar{x}_{111}'.$$

Each of the comparisons is subject to different variances. Thus,

$$V(\bar{x}_{000} - \bar{x}_{001}') = V\left\{\frac{[(A)_0 - (A)_0 + (B)_0 - (B)_0 + (C)_0 - (C)_1]_{wt\cdot d}}{k^2}\right.$$
$$+ \frac{[(AB)_0 - (AB)_0 + \cdots + (BC^{k-1})_0 - (BC^{k-1})_{k-1}]_{wt\cdot d}}{k^2}$$
$$\left.+ \frac{(ABC)_0 - (ABC)_1 + \cdots + (AB^{k-1}C^{k-1})_0 - (AB^{k-1}C^{k-1})_{k-1}}{k^2}\right\}$$
$$= \frac{2(1)}{k^2(\omega + 2\omega')} + \frac{(2)(2)(k-1)}{k^2(2\omega + \omega')} + \frac{2(k-1)^2}{3k^2\omega}$$
$$= \frac{2}{k^2}\left\{\frac{1}{\omega + 2\omega'} + \frac{2(k-1)}{2\omega + \omega'} + \frac{(k-1)^2}{3\omega}\right\} \qquad \text{(XI-127)}$$

(see formula (XI-69)). Also,

$$V(\bar{x}_{000}' - \bar{x}_{011}') = V\left\{\frac{[(A)_0 - (A)_0 + (B)_0 - (B)_1 + (C)_0 - (C)_1]_{wt\cdot d}}{k^2}\right.$$
$$+ \frac{[(AB)_0 - (AB)_1 + \cdots + (BC)_0 - (BC)_0 + \cdots + (BC^{k-1})_{k-1} - (BC^{k-1})_0]_{wt\cdot d}}{k^2}$$
$$\left.+ \frac{(ABC)_0 - (ABC)_2 + \cdots + (AB^{k-1}C^{k-1})_0 - (AB^{k-1}C^{k-1})_{2(k-1)}}{k^2}\right\}$$
$$= \frac{2(2)}{k^2(\omega + 2\omega')} + \frac{2(3k-4)}{k^2(2\omega + \omega')} + \frac{2(k-1)(k-2)}{k^2 3\omega}$$
$$= \frac{2}{k^2}\left\{\frac{2}{\omega + 2\omega'} + \frac{3k-4}{2\omega + \omega'} + \frac{(k-1)(k-2)}{3\omega}\right\} \qquad \text{(XI-128)}$$

(see formula (XI-70).)

$$V(\bar{x}_{000}' - \bar{x}_{111}') = V\left\{\frac{[(A)_0 - (A)_1 + (B)_0 - (B)_1 + (C)_0 - (C)_1]_{wt\cdot d}}{k^2}\right.$$
$$+ \frac{[(AB)_0 - (AB)_2 + \cdots + (BC^{k-1})_0 - (BC^{k-1})_0]_{wt\cdot d}}{k^2}$$
$$\left.+ \frac{(ABC)_0 - (ABC)_3 + \cdots + (AB^{k-1}C^{k-1})_0 - (AB^{k-1}C^{k-1})_{2k-1}}{k^2}\right\}$$
$$= \frac{2(3)}{k^2(\omega + 2\omega')} + \frac{2(3)(k-2)}{k^2(2\omega + \omega')} + \frac{2(k^2 - 3k + 3)}{3k^2\omega}$$
$$= \frac{2}{k^2}\left\{\frac{3}{\omega + 2\omega'} + \frac{3(k-2)}{2\omega + \omega'} + \frac{k^2 - 3k + 3}{3\omega}\right\} \qquad \text{(XI-129)}$$

(see formula (XI-71).)

CHAPTER XII

Lattice Designs with More Than One Restriction on the Allocation of Treatments in the Complete Block

XII-1 Introduction

The experimental units within a complete block are often subject to more than one source of variation. Recognizing the need for appropriate experimental designs for such conditions, Yates [323, 324, 328] developed a group of incomplete block designs with two- or more-way elimination of heterogeneity among the experimental units within the complete block. The most popular and widely known of these designs are the semi-balanced and balanced lattice squares [328]. A number of other workers [52, 104, 106, 171, 175, 178] have added to the group of designs proposed by Yates.

Examples of more than one type of variation within a complete block may be found in most fields of experimentation. For example, in field husbandry experiments, soil gradients may extend in two directions. The day-to-day and worker-to-worker variations in laboratory experiments represent two sources of variation which may be controlled with the lattice square design. Batch-to-batch and order-of-preparation or evaluation of material represent other examples of two-way variation. In animal experiments the litters may form one source of variation with the time period on each individual within a litter forming a second source of variation; also, in certain types of experiments a third restriction in the experimental design may be used to control the variation due to size of animal. Under such circumstances, the whole plots might be the litters, the rows of split plots in a whole plot might be the size of animal, and the columns of split plots in a whole plot might be the time of observation. The two-way control of variation within a whole plot would correspond to the ordinary rows and columns in a lattice square design.

When recovering interrow, intercolumn, interwhole plot, etc. information, care should be taken to have sufficient degrees of freedom associated with each variance. It is suggested that at least 10 to 14 degrees of freedom, and preferably 16 to 20 degrees of freedom, be associated with each variance. If fewer than 10 to 14 degrees of freedom are available, then only intrablock information should be recovered, provided that there are sufficient degrees of freedom associated with the intrablock variance. In some instances the mean square

due to rows, columns, or blocks is less than the intrablock error mean square, resulting in less information from residual or intrablock error than from the other source. The recommendation here is to set the mean square which is less than the intrablock error, E_e, equal to it; in this case, the adjustment to a treatment mean, due to this source, is equal to zero. For example, suppose that the row (eliminating treatment) mean square, E_r, is less than E_e in a lattice square. The recommendation is to set E_r equal to E_e; this results in zero adjustments for row effects.

XII-2 Lattice Square Designs

The two-dimensional two-restrictional lattice (the quasi-latin square or the lattice square) is an incomplete block design of k^2 treatments in which the treatments are grouped into incomplete blocks in two ways, similar to the grouping by rows and columns in a latin square. The comparison of a two-restrictional lattice with a one-restrictional lattice may be likened to the comparison of the latin square design with the randomized complete block design. The relative efficiencies are also comparable, with the two-restrictional lattice generally being more efficient than the one-restrictional lattice [50, 73, 168].

The efficiency of the lattice square design relative to the randomized complete block design has been obtained for a relatively large number of experiments. Cochran [50] found that for corn in 2 × 10 hill plots the increase in accuracy for the lattice square was about one replicate in six for 25 treatments, one replicate in five for 49 or 81 treatments, and one replicate in three for 121 treatments. Bliss and Dearborn [25] and Ma and Harrington [201] obtained similar results on other crops, but Cox [73] found somewhat greater advantages for the lattice square over all types of experiments. Cochran [50] found that a lattice square in three replicates resulted in a 15 per cent gain in efficiency over the triple lattice design; the data from three uniformity trials were used for this comparison. Johnson and Murphy [168], using oat uniformity trial data, found that the lattice square design was more efficient than the triple lattice design and that the amount of variation controlled by rows and columns in the lattice square design was roughly proportional to the width and length of the replicate; they point out that replicate shape affects apparent precision of the lattice design. Zuber [340], using corn uniformity trial data, obtained similar results.

The layout of a lattice square design in an experiment should be such that the complete replicate represents what would have been used for a randomized complete block design if one is to compute relative efficiency. A poor replicate shape for a randomized complete block design by intent results in a biased estimate of the efficiency of the lattice square. As with the latin square an attempt should be made to obtain approximately equal information on rows and columns. (If the whole of the variation within a complete block can

be controlled with either rows or columns, a one-restrictional design should be used.) The shape of the experimental unit can be altered in some experiments to effect this, although Cochran [50] found that plots of 4×5 hills of corn were no more accurate than 2×10 hills; Bliss and Dearborn [25], using a 30-foot single row, came to a similar conclusion. In general, it is suggested that the variation in both directions be equalized and that the plot shape be adjusted accordingly. The same recommendation holds for laboratory and greenhouse experiments wherever applicable. In the absence of knowledge concerning variation in field experiments, both the complete block and the experimental unit should be square, provided that these shapes are consonant with cultural operations. In field and laboratory experimentation the rows may represent order within the incomplete blocks which are the columns, and the columns may be laid end to end, provided that the experimental conditions (e.g., contours or order of performing an observation) warrant such a design.

The randomization procedure for the lattice square design is as follows:

(i) Assign the entry numbers to the treatments at random unless otherwise indicated;
(ii) assign the arrangements to the replicates at random;
(iii) assign the levels of the pseudo-effects confounded with columns to the columns at random within each replicate;
(iv) assign the levels of the pseudo-effect confounded with rows to the rows at random within each replicate.

The above procedure of randomization preserves the arrangements or the systems of confounding pseudo-effects in each replicate. This procedure is similar to that for the latin square design in that the basic arrangement is preserved.

Clem and Federer [42] have tabulated arrangements for balanced lattice squares and have prepared twelve randomizations of each of the $k + 1$ arrangements for $k = 5$, 7, 8, and 9, and six randomizations each for $k = 11$ and 13. If a semi-balanced lattice square is desired, the first $(k + 1)/2$ (or the last $(k + 1)/2$) arrangements given by Clem and Federer [42] may be used. For other lattice square designs a number of arrangements not equal to $(k + 1)/2$ or $k + 1$ may be taken from their listing.

XII-2.1 SEMI-BALANCED LATTICE SQUARE

The semi-balanced lattice square for k^2 treatments as proposed by Yates [328] consists of $(k + 1)/2 = r$ arrangements, with every treatment compared with every other treatment in a row *or* in a column. Each of the pseudo-effects is confounded either with the rows *or* with the columns in one of the replicates. $(k + 1)/2$ arrangements are possible for k = an odd prime number or power of an odd prime number; i.e., $k = 3, 5, 7, 9, 11, 13$, etc. For example, in a

5×5 lattice square with twenty-five treatments a system of confounding for the $(5 + 1)/2 = 3$ arrangements is

Pseudo-effect confounded with:	Arrangement		
	I	II	III
Rows	A	AB	AB^3
Columns	B	AB^2	AB^4

Also, any other permutation of the six pseudo-effects may be used. Other methods of construction may be used; e.g., use of orthogonal latin squares [305, 323], exhaustive enumeration, etc.

The general form of the analysis of variance for q sets of the basic set of $(k + 1)/2$ arrangements is

Source of variation	df	Mean square	
		Observed	Expected value
Replicate	$(r - 1) = q(k + 1)/2 - 1$	—	—
Treatment (ignoring row and column)	$k^2 - 1$	—	—
Row (eliminating treatment)	$q(k^2 - 1)/2$	E_r	$\sigma_\epsilon^2 + (r - 1)k\sigma_\rho^2/r$
Component (a)	$(q - 1)(k^2 - 1)/2$	—	$\sigma_\epsilon^2 + k\sigma_\rho^2$
Component (b)	$(k^2 - 1)/2$	—	$\sigma_\epsilon^2 + k(k - 1)\sigma_\rho^2/(k + 1)$
Column (eliminating treatment)	$q(k^2 - 1)/2$	E_c	$\sigma_\epsilon^2 + (r - 1)k\sigma_\gamma^2/r$
Component (a)	$(q - 1)(k^2 - 1)/2$	—	$\sigma_\epsilon^2 + k\sigma_\gamma^2$
Component (b)	$(k^2 - 1)/2$	—	$\sigma_\epsilon^2 + k(k - 1)\sigma_\gamma^2/(k + 1)$
Intrablock	$(k^2 - 1)(qk - q - 2)/2$	E_e	σ_ϵ^2
Total	$qk^2(k + 1)/2 - 1$	—	—

The component (a) sum of squares for rows represents the interaction sum of squares of the levels of pseudo-effects and the replicates in which the effects are confounded with row differences. Likewise, the component (a) sum of squares for columns is the interaction sum of squares of the levels of the pseudo-effects and the replicates in which the effect is confounded with column differences. (These two sums of squares are of the same nature as the component (a) sum of squares obtained in one-restrictional lattices.)

The component (b) sum of squares (similar to the component (b) sum of squares in the one-restrictional lattice) is composed of the sums of squares of the contrasts of the levels of the pseudo-effects in the replicates in which the effects are unconfounded with the same levels in the replicates in which the effects are confounded. In semi-balanced lattice squares, pseudo-effects

confounded with rows are unconfounded with columns, and *vice versa*. Such a scheme of confounding obviates the necessity of removing column effects from row effects, and *vice versa*, since these effects are independent.

Effort should be made to obtain equal information on all effects. Therefore, rather than repeating the basic set of $(k+1)/2$ arrangements, it is recommended that additional arrangements be used. For example, instead of using two sets of $(k+1)/2$ arrangements of a semi-balanced lattice square, it is more desirable from the standpoint of all-around efficiency to use a balanced lattice square with $k+1$ arrangements.

Homeyer *et al.* [160] give detailed instructions for calculating the analysis of variance table, for adjusting the treatment means, and for obtaining the standard errors. They present procedures for both punched card and calculating machines.

Example XII-1. A $k^2 = 3^2$ semi-balanced lattice square example in $(k+1)/2 = 2$ replicates is given in table XII-1. The A effect is confounded with row differences and the B effect with column differences in replicate I and AB with rows and AB^2 with columns in replicate II.

The data are systematically rearranged in table XII-2, and the totals for the analysis of variance are computed. The variety (treatment) totals are given in the last part of the table, and from the totals of rows and columns the levels of the effects A, B, AB, and AB^2 are obtained. The totals for levels of the effects are placed opposite the corresponding row or column total in the top part of table XII-2. The $(A)_{\cdot i} - r(A)_{1i}$, $(B)_{\cdot j} - r(B)_{1j}$, $(AB)_{\cdot g} - r(AB)_{2g}$, and $(AB^2)_{\cdot h} - r(AB^2)_{2h}$ quantities are computed in the same manner as for the double and triple lattices. The second $(AB)_{\cdot g} - 2(AB)_{2g}$ value is equal to $24 - 2(9) = 6$. The sum of the $(A)_{\cdot i} - 2(A)_{1i} = -8$ must equal the sum of the $(B)_{\cdot j} - 2(B)_{1j} = -8$ in replicate I and similarly in replicate II. The sum of the $(A)_{\cdot i} - 2(A)_{1i}$ and the $(AB)_{\cdot g} - 2(AB)_{2g}$ must equal zero in the 3^2 semi-balanced lattice square; i.e., the row contrasts must equal zero. Also, the sum of all the column contrasts must equal zero; thus, $8 + (-8) = 0$.

The last row and column of the tables headed "Replicate I" and "Replicate II" in table XII-2 are obtained after computing the analysis of variance and the weighting factors. (In this case, no row and column corrections are obtainable, since there are zero degrees of freedom for the intrablock error sum of squares.)

TABLE XII-1. Yields and experimental layout for a semi-balanced lattice square with 3^2 varieties in two replicates (Variety numbers in parentheses)

Replicate I							Replicate II						
(12)	2	(10)	3	(11)	7	$12=(A)_{11}$	(10)	3	(01)	2	(22)	7	$12=(AB)_{21}$
(02)	3	(00)	8	(01)	3	$14=(A)_{10}$	(02)	4	(20)	2	(11)	3	$9=(AB)_{22}$
(22)	6	(20)	5	(21)	3	$14=(A)_{12}$	(21)	2	(12)	3	(00)	6	$11=(AB)_{20}$
$(B)_{12}=11$		$(B)_{10}=16$		$(B)_{11}=13$		40	$(AB^2)_{21}=9$		$(AB^2)_{22}=7$		$(AB^2)_{20}=16$		32

TABLE XII-2. Systematic arrangement of yields and totals required for the analysis of variance

	Replicate I			$(A)_{1i}$	$(A)_{.i}$	$(A)_{.i} - 2(A)_{1i}$	c_x'
	(00) 8	(01) 3	(02) 3	14	26	-2	
	(10) 3	(11) 7	(12) 2	12	21	-3	
	(20) 5	(21) 3	(22) 6	14	25	-3	
$(B)_{1j}$	16	13	11	40			
$(B)_{.j}$	27	20	25		72		
$(B)_{.j} - 2(B)_{1j}$	-5	-6	3			-8	
c_y'							

	Replicate II			$(AB)_{2g}$	$(AB)_{.g}$	$(AB)_{.g} - 2(AB)_{2g}$	c_w'
	(00) 6	(21) 2	(12) 3	11	24	2	
	(22) 7	(10) 3	(01) 2	12	24	0	
	(11) 3	(02) 4	(20) 2	9	24	6	
$(AB^2)_{2h}$	16	9	7	32			
$(AB^2)_{.h}$	37	18	17		72		
$(AB^2)_{.h} - 2(AB^2)_{2h}$	5	0	3			8	
c_z'							

	Variety totals			$(A)_{.i}$		Variety totals			$(AB)_{.g}$
	(00) 14	(01) 5	(02) 7	26		(00) 14	(21) 5	(12) 5	24
	(10) 6	(11) 10	(12) 5	21		(22) 13	(10) 6	(01) 5	24
	(20) 7	(21) 5	(22) 13	25		(11) 10	(02) 7	(20) 7	24
$(B)_{.j}$	27	20	25	72	$(AB^2)_{.h}$	37	18	17	72

TABLE XII-3. Analysis of variance

Source of variation	df	ss	ms
Replicate	1	3.56	3.56
Variety (ignoring row and column)	8	49.00	6.12
Error (r. c. b.)	8	13.44	1.680
Column (eliminating variety)	4	10.22	$2.555 = E_c$
Row (eliminating variety)	4	3.22	$0.805 = E_r$
Intrablock	0	0.00	$0.000 = E_e$
Total	17	66.00	-
Correction for mean	1	288.00	-

The sum of squares of the quantities,

$$\frac{\sum[(A)_{.i} - r(A)_{1i}]^2}{k(k^2-1)/4} - \frac{\{\sum[(A)_{.i} - r(A)_{1i}]\}^2}{k^2(k^2-1)/4} + \frac{\sum[(AB)_{.g} - r(AB)_{2g}]^2}{k(k^2-1)/4}$$

$$- \frac{\{\sum[(AB)_{.g} - r(AB)_{2g}]\}^2}{k^2(k^2-1)/4} \tag{XII-1}$$

$= \dfrac{(-2)^2 + (-3)^2 + (-3)^2}{6} - \dfrac{(-8)^2}{18} + \dfrac{2^2 + 6^2 + 0^2}{6} - \dfrac{8^2}{18} = 3.22$, is equal to the

row (eliminating variety) sum of squares with $r(k-1) = \frac{k+1}{2}(k-1) = \frac{k^2-1}{2}$ $= 4$ degrees of freedom.

The column (eliminating variety) sum of squares is equal to

$$\frac{\sum[(B)._j - r(B)_{1j}]^2}{k(k^2-1)/4} - \frac{\{\sum[(B)._j - r(B)_{1j}]\}^2}{k^2(k^2-1)/4} + \frac{\sum[(AB^2)._h - r(AB^2)_{2h}]^2}{k(k^2-1)/4}$$
$$- \frac{\{\sum[(AB^2)._h - r(AB^2)_{2h}]\}^2}{k^2(k^2-1)/4} \tag{XII-2}$$
$$= \frac{(-5)^2 + (-6)^2 + 3^2}{6} - \frac{(-8)^2}{18} + \frac{5^2 + 3^2 + 0^2}{6} - \frac{8^2}{18} = 10.22,$$

with $r(k-1) = 4$ degrees of freedom.

The intrablock error sum of squares with $(k^2-1)(k-3)/2 = 0$ degrees of freedom is obtained by subtracting the row (eliminating variety) and column (eliminating variety) sums of squares from the randomized complete block error sum of squares, $13.44 - 3.22 - 10.22 = 0.00$; in this example the residual sum of squares is equal to zero, since all variation and all degrees of freedom are allotted.

The weighting factor for the column corrections is equal to[1]

$$\mu = \frac{E_c - E_e}{k(r-1)E_c} = \frac{2(E_c - E_e)}{k(k-1)E_c} = \frac{2(w - w_c)}{k[(k-1)w + 2w_c]}, \tag{XII-3}$$

and the weighting factor for the row corrections is equal to[1]

$$\lambda = \frac{E_r - E_e}{k(r-1)E_r} = \frac{2(E_r - E_e)}{k(k-1)E_r} = \frac{2(w - w_r)}{k[(k-1)w + 2w_r]}, \tag{XII-4}$$

where $r = (k+1)/2$,

$$w = 1/E_e, \tag{XII-5}$$
$$w_r = (r-1)/(rE_r - E_e), \tag{XII-6}$$

and $$w_c = (r-1)/(rE_c - E_e). \tag{XII-7}$$

For this particular example the weighting factors are not computed, since they are not obtainable for k less than five for the semi-balanced lattice square. In the event that k is five or greater, the row corrections are computed as

$$c_x' = \lambda[(A)._i - 2(A)_{1i}],\ c_w' = \lambda[(AB)._g - 2(AB)_{2g}],\ \text{etc.}, \tag{XII-8}$$

and the column corrections as

$$c_y' = \mu[(B)._j - 2(B)_{1j}],\ c_z' = \mu[(AB^2)._h - 2(AB^2)_{2h}],\ \text{etc.} \tag{XII-9}$$

The corresponding row and column corrections from each replicate are added to the variety totals to obtain the adjusted totals. Since every pseudo-main effect or interaction is confounded either with a row or a column, there are $k + 1$ adjustments added to each variety total.

The average effective error variance is $E_e\left[1 + \frac{k(\lambda + \mu)}{2}\right]$, and the efficiency of the semi-balanced lattice square relative to the randomized complete block design is the ratio of the two average effective error variances,

[1]If $q > 1$, divide extreme right-hand side of formulae (XII-3) and (XII-4) by q, and set $r = q(k+1)/2$ in formulae (XII-6) and (XII-7).

$$\frac{\text{randomized block error} \times 100}{E_e\left[1 + \dfrac{k(\lambda + \mu)}{2}\right]}. \tag{XII-10}$$

The average standard error of a difference of any two adjusted variety means is

$$\sqrt{\frac{2E_e}{k+1}\left\{2 + k(\lambda + \mu)\right\}} = \sqrt{\frac{2}{k+1}\left\{\frac{k+1}{(k-1)w + 2w_r} + \frac{k+1}{(k-1)w + 2w_c}\right\}}. \tag{XII-11}$$

The standard error of a mean difference for two varieties appearing together in a row is

$$\sqrt{\frac{2E_e}{k+1}\{2 + (k-1)\lambda + (k+1)\mu\}}, \tag{XII-12}$$

and for two varieties appearing together in one of the columns is

$$\sqrt{\frac{2E_e}{k+1}\{2 + (k+1)\lambda + (k-1)\mu\}}. \tag{XII-13}$$

XII-2.2 BALANCED LATTICE SQUARE

The balanced lattice square [328] is made up of $k + 1$ arrangements (or sets of $k + 1$ arrangements) in which every pseudo-effect is confounded once with rows and once with columns in one of the $k + 1$ arrangements. This also means that any pair of treatments occurs together once in a row *and* once in a column. Balanced lattice squares are available for all prime numbers or powers of prime numbers ($k = 2, 3, 4, 5, 7, 8, 9, 11, 13$, etc.; [see 42]). The scheme of confounding of the pseudo-effects for a 5×5 lattice square is

Pseudo-effect confounded with	Arrangement					
	I	II	III	IV	V	VI
Rows	A	AB	AB^3	B	AB^2	AB^4
Columns	B	AB^2	AB^4	A	AB	AB^3

or any other permutation of the $k + 1 = 6$ pseudo-effects in the first three and in the last three arrangements.

The general form of the analysis of variance for q sets of the basic set of $k + 1$ arrangements is

Source of variation	df	Mean square	
		Observed	Expected value
Replicate	$q(k+1) - 1$	—	—
Treatment (ignoring row and column)	$k^2 - 1$	—	—
Row (eliminating treatment; ignoring column)	$q(k^2 - 1)$	—	—
Component (a)	$(q-1)(k^2-1)$	—	—
Component (b)	$(k^2 - 1)$	—	—
Column (eliminating treatment and row)	$q(k^2 - 1)$	E_c	$\sigma_e^2 + (qk-1)\sigma_\gamma^2/q$
Component (a)	$(q-1)(k^2-1)$	—	$\sigma_e^2 + k\sigma_\gamma^2$
Component (b)	$(k^2 - 1)$	—	$\sigma_e^2 + (k-1)\sigma_\gamma^2$
Intrablock	$(k^2 - 1)(qk - q - 1)$	E_e	σ_e^2

Total	$qk^2(k+1)-1$	—	—
Column (eliminating treatment; ignoring row)	$q(k^2-1)$	—	—
Component (a)	$(q-1)(k^2-1)$	—	—
Component (b)	(k^2-1)	—	—
Row (eliminating treatment and column)	$q(k^2-1)$	E_r	$\sigma_e^2 + (qk-1)\sigma_\rho^2/q$
Component (a)	$(q-1)(k^2-1)$	—	$\sigma_e^2 + k\sigma_\rho^2$
Component (b)	(k^2-1)	—	$\sigma_e^2 + (k-1)\sigma_\rho^2$

The component (a) and (b) sums of squares are of the same form as for one-restrictional lattices and for the semi-balanced lattice squares. The sums of squares for row (eliminating both treatment and column effects) and for column (eliminating both row and treatment effects) add a new feature to the analysis. The method is illustrated below with an example. Also, Homeyer *et al.* [160] describe the numerical procedure on both punched card and calculating machines; their symbolism is retained in the present example.

Example XII-2. An illustrative example of a $k^2 = 3^2$ balanced lattice square in $k+1=4$ replicates with the row, column, and replicate totals is given in table XII-4. Effect A is confounded with rows in replicate I and columns in replicate III, effect B with columns in replicate I and rows in replicate III, effect AB with rows in replicate II and columns in replicate IV, and effect AB^2 with columns in replicate II and rows in replicate IV.

Table XII-4 contains the totals necessary for computing the analysis of variance. The column headed variety total $= V$ contains the totals of the varieties from the four replicates. The total yield for variety 01 is $3+2+2+3=10$. Column 3 with the heading SR represents the sum of the row totals in which the variety appeared. For example, the value SR for variety 02 is obtained as $14+9+13+9=45$. The sum of the SR values should equal k times the grand total, $kG = 3(146) = 438$. The column headed SC contains the total of column sums from the individual replicates for a variety. For variety 01 the SC value is $13+7+13+13=46$. The sum of the SC's should equal k times the grand total, $3(146) = 438$. Column 5 is the difference of columns 3 and 4; thus, for variety 01, $SR - SC = 44 - 46 = -2 = D$. The L' values in column 6 are obtained as $kV + G - (k+1)SR = L'$; for variety 10, $L' = 3(12) + 146 - (3+1)47 = -6$. The J values are the sum of the corresponding D and L' values; for variety 21, $J = D + L' = -3 + 1 = -2$. $K = J + (k-1)D$; for variety 20, $K = J + (k-1)D = 4 + (3-1)(0) = 4$. $M' = D + K$; for variety 00, $M' = D + K = 1 + 0 = 1$. The sum of any of the columns 5 to 9 should equal zero exactly. As a further check, $M' = kV + G - (k+1)SC$; for variety 01, $M' = 3(10) + 146 - (3+1)46 = -8$, which checks with the value obtained before.

The replicate, variety (ignoring row and column), and the total sums of squares (table XII-5) are computed in the usual manner. The various row and column sums of squares are computed as the sums of squares of the L', J, K, and M' values. The row (eliminating variety effect but ignoring column effect) sum of squares with $(k+1)(k-1) = k^2 - 1 = 8$ degrees of freedom is

TABLE XII-4. Yields, experimental layout, and totals for a balanced lattice square design with 3^2 varieties in four replicates (Variety numbers in parentheses)

Replicate I								Replicate II							
(12)	2	(10)	3	(11)	7		12	(10)	3	(01)	2	(22)	7		12
(02)	3	(00)	8	(01)	3		14	(02)	4	(20)	2	(11)	3		9
(22)	6	(20)	5	(21)	3		14	(21)	2	(12)	3	(00)	6		11
	11		16		13		40		9		7		16		32

Replicate IV								Replicate III							
(21)	2	(10)	3	(02)	4		9	(20)	4	(00)	7	(10)	3		14
(12)	3	(01)	3	(20)	3		9	(22)	5	(02)	4	(12)	4		13
(00)	8	(22)	7	(11)	5		20	(21)	2	(01)	2	(11)	5		9
	13		13		12		38		11		13		12		36

Variety totals and other totals used in analysis; adjusted means

Variety No.	Variety total = V	SR	SC	SR − SC = D	kV + G − (k + 1)SR = L'	D + L' = J	J + (k − 1)D = K	D + K = M'	λL' + μM'	Adj. means
00	29	59	58	1	−3	−2	0	1	−.0765	7.231
01	10	44	46	−2	0	−2	−6	−8	−.7416	2.315
02	15	45	45	0	11	11	11	11	1.6401	4.160
10	12	47	50	−3	−6	−9	−15	−18	−2.0070	2.498
11	20	50	53	−3	6	3	−3	−6	−.2178	4.946
12	12	45	43	2	2	4	8	10	1.0398	3.260
20	14	46	46	0	4	4	4	4	.5964	3.649
21	9	43	46	−3	1	−2	−8	−11	−.9633	2.009
22	25	59	51	8	−15	−7	9	17	.7299	6.432
Sum	146 = G	438	438	0	0	0	0	0	0.0000	36.500

$$\frac{\sum(L')^2}{k^3(k+1)} = \frac{(-3)^2 + 0^2 + \cdots + 1^2 + (-15)^2}{27(4)} = 4.148. \qquad \text{(XII-14)}$$

The row (eliminating variety and column effect) sum of squares with $r(k-1) = 8$ degrees of freedom is

$$\frac{\sum J^2}{k^3(k-1)} = \frac{(-2)^2 + (-2)^2 + \cdots + (-2)^2 + (-7)^2}{27(2)} = 5.630. \qquad \text{(XII-15)}$$

The column (eliminating variety effect and ignoring row effect) sum of squares with $r(k-1) = 8$ degrees of freedom is

$$\frac{\sum(M')^2}{k^3(k+1)} = \frac{1^2 + (-8)^2 + \cdots + (-11)^2 + 17^2}{27(4)} = 9.926. \qquad \text{(XII-16)}$$

The column (eliminating both variety and row effects) sum of squares with $(k+1)(k-1) = 8$ degrees of freedom is

$$\frac{\sum K^2}{k^3(k-1)} = \frac{0^2 + (-6)^2 + \cdots + (-8)^2 + 9^2}{27(2)} = 11.407. \qquad \text{(XII-17)}$$

The sum of the row (eliminating variety and ignoring column effects) and the column

TABLE XII-5. Analysis of variance

Source of variation	df	ss	ms
Replicate	3	3.89	1.30
Variety (ign. row and column)	8	96.89	12.11
Error (r. c. b.)	24	19.11	0.796
Row (ign. column; elim. variety)	8	4.148	-
Column (elim. row and variety)	8	11.407	1.426 = E_c
Column (elim. variety; ign. row)	8	9.926	-
Row (elim. variety and column)	8	5.630	0.704 = E_r
Intrablock	8	3.554	0.444 = E_e
Total	35	119.89	-
Correction for mean	1	592.11	-

(eliminating variety and row effects) sums of squares should equal the sum of the column (eliminating variety and ignoring row effects) and the row (eliminating variety and column effects) sums of squares within rounding errors; thus: 4.148 + 11.407 = 15.555, and 9.926 + 5.630 = 15.556. Either of the above sums is subtracted from the randomized complete block error sum of squares to obtain the intrablock error sum of squares, 19.11 − 15.556 = 3.554, with $k(k^2 - 1) - 2(k^2 - 1) = k^3 - k - 2k^2 + 2 = 8$ degrees of freedom.

The row weighting factor is

$$\lambda = \frac{(E_r - E_e)(kE_c - E_e)}{(k-1)(k^2E_rE_c - E_e^2)} = \frac{(0.704 - 0.444)[3(1.426) - 0.444]}{(3-1)[9(0.704)(1.426) - (0.444)^2]} = 0.0564, \quad \text{(XII-18)}$$

and the column weighting factor is

$$\mu = \frac{(E_c - E_e)(kE_r - E_e)}{(k-1)(k^2E_rE_c - E_e^2)} = \frac{(1.426 - 0.444)[3(0.704) - 0.444]}{(3-1)[9(0.704)(1.426) - (0.444)^2]} = 0.0927. \quad \text{(XII-19)}$$

The adjustment for a variety total is $\mu M' + \lambda L'$; for variety 00 the adjustment is (.0927)(1) + (.0564)(−3) = − 0.0765, and the adjusted mean is [29 + (−.0765)]/4 = 7.231.

The standard error of a mean difference between any two variety means is

$$\sqrt{\frac{2E_e}{k+1}[1 + k(\lambda + \mu)]} = \sqrt{\frac{2(0.444)}{4}[1 + 3(0.1491)]} = 0.567. \quad \text{(XII-20)}$$

There is a single standard error of a mean difference, since all comparisons are of equal accuracy.

The average effective error variance is

$$E_e[1 + k(\lambda + \mu)] = 0.444[1 + 3(0.1491)] = 0.643, \quad \text{(XII-21)}$$

and the efficiency of the balanced lattice square relative to the randomized complete block design is

$$\frac{\text{randomized complete block error}}{E_e[1 + k(\lambda + \mu)]} \times 100 = \frac{0.796}{0.643} \times 100 = 124 \text{ per cent.} \quad \text{(XII-22)}$$

XII-2.3 OTHER LATTICE SQUARES

It is possible to set up lattice rectangle designs for $k(k-1)$ treatments, but the usefulness of such designs in experimental work is limited. As an example, suppose that fifteen workers of three skills are available, that these workers are capable of performing only four treatments per period, that twenty treatments are to be compared, and that the fourteen batches of material differ with regard to the characteristic of interest. A possible design is the following:

Skill I

Batch No.	Worker No. 1	2	3	4	5
1	1	2	3	4	5
2	6	7	8	9	10
3	11	12	13	14	15
4	16	17	18	19	20
	—	—	—	—	—

Skill II

Batch No.	Worker No. 6	7	8	9	10
5	1	10	14	18	—
6	—	2	6	15	19
7	20	—	3	7	11
8	12	16	—	4	8
9	9	13	17	—	5

Skill III

Batch No.	Worker No. 11	12	13	14	15
10	1	8	15	17	—
11	—	2	9	11	18
12	19	—	3	10	12
13	13	20	—	4	6
14	7	14	16	—	5

Designs of the above type are constructed by deleting the last k treatments from a set of treatments numbered from one to k^2. In such designs, each pair of treatments is compared either in a "row" (batch) or in a "column" (worker). Also, two or more of the arrangements may be used. If worker and batch variation estimates are of no interest and if a fifteenth batch is available, an alternative design is the triple rectangular lattice design with the worker and batch differences being completely confounded; i.e., each worker completes one batch.

The partially balanced lattice squares consist of lattice squares with 2, 3, $\cdots$, $(k-1)/2$ arrangements of the $k \times k$ lattice square [52, 178]. For example, if three arrangements of the 7×7 lattice square are desired, the following scheme of confounding may be used:

Pseudo-effect confounded with	Arrangement 1	2	3
Rows	A	AB	AB^3
Columns	B	AB^2	AB^4

If a fourth arrangement, confounding the AB^5 and AB^6 effects with rows and columns, respectively, is used, a semi-balanced lattice square results. If a fifth arrangement (e.g., confounding A with columns and B with rows) is used, the design becomes unbalanced. Designs of both types are illustrated in sections XII-2.3.1 and XII-2.3.2.

Another type of confounding suitable for $k = 6$, 10, and 12 has been proposed [106, 178]. The three pseudo-effects A, B, C from a set of k^2 treatments are each confounded once with row and once with column differences. Also,

each effect is unconfounded in one of the three arrangements (table XII-8). For $k = 12$ it is possible to set up four arrangements such that each of four pseudo-effects is confounded once with rows and once with columns and each is unconfounded in two of the arrangements.

A fourth type of lattice square has been proposed by Kempthorne [175, sec. 24.4] for p^n treatments in a lattice rectangle of p^s columns and p^r rows $(s + r = n)$. For example, thirty-two treatments could be arranged in eight columns and four rows. Kempthorne proposes a scheme of confounding of the pseudo-effects in three arrangements such that intrablock information is obtained on all effects.

XII-2.3.1 Analysis for 2, 3, $\cdots$, $(k - 1)/2$ arrangements. If one or more replicates of a semi-balanced lattice square is lost, if it is necessary to use fewer replicates than $(k + 1)/2$ because of lack of material or facilities, or if a greater degree of precision is not required, the result is a lattice square design with less than $(k + 1)/2$ arrangements [52, 175, 178]. A $k \times k$ lattice square in two replicates is selected to illustrate the method of analysis for partially balanced lattice squares. Suppose that pseudo-effects A and AB are confounded with row differences and B and AB^2 with column differences in the two arrangements; thus:

Pseudo-effect confounded with	Replicate 1	Replicate 2
Rows	A	AB
Columns	B	AB²

If $X_{1..}$, $X_{2..}$, $X_{.ij}$, and $X_{...}$ represent the replicate 1, replicate 2, treatment, and grand totals, respectively, and if the totals for levels of effects are represented as before, the analysis of variance and the sums of squares are as given in table XII-6. The sums of squares for row (eliminating treatment) and for column (eliminating treatment) are of the same form as the component (*b*) sum of squares in the double lattice design.

The weights are estimated from the following formulae:

$$w = 1/E_e. \tag{XII-23}$$

$$w_r = 1/(2E_r - E_e). \tag{XII-24}$$

$$w_c = 1/(2E_c - E_e). \tag{XII-25}$$

The row adjustment factor for the differences $(A)_{.u} - 2(A)_{1u}$ and $(AB)_{.u} - 2(AB)_{2u}$ is equal to

$$\frac{w - w_r}{k(w + w_r)} \tag{XII-26}$$

The column adjustment factor for the differences $(B)_{.u} - 2(B)_{1u}$ and $(AB^2)_{.u} - 2(AB^2)_{2u}$ is equal to

$$\frac{w - w_c}{k(w + w_c)} \tag{XII-27}$$

TABLE XII-6. Analysis of variance for a k $\times$ k lattice square in 2 replicates

Source of variation	df	ss	ms Observed	ms Expected value
Replicate	1	$\frac{X^2_{1..} + X^2_{2..}}{k^2} - \frac{X^2_{...}}{2k^2}$	-	-
Treatment (ignoring row and column effects)	$k^2 - 1$	$\Sigma\Sigma \frac{X^2_{.ij}}{2} - \frac{X^2_{...}}{2k^2}$	-	-
Remainder = estimate of randomized complete block error	$k^2 - 1$	By subtraction	E_e'	-
Row (eliminating treatment)	$2(k - 1)$	$\sum_{u=0}^{k-1} \left\{ \frac{[(A)_{.u} - 2(A)_{1u}]^2 + [(AB)_{.u} - 2(AB)_{2u}]^2}{2k} \right\} - \frac{2(X_{1..} - X_{2..})^2}{2k^2}$	E_r	$\sigma_\epsilon^2 + k\sigma_\rho^2/2$
Column (eliminating treatment)	$2(k - 1)$	$\sum_{u=0}^{k-1} \left\{ \frac{[(B)_{.u} - 2(B)_{1u}]^2 + [(AB^2)_{.u} - 2(AB^2)_{2u}]^2}{2k} \right\} - \frac{2(X_{1..} - X_{2..})^2}{2k^2}$	E_c	$\sigma_\epsilon^2 + k\sigma_\gamma^2/2$
Intrablock error	$(k - 1)(k - 3)$	By subtraction or by direct computation as the interaction of levels of effects $AB^3, \ldots, AB^{k-1}$ with replicates	E_e	σ_ϵ^2
Total	$2k^2 - 1$	$\Sigma\Sigma\Sigma X^2_{gij} - X^2_{...}/2k^2$		

Four adjustments, two each for rows and columns, are added to the unadjusted total to obtain the adjusted treatment total.

The average effective error variance is

$$\frac{2}{k+1}\left\{\frac{2}{w+w_r}+\frac{2}{w+w_c}+\frac{k-3}{2w}\right\}, \tag{XII-28}$$

and the average standard error of a difference between two adjusted means is the square root of formula (XII-28). The efficiency of this lattice square design is $100E_e'$ divided by the average effective error variance (formula (XII-28)).

XII-2.3.2 Analysis for more than $(k+1)/2$ arrangements. If more than $(k+1)/2$ but fewer than $(k+1)$ arrangements are used, the analysis becomes slightly more difficult. To illustrate, suppose that four replicates of a 5×5 lattice square are used with the following system of confounding of the pseudo-effects in the four replicates:

Pseudo-effect confounded with	Replicate 1	2	3	4
Rows	A	AB	AB^3	B
Columns	B	AB^2	AB^4	A

The first three replicates form a semi-balanced lattice square.

If the system described in section XII-2.3.1 is utilized, the analysis of variance is of the form presented in table XII-7. The formulae for the various sums of squares are given in the table, along with the expectation of the various mean squares.

The weights are estimated from the mean squares in table XII-7 by the following formulae:

$$w = 1/E_e, \tag{XII-29}$$

$$w_r = 17/(24E_r - 7E_e), \tag{XII-30}$$

and

$$w_c = 17/(24E_c - 7E_e). \tag{XII-31}$$

The various weighting factors are

$$\frac{w-w_c}{5(w_r+w_c+2w)}, \tag{XII-32}$$

$$\frac{w-w_r}{5(w_r+w_c+2w)}, \tag{XII-33}$$

$$\frac{w-w_r}{5(w_r+3w)}, \tag{XII-34}$$

and

$$\frac{w-w_c}{5(w_c+3w)}. \tag{XII-35}$$

TABLE XII-7. Analysis of variance for 4 replicates of a 5 × 5 lattice square

Source of variation	df		ms values Obs.	Exp.
Replicate	3	$\Sigma X^2_{g..}/25 - X^2_{...}/100$	-	-
Treatment (ign. row and column)	24	$\Sigma\Sigma X^2_{.ij}/4 - X^2_{...}/100$	-	-
Remainder = est. of r. c. b. error	72	By subtraction	E_e'	-
Row (elim. trt.; ign. column)	16	$\frac{1}{60}\Sigma\left\{[(A)_{.u}-4(A)_{1u}]^2+[(AB)_{.u}-4(AB)_{2u}]^2+[(AB^3)_{.u}-4(AB^3)_{3u}]^2+[(B)_{.u}-4(B)_{4u}]^2\right\}$ $-[(X_{...}-4X_{1..})^2+(X_{...}-4X_{2..})^2+(X_{...}-4X_{3..})^2+(X_{...}-4X_{4..})^2]/300$	-	-
Column (elim. trt. and row)	16	$\frac{1}{30}\Sigma\left\{[(A)_{.u}-3(A)_{4u}-(A)_{1u}]^2+[(B)_{.u}-3(B)_{1u}-(B)_{4u}]^2\right\}+\frac{1}{60}\Sigma\left\{[(AB^2)_{.u}-4(AB^2)_{2u}]^2+[(AB^4)_{.u}-4(AB^4)_{3u}]^2\right\}$ $-[(X_{2..}+X_{3..}-2X_{1..})^2+(X_{2..}+X_{3..}-2X_{4..})^2]/150-[(X_{...}-4X_{2..})^2+(X_{...}-4X_{3..})^2]/300$	E_c	$\sigma^2_\epsilon+85\sigma^2_\gamma/24$
Intrablock	40	By subtraction or by direct computation of levels of effects with replicates in which the effects are unconfounded	E_e	σ^2_ϵ
Total	99	$\Sigma\Sigma\Sigma X^2_{gij} - X^2_{...}/100$	-	-
Row (elim. trt. and column	15	$\frac{1}{30}\Sigma\left\{[(A)_{.u}-3(A)_{1u}-(A)_{4u}]^2+[(B)_{.u}-3(B)_{4u}-(B)_{1u}]^2\right\}+\frac{1}{60}\Sigma\left\{[(AB)_{.u}-4(AB)_{2u}]^2+[(AB^3)_{.u}-4(AB^3)_{3u}]^2\right\}$ $-\frac{1}{150}[(X_{2..}+X_{3..}-2X_{1..})^2+(X_{2..}+X_{3..}-2X_{4..})^2]-\frac{1}{300}[(X_{...}-4X_{2..})^2+(X_{...}-4X_{3..})^2]$	E_r	$\sigma^2_\epsilon+85\sigma^2_\rho/24$
Column (elim. trt; ign. row)	16	$\Sigma\left\{[(B)_{.u}-4(B)_{1u}]^2+[(AB^2)_{.u}-4(AB^2)_{2u}]^2+[(AB^4)_{.u}-4(AB^4)_{3u}]^2+[(A)_{.u}-4(A)_{4u}]^2\right\}/60$ $-[(X_{...}-4X_{1..})^2+(X_{...}-4X_{2..})^2+(X_{...}-4X_{3..})^2+(X_{...}-4X_{4..})^2]/300$	-	-

Formula (XII-33) is the weighting factor for the differences $(A)_{\cdot u} - 4(A)_{1u}$ and $(B)_{\cdot u} - 4(B)_{4u}$; formula (XII-32) is the weighting factor for the differences $(A)_{\cdot u} - 4(A)_{4u}$ and $(B)_{\cdot u} - 4(B)_{1u}$; formula (XII-34) is the weighting factor for the differences $(AB)_{\cdot u} - 4(AB)_{2u}$ and $(AB^3)_{\cdot u} - 4(AB^3)_{3u}$; and formula (XII-35) is the weighting factor for the differences $(AB^2)_{\cdot u} - 4(AB^2)_{2u}$ and $(AB^4)_{\cdot u} - 4(AB^4)_{3u}$. After multiplying each of the differences by the appropriate weighting factor, the four row adjustments and the four column adjustments corresponding to any treatment are added to the unadjusted treatment total to obtain the adjusted total.

The average effective error variance of the above design is

$$\frac{4}{3}\left\{\frac{1}{w_r + w_c + 2w} + \frac{1}{w_r + 3w} + \frac{1}{w_c + 3w}\right\}. \tag{XII-36}$$

The average standard error of the mean difference for any pair of adjusted treatment means is

$$\sqrt{\frac{2}{3}\left\{\frac{1}{w_r + w_c + 2w} + \frac{1}{w_r + 3w} + \frac{1}{w_c + 3w}\right\}}. \tag{XII-37}$$

average efficiency of this design relative to the randomized complete is $100E_e'$ divided by the value of formula (XII-36); E_e' is obtained from XII-7.

II-2.3.3 Lattice square designs for $k = 6$, 10, and 12 in three cates. Lattice square designs discussed in the preceding two sections or values of k equal to a prime number or power of a prime number. the scheme of confounding for prime numbers is not appropriate for and 10, the following scheme of confounding in the rows and columns and 100 treatments is suggested [106, 178]:

Pseudo-effect confounded with	Arrangement 1	2	3
Rows	B	C	A
Columns	A	B	C

Each of the three effects is confounded once with row differences and once with column differences, and each is unconfounded in one of the arrangements. One method of constructing such designs for $k = 6$, 10, and 12 is to add row and column numbers to the numbers in a $k \times k$ latin square such that the first set of numbers refers to the A effect, the second to the B effect, and the third to the C effect. This is the method used to construct the arrangements in table XII-8. An added advantage of this system of numbering is in the analysis of such designs on punched card equipment. The various levels of each effect are already coded. The use of an x and a y in the 12×12 lattice square facilitates the coding for punched card analyses.

Also, the first two digits, i and j, may be likened to the levels of effects in

TABLE XII-8. Incomplete lattice square arrangements for 36, 100, and 144 treatments

6 x 6 incomplete lattice square

Arrangement I

000	101	202	303	404	505
015	110	211	312	413	514
024	125	220	321	422	523
033	134	235	330	431	532
042	143	244	345	440	541
051	152	253	354	455	550

Arrangement II

000	110	220	330	440	550
101	211	321	431	541	051
202	312	422	532	042	152
303	413	523	033	143	253
404	514	024	134	244	354
505	015	125	235	345	455

Arrangement III

000	051	042	033	024	015
110	101	152	143	134	125
220	211	202	253	244	235
330	321	312	303	354	345
440	431	422	413	404	455
550	541	532	523	514	505

10 x 10 incomplete lattice square

Arrangement I

000	101	202	303	404	505	606	707	808	909
019	110	211	312	413	514	615	716	817	918
028	129	220	321	422	523	624	725	826	927
037	138	239	330	431	532	633	734	835	936
046	147	248	349	440	541	642	743	844	945
055	156	257	358	459	550	651	752	853	954
064	165	266	367	468	569	660	761	862	963
073	174	275	376	477	578	679	770	871	972
082	183	284	385	486	587	688	789	880	981
091	192	293	394	495	596	697	798	899	990

Arrangement II

000	110	220	330	440	550	660	770	880	990
101	211	321	431	541	651	761	871	981	091
202	312	422	532	642	752	862	972	082	192
303	413	523	633	743	853	963	073	183	293
404	514	624	734	844	954	064	174	284	394
505	615	725	835	945	055	165	275	385	495
606	716	826	936	046	156	266	376	486	596
707	817	927	037	147	257	367	477	587	697
808	918	028	138	248	358	468	578	688	798
909	019	129	239	349	459	569	679	789	899

Arrangement III

000	091	082	073	064	055	046	037	028	019
110	101	192	183	174	165	156	147	138	129
220	211	202	293	284	275	266	257	248	239
330	321	312	303	394	385	376	367	358	349
440	431	422	413	404	495	486	477	468	459
550	541	532	523	514	505	596	587	578	569
660	651	642	633	624	615	606	697	688	679
770	761	752	743	734	725	716	707	798	789
880	871	862	853	844	835	826	817	808	899
990	981	972	963	954	945	936	927	918	909

TABLE XII-8. (*continued*)

12 x 12 incomplete lattice square

Arrangement I

000	101	202	303	404	505	606	707	808	909	x0x	y0y
01y	110	211	312	413	514	615	716	817	918	x19	y1x
02x	12y	220	321	422	523	624	725	826	927	x28	y29
039	13x	23y	330	431	532	633	734	835	936	x37	y38
048	149	24x	34y	440	541	642	743	844	945	x46	y47
057	158	259	35x	45y	550	651	752	853	954	x55	y56
066	167	268	369	46x	56y	660	761	862	963	x64	y65
075	176	277	378	479	57x	67y	770	871	972	x73	y74
084	185	286	387	488	589	68x	78y	880	981	x82	y83
093	194	295	396	497	598	699	79x	89y	990	x91	y92
0x2	1x3	2x4	3x5	4x6	5x7	6x8	7x9	8xx	9xy	xx0	yx1
0y1	1y2	2y3	3y4	4y5	5y6	6y7	7y8	8y9	9yx	xyy	yy0

Arrangement II

000	110	220	330	440	550	660	770	880	990	xx0	yy0
101	211	321	431	541	651	761	871	981	x91	yx1	0y1
202	312	422	532	642	752	862	972	x82	y92	0x2	1y2
303	413	523	633	743	853	963	x73	y83	093	1x3	2y3
404	514	624	734	844	954	x64	y74	084	194	2x4	3y4
505	615	725	835	945	x55	y65	075	185	295	3x5	4y5
606	716	826	936	x46	y56	066	176	286	396	4x6	5y6
707	817	927	x37	y47	057	167	277	387	497	5x7	6y7
808	918	x28	y38	048	158	268	378	488	598	6x8	7y8
909	x19	y29	039	149	259	369	479	589	699	7x9	8y9
x0x	y1x	02x	13x	24x	35x	46x	57x	68x	79x	8xx	9yx
y0y	01y	12y	23y	34y	45y	56y	67y	78y	89y	9xy	xyy

Arrangement III

000	0y1	0x2	093	084	075	066	057	048	039	02x	01y
110	101	1y2	1x3	194	185	176	167	158	149	13x	12y
220	211	202	2y3	2x4	295	286	277	268	259	24x	23y
330	321	312	303	3y4	3x5	396	387	378	369	35x	34y
440	431	422	413	404	4y5	4x6	497	488	479	46x	45y
550	541	532	523	514	505	5y6	5x7	598	589	57x	56y
660	651	642	633	624	615	606	6y7	6x8	699	68x	67y
770	761	752	743	734	725	716	707	7y8	7x9	79x	78y
880	871	862	853	844	835	826	817	808	8y9	8xx	89y
990	981	972	963	954	945	936	927	918	909	9yx	9xy
xx0	x91	x82	x73	x64	x55	x46	x37	x28	x19	x0x	xyy
yy0	yx1	y92	y83	y74	y65	y56	y47	y38	y29	y1x	y0y

a $k \times k$ factorial. This is the method followed in the analysis of the 3×3 numerical example selected to illustrate the computational procedure for the incomplete lattice square. It should be re-emphasized that better schemes of confounding are available for values of k other than 6, 10, and 12. For $k = 12$ it is possible to set up four arrangements such that each of four effects is confounded once with rows and once with columns, and each is unconfounded in two replicates. The randomization procedure follows that for other lattice squares.

Example XII-3. Uniformity trial data are available [340] for yield of ear corn from plots four hills wide by five hills long. Using these data, three replicates each twelve by fifteen hills (nine plots) were constructed. The variety numbers, 000, 011,

022, 101, 112, 120, 202, 210, and 221 are assigned to three rows and three columns in a group or arrangement in such a way that like numbers of the first subscript, say i, are in the same row and like numbers of the second subscript, say j, are in the same column in group I; like numbers of the second subscript, j, are in the same row and of the third subscript, say h, in the same column in group II; and the like numbers of the third subscript are in the same row and like numbers of the first subscript are in the same column in group III. After constructing these arrangements the groups are randomly assigned to the replicates, and the rows are randomized and then the columns are randomized, thus retaining the above described arrangement for the three replicates.

TABLE XII-9. 3 × 3 incomplete lattice square in 3 replicates (pounds of ear corn per 4 × 5 hill plot; variety numbers in parentheses)

Replicate I				Row total
	(202) 27.0	(221) 24.6	(210) 27.5	79.1
	(101) 28.8	(120) 26.1	(112) 27.4	82.3
	(000) 27.6	(022) 27.8	(011) 26.9	82.3
Column total	83.4	78.5	81.8	243.7
Replicate II				**Row total**
	(120) 27.9	(221) 27.7	(022) 30.9	86.5
	(000) 25.6	(101) 26.7	(202) 28.3	80.6
	(210) 28.5	(011) 27.8	(112) 26.4	82.7
Column total	82.0	82.2	85.6	249.8
Replicate III				**Row total**
	(112) 27.0	(202) 26.1	(022) 24.8	77.9
	(101) 29.1	(221) 28.4	(011) 24.4	81.9
	(120) 30.2	(210) 29.0	(000) 30.5	89.7
Column total	86.3	83.5	79.7	249.5
Total over all replicates				743.0

Another way of considering the arrangement in table XII-9 is to consider only the first two subscripts or the subscripts for the pseudo-factors a and b (this method works only for k equal to a power of a prime number). Following the notation of previous chapters, the k^2 treatments are designated as 00, 01, 02, 10, 11, 12, 20, 21, and 22. The pseudo-effects are A, B, AB, and $AB^2 = AB^{k-1}$. The pseudo-effects A and B are con-

founded with row and column differences, respectively, in replicate I; B and AB with row and column differences, respectively, in replicate II; and AB and A with row and column differences, respectively, in replicate III. The remaining two-factor interaction, AB^2, is unconfounded.

The variety (treatment) designations (in parentheses) and the plot yields are given (table XII-9) for the $k^2 = 3^2$ plots in each of the three replicates. The row, column, and replicate totals are given also.

For the two methods of analysis presented below (pseudo-effects; row and column totals), the total, replicate, and variety (ignoring row and column effects) sums of squares are computed in the usual manner for a randomized complete block design.

(i) Method of pseudo-effects

For this method of analysis it is necessary to construct a table of totals for the levels of the effects A, B, AB, and AB^2 for each of the replicates. The $k = 3$ treatment combinations making up the zero level of effect A, or $(A)_0$, are 000, 011, and 022 (the last subscript will be omitted hereafter) = 00, 01, and 02. The $(A)_0$ total (of $k = 3$ plots) in replicate I is

$$(A)_{10} = 27.6 + 26.9 + 27.8 = 82.3.$$

The totals for the levels of all effects are presented for each replicate in table XII-10. The weighted levels of each effect (lower half of table XII-10) are explained later. The various levels of effects A and B in replicate I may be obtained from the row and column totals; also, the row and column totals may be used to obtain the 0, 1, and 2 levels of effects B and AB, and AB and A in replicates II and III, respectively.

TABLE XII-10. Totals for levels of each of the pseudo-effects A, B, AB, and AB² by replicate and weighted effects per level

Pseudo-effect	Replicate I			Replicate II			Replicate III		
	level =			level =			level =		
	0	1	2	0	1	2	0	1	2
A	82.3	82.3	79.1	84.3	81.0	84.5	79.7	86.3	83.5
B	83.4	81.8	78.5	80.6	82.7	86.5	85.7	80.4	83.4
AB	81.2	80.3	82.2	82.0	82.2	85.6	89.7	81.9	77.9
AB²	79.6	84.1	80.0	79.7	86.1	84.0	85.9	82.9	80.7

$(k + 1)X_{...} = 4(743.0) = 2972.0$

Weighted effects per level

	0	1	2
A	81.4069	84.1870	83.1475
B	83.6560	81.5270	81.0804
AB	82.8911	81.6078	83.4964
AB²	81.7333	84.3667	81.5667

3(Total) 3(990.6668) = 2972.0004

(i-1) Sum of squares for row (eliminating variety and ignoring column effects)

The comparison, by levels, of the mean of the effect confounded in rows with the mean of the effect in the remaining replicates yields the sum of squares for row (elimi-

nating variety and ignoring column effects). The sum of squares for effect A is

$$\frac{[84.3 + 79.7 - 2(82.3)]^2 + [81.0 + 86.3 - 2(82.3)]^2 + [84.5 + 83.5 - 2(79.1)]^2}{6k = 18} - \frac{[249.8 + 249.5 - 2(243.7)]^2}{6k^2 = 54} = 3.1382,$$

with $k - 1 = 2$ degrees of freedom. The pooled sum of squares for A, B, and AB with $3(k - 1) = 6$ degrees of freedom is

$$3.1382 + 10.1226 + 22.1137 = 35.3745.$$

(i-2) Sum of squares for column (eliminating both variety and row effects)

The comparison of the level of an effect unconfounded with row or column differences with the level of the effect confounded with column differences yields the sum of squares for column (eliminating the row and variety effects). For example, effect B is confounded with columns in replicate I and is unconfounded in replicate III; the resulting sum of squares is

$$\frac{[85.7 - 83.4]^2 + [80.4 - 81.8]^2 + [83.4 - 78.5]^2}{2k = 6} - \frac{[249.5 - 243.7]^2}{2k^2 = 18}$$
$$= 3.3411, \text{ with } (k - 1) = 2 \text{ degrees of freedom.}$$

The pooled sum of squares of this comparison of effects A, B, and AB is

$$8.3700 + 3.3411 + 0.5678 = 12.2789,$$

with $3(k - 1) = 6$ degrees of freedom.

(i-3) Sum of squares for column (eliminating variety and ignoring row effects)

The comparison which yields the sum of squares for column (eliminating variety and ignoring row effects) is similar to that for row (eliminating variety and ignoring column effects). The sum of squares for the comparison of the level of an effect unconfounded with columns with the effect confounded with column differences for effect A is

$$\frac{[82.3 + 84.3 - 2(79.7)]^2 + [82.3 + 81.0 - 2(86.3)]^2 + [79.1 + 84.5 - 2(83.5)]^2}{6k = 18} - \frac{[243.7 + 249.8 - 2(249.5)]^2}{6k^2 = 54} = 7.7670,$$

with $k - 1 = 2$ degrees of freedom. The pooled sum of squares for effects A, B, and AB is

$$7.7670 + 6.6504 + 9.0004 = 23.4178,$$

with $3(k - 1) = 6$ degrees of freedom.

(i-4) Sum of squares for row (eliminating both variety and column effects)

The sum of squares for row (eliminating both variety and column effects) is similar to the sum of squares due to column (eliminating both variety and row effects). The sum of squares for the comparison of effect A confounded with rows in replicate I and unconfounded in replicate II is

$$\frac{[84.3 - 82.3]^2 + [81.0 - 82.3]^2 + [84.5 - 79.1]^2}{2k = 6} - \frac{[249.8 - 243.7]^2}{2k^2 = 18}$$
$$= 3.7411, \text{ with } k - 1 = 2 \text{ degrees of freedom.}$$

The pooled sum of squares for A, B, and AB is

$$3.7411 + 6.8133 + 13.6811 = 24.2355,$$

with a total of $3(k-1) = 6$ degrees of freedom.

The sums of squares given in (i-1) and (i-2) should sum to those in (i-3) and (i-4) within rounding errors;

$$35.3745 + 12.2789 = 47.6534 \text{ and}$$
$$23.4178 + 24.2355 = 47.6533.$$

As a partial check the sum of the values in the correction terms in either of the four sums of squares given above should equal zero; for example, in (i-1),

$$11.9 - 6.4 - 5.5 = 0.$$

The intrablock error sum of squares may be obtained by subtracting the sums of squares for row (eliminating variety; ignoring column) and for column (eliminating both variety and row) from the randomized block error sum of squares; thus:

$$58.4926 - 35.3745 - 12.2789 = 10.8392,$$

with $16 - 6 - 6 = 4 = 2(k-2)(k-1)$ degrees of freedom (table XII-11). Also, the intrablock error sum of squares may be computed directly as the interaction sums of squares of levels of the effects and the replicates in which the effects are unconfounded. The only effect unconfounded in more than one replicate is AB^2, which is unconfounded in all three replicates. The interaction sum of squares of levels of effect AB^2 with replicates is obtained from the following two-way table:

Level of effect AB^2	Replicate I	II	III	Total
0	79.6	79.7	85.9	245.2
1	84.1	86.1	82.9	253.1
2	80.0	84.0	80.7	244.7
Total	243.7	249.8	249.5	743.0

$$\frac{79.6^2 + \cdots + 80.7^2}{k = 3} - \frac{245.2^2 + \cdots + 244.7^2}{3k = 9} - \frac{243.7^2 + \cdots + 249.5^2}{k^2 = 9}$$
$$+ \frac{(743.0)^2}{3k^2 = 27} = 20{,}464.6600 - 20{,}451.1933 - 20{,}448.8867 + 20{,}446.2593$$

$= 10.8393$, with $2(k-1) = 4$ degrees of freedom. This sum of squares is equal, within rounding errors, to that obtained by subtraction (table XII-11).

From the mean squares and the expectations of mean squares in table XII-11, it is possible to obtain the various row, column, and intrablock weights used for adjusting the variety means. The reciprocal of the expected intrablock variance is defined as the amount of information, w, for intrablock comparisons and is estimated by the reciprocal of the intrablock error mean square,

$$w = \frac{1}{E_e} = \frac{1}{2.7098} = 0.369031. \tag{XII-38}$$

TABLE XII-11. Analysis of variance

Source of variation	df	ss	ms	Expectation of ms
Replicate	2	2.6274	1.31	
Variety (ign. row and column)	8	11.6207	1.45	
Error (r. c. b.)	16	58.4926	3.6558	
Row (elim. variety; ign. column)	6	35.3745	-	-
Column (elim. variety and row)	6	12.2789	2.0465 = E_c	$\sigma_\epsilon^2 + k\sigma_\gamma^2/2$
Column (elim. variety; ign. row)	6	23.4178	-	-
Row (elim. variety and column)	6	24.2355	4.0392 = E_r	$\sigma_\epsilon^2 + k\sigma_\rho^2/2$
Intrablock	4	10.8392	2.7098 = E_e	σ_ϵ^2
Total	26	72.7407	-	-

Likewise, the reciprocals of the expected row $(\sigma_\epsilon^2 + k\sigma_\rho^2)$ and column $(\sigma_\epsilon^2 + k\sigma_\gamma^2)$ variances are defined to be the amount of information obtained on row and column effects, w_r and w_c, respectively. They are estimated by

$$w_r = \frac{1}{2E_r - E_e} = \frac{1}{5.3686} = 0.186268, \qquad \text{(XII-39)}$$

and

$$w_c = \frac{1}{2E_c - E_e} = \frac{1}{1.3832} = 0.722961. \qquad \text{(XII-40)}$$

The amount of information obtained on row and column comparisons theoretically should be no greater than the information on within row and column comparisons. However, sample estimates of w_r and w_c are sometimes larger than w. Whenever this is true, the value that is larger is taken equal to w. This results in zero corrections to the mean for the row or column. In this example, w_c is larger than w and therefore is set equal to w, resulting in zero adjustments for columns. *Since this is an illustrative example*, the rule will *not* be followed and the estimates of w_r, w_c, and w as found will be used.

The levels of effect A are estimated with variance $1/w_r$ in replicate I, $1/w$ in replicate II, and $1/w_c$ in replicate III. Weighting a level of an effect (table XII-10) inversely to the variance with which it is estimated, a weighted mean (of k plots) is obtained. The weighted mean (of k plots) for the one level of A (lower half of table XII-10) is equal to

$$\frac{0.186268(82.3) + 0.369031(81.0) + 0.722961(86.3)}{0.369031 + 0.722961 + 0.186268 = w + w_c + w_r} = 84.1870.$$

The remaining weighted levels of the effects (table XII-10) are obtained in a similar manner. The weighted levels of AB^2 are obtained as the average from the three replicates, since the amount of information in each replicate is equal to w.

Using the weighted levels of the effects from table XII-10 and the designated levels

in table XII-12 the weighted mean for, say, variety 01 = 011 is

$$\frac{1}{k}\Big((A)_0 + (B)_1 + (AB)_1 + \cdots + (AB^{k-1})_{k-1}\Big) - k\bar{x} \qquad \text{(XII-41)}$$

$$= \frac{1}{3}\Big(81.4069 + 81.5270 + 81.6078 + 81.5667\Big) - \frac{3(743.0)}{27} = 26.15.$$

The remaining adjusted means (table XII-12) are obtained similarly.

TABLE XII-12. Table for obtaining levels of an effect to use in adjusting variety means, adjusted and unadjusted variety means

Treatment or variety combination	Level of effect $(A)_i$	$(B)_j$	$(AB)_{i+j}$	$(AB^2)_{i+2j}$	Mean Adjusted	Unadjusted
000	0	0	0	0	27.34	27.90
011	0	1	1	2	26.15	26.37
022	0	2	2	1	27.56	27.83
101	1	0	1	1	28.72	28.20
112	1	1	2	0	27.76	26.93
120	1	2	0	2	27.35	28.07
202	2	0	2	2	28.07	27.13
210	2	1	0	1	28.09	28.33
221	2	2	1	0	26.63	26.90
Total	9	9	9	9	247.67	247.66

(ii) Method of row and column totals

The method of analysis for a $k \times k$ design constructed in an incomplete lattice square, as described earlier, may be generalized for k = any integer by making use of the row and column totals in the three replicates. In order to do this, it is advantageous to construct a table similar to table XII-14. It is not necessary to systematize the variety yields for each plot, but this may as well be done if the yields are recopied. Otherwise, the additional computations in table XII-14 could be included in table XII-9.

TABLE XII-13. Variety totals in the 3 arrangements used to construct replicates I, II, and III (Variety numbers in parentheses)

	Replicate I			V_A = Total		Replicate II			V_B = Total
	(000) 83.7	(011) 79.1	(022) 83.5	246.3		(000) 83.7	(101) 84.6	(202) 81.4	249.7
	(101) 84.6	(112) 80.8	(120) 84.2	249.6		(210) 85.0	(011) 79.1	(112) 80.8	244.9
	(202) 81.4	(210) 85.0	(221) 80.7	247.1		(120) 84.2	(221) 80.7	(022) 83.5	248.4
Total = V_B	249.7	244.9	248.4	743.0	Total = V_C	252.9	244.4	245.7	743.0

	Replicate III			V_C = Total
	(000) 83.7	(120) 84.2	(210) 85.0	252.9
	(011) 79.1	(101) 84.6	(221) 80.7	244.4
	(022) 83.5	(112) 80.8	(202) 81.4	245.7
Total = V_A	246.3	249.6	247.1	743.0

TABLE XII-14. Systematized arrangement of variety yields, totals necessary for computation of analysis of variance, and adjustments for variety means

Replicate I (ab arrangement)	Plot yields	Total in row $= R_A$	V_A	V_A-3R_A	$V_A-2R_A-C_A$ $= d$	d'	R_A-C_A $= e$	e'
	(000) (011) (022) 27.6 26.9 27.8	82.3	246.3	-0.6	2.0	.032	2.6	-.121
	(101) (112) (120) 28.8 27.4 26.1	82.3	249.6	2.7	-1.3	-.021	-4.0	.187
	(202) (210) (221) 27.0 27.5 24.6	79.1	247.1	9.8	5.4	.086	-4.4	.205
Total in col.= C_B	83.4 81.8 78.5	243.7			6.1		-5.8	
V_B	249.7 244.9 248.4		743.0					
V_B-3C_B	-0.5 -0.5 12.9			11.9				
$V_B-2C_B-R_B=f$	2.3 -1.4 4.9	5.8						
f'	-.071 .043 -.151							
Replicate II (bc arrangement)		R_B	V_B	V_B-3R_B	$V_B-2R_B-C_B$ $= d$	d'	R_B-C_B $= e$	e'
	(000) (101) (202) 25.6 26.7 28.3	80.6	249.7	7.9	5.1	.081	-2.8	.131
	(210) (011) (112) 28.5 27.8 26.4	82.7	244.9	-3.2	-2.3	-.037	0.9	-.042
	(120) (221) (022) 27.9 27.7 30.9	86.5	248.4	-11.1	-3.1	-.049	8.0	-.373
C_C	82.0 82.2 85.6	249.8			-0.3		6.1	
V_C	252.9 244.4 245.7		743.0					
V_C-3C_C	6.9 -2.2 -11.1			- 6.4				
$V_C-2C_C-R_C=f$	-0.8 -1.9 - 3.4	-6.1						
f'	.025 .058 .105							
Replicate III (ca arrangement)		R_C	V_C	V_C-3R_C	$V_C-2R_C-C_C$ $= d$	d'	R_C-C_C $= e$	e'
	(000) (120) (210) 30.5 30.2 29.0	89.7	252.9	-16.2	-8.5	-.135	7.7	-.359
	(011) (101) (221) 24.4 29.1 28.4	81.9	244.4	- 1.3	-1.6	-.025	-0.3	.014
	(022) (112) (202) 24.8 27.0 26.1	77.9	245.7	12.0	4.3	.068	-7.7	.359
C_A	79.7 86.3 83.5	249.5			-5.8		-0.3	
V_A	246.3 249.6 247.1		743.0					
V_A-3C_A	7.2 -9.3 - 3.4			- 5.5				
$V_A-2C_A-R_A=f$	4.6 -5.3 1.0							
f'	-.142 .163 -.031	0.3						

Prior to constructing table XII-14, it may be advantageous to construct a table (table XII-13) of varietal totals arranged in the same manner as the varieties are in the three replicates. The totals V_A, V_B, and V_C are obtained (table XII-13) for use in table XII-14. The totals R_i or C_i ($i = A$, B, or C) are obtained by summing the plot

yields in the rows and columns, respectively. R_A in replicate I for the first row is $27.6 + 26.9 + 27.8 = 82.3$. As a partial check the sum of the R's or C's in a replicate is equal to the replicate total. The quantity $V - 3R$ is obtained as the sum of the variety totals in a row, minus three times the sum of the plot yields in a row for the same varieties. For example, the first $V_A - 3R_A$ is

$$246.3 - 3(82.3) = -0.6.$$

The quantities $V - 3C$ are obtained similarly. As a partial check the sum of the quantities $V - 3R$ is equal to the sum of the $V - 3C$ within each replicate. Also, the sum of the $V - 3R$ values in any particular replicate is equal to the grand total, 743.0, minus three times the replicate total. In replicate I,

$$-0.6 + 2.7 + 9.8 = -0.5 - 0.5 + 12.9 = 11.9 = 743.0 - 3(243.7).$$

The next quantities to be computed are the $d = V_i - 2R_i - C_i$ values; d is the sum of the variety totals in a row minus two times the row total minus the sum of the same variety yields in the replicate in which the varieties appear together in the same column. The first $V_A - 2R_A - C_A$ value is

$$d = 246.3 - 2(82.3) - 79.7 = 2.0.$$

The remaining quantities, $V - 2R - C$ and $V - 2C - R$, are computed similarly. The sum of these quantities within a replicate should equal the grand total minus 3 times the replicate total. For replicate I, then,

$$6.1 + 5.8 = 11.9 = 743.0 - 3(243.7).$$

The quantities $R_i - C_i$ are computed from the row totals involving the same varieties as the column totals. The first $R_B - C_B$ value in replicate II is

$$80.6 - 83.4 = -2.8.$$

As a partial check the sum of the $R_i - C_i$ in a replicate should equal the difference between the two replicates involved. In replicate I, the sum of the $R_A - C_A$ is equal to $2.6 - 4.0 - 4.4 = -5.8 = 243.7 - 249.5$. The calculations for the d', e', and f' values are explained later. They are the adjustments for the variety means. As a further partial check, the sum of any of the quantities, $V_i - 3R_i$, $V_i - 3C_i$, $V_i - 2R_i - C_i$, $V_i - 2C_i - R_i$, or $R_i - C_i$, should equal zero.

(ii-1) Sum of squares for row (eliminating variety and ignoring column effects)

The within-replicate sums of squares of the quantities $V_i - 3R_i$ yield the sum of squares for row (eliminating variety and ignoring column effects) with $3(k - 1) = 6$ degrees of freedom; thus:

$$\frac{(-0.6)^2 + (2.7)^2 + (9.8)^2}{6k = 18} - \frac{(11.9)^2}{6k^2 = 54} + \frac{(7.9)^2 + (-3.2)^2 + (-11.1)^2}{18} - \frac{(-6.4)^2}{54}$$
$$+ \frac{(-16.2)^2 + (-1.3)^2 + (12.0)^2}{18} - \frac{(-5.5)^2}{54} = 35.3745.$$

(ii-2) Sum of squares for column (eliminating both row and variety effects)

The sum of squares for column (eliminating both row and variety effects) is

obtained from the within-replicate sums of squares of the quantities, $V_i - 2C_i - R_i$:

$$\frac{(2.3)^2 + (-1.4)^2 + (4.9)^2}{2k = 6} - \frac{(5.8)^2}{2k^2 = 18} + \frac{(-0.8)^2 + (-1.9)^2 + (-3.4)^2}{6} - \frac{(-6.1)^2}{18}$$

$$+ \frac{(4.6)^2 + (-5.3)^2 + (1.0)^2}{6} - \frac{(0.3)^2}{18} = 12.2789, \text{ with } 3(k-1) = 6 \text{ degrees of freedom.}$$

(ii-3) Sum of squares for column (eliminating variety and ignoring row effects)

The within-replicate sum of squares of the quantities $V_i - 3C_i$ is

$$\frac{(-0.5)^2 + (-0.5)^2 + (12.9)^2}{6k = 18} - \frac{(11.9)^2}{6k^2 = 54} + \frac{(6.9)^2 + (-2.2)^2 + (-11.1)^2}{18} - \frac{(-6.4)^2}{54}$$

$$+ \frac{(7.2)^2 + (-9.3)^2 + (-3.4)^2}{18} - \frac{(-5.5)^2}{54} = 23.4178.$$

(ii-4) Sum of squares for row (eliminating variety and column effects)

The sum of squares for row (eliminating variety and column effects) is obtained from the within-replicates sums of squares of the quantities $V_i - 2R_i - C_i$,

$$\frac{(2.0)^2 + (-1.3)^2 + (5.4)^2}{2k = 6} - \frac{(6.1)^2}{2k^2 = 18} + \frac{(5.1)^2 + (-2.3)^2 + (-3.1)^2}{6} - \frac{(-0.3)^2}{18}$$

$$+ \frac{(-8.5)^2 + (-1.6)^2 + (4.3)^2}{6} - \frac{(-5.8)^2}{18} = 24.2356, \text{ with } 3(k-1) = 6 \text{ degrees}$$

of freedom.

As a check the sum of squares of sections (ii-1) plus (ii-2) should equal the sum of those from (ii-3) and (ii-4):

$$35.3745 + 12.2789 = 47.6534 = 23.4178 + 24.2356.$$

The sums of squares obtained in section (ii) are identical to those obtained in section (i), within rounding errors, and are summarized in table XII-11. The weights w, w_r, and w_c are the same as obtained previously. For the method of row and column totals the following combinations of weights are needed to obtain the adjustments for the variety means:

$$\frac{w - w_r}{3k(w_r + w_c + w)} = .015886, \qquad \text{(XII-42)}^1$$

$$\frac{w - w_c}{3k(w_r + w_c + w)} = -.030765, \qquad \text{(XII-43)}^1$$

and

$$\frac{w_r - w_c}{3k(w_r + w_c + w)} = -.046651. \qquad \text{(XII-44)}$$

The adjusted variety means are obtained by adding nine adjustments, three from each of the three replicates, to the unadjusted variety mean. The adjustments are obtained by multiplying the d values by $\frac{w - w_r}{3k(w_r + w_c + w)}$, the e values by $\frac{w_r - w_c}{3k(w_r + w_c + w)}$

[1]Whenever a weighting factor is negative, the factor is set equal to zero; this rule is not followed here, as it is desired to illustrate the numerical procedure.

and the f values by $\dfrac{w - w_c}{3k(w_r + w_c + w)}$ to form the d', e', and f' values, respectively.[1] Thus, the first d', e', and f' values in replicate II of table XII-14 are

$$d' = .015886\,(5.1) = .081,$$
$$e' = -.046651\,(-2.8) = .131,$$
and
$$f' = -.030765\,(-0.8) = .025.$$

As a partial check the sum of the d', of the e', and of the f' values should equal zero, within rounding errors. The nine adjustments for a variety mean are those in the same row and column in each of the replicates as the variety plot yield. The adjusted mean for variety 022 is

$$\frac{27.8 + 30.9 + 24.8}{3} + .032 - .121 - .151 - .049 - .373 + .105$$
$$+ .068 + .359 - .142 = 27.83 - .272 = 27.56.$$

The remaining adjusted means (table XII-15) are obtained similarly.

TABLE XII-15. Unadjusted and adjusted variety means for 3^2 varieties in an incomplete lattice square of 3 replicates

Variety number	Unadjusted mean	Sum of 9 adjustments	Adjusted mean
000	27.90	-.559	27.34
011	26.37	-.220	26.15
022	27.83	-.272	27.56
101	28.20	.517	28.72
112	26.93	.825	27.76
120	28.07	-.713	27.36
202	27.13	.933	28.06
210	28.33	-.245	28.08
221	26.90	-.266	26.63
Total	247.66	-.000	247.66

The average effective error variance per plot is

$$\left(\frac{3}{2}\right)\left(\frac{2}{k+1}\right)\left(\frac{3}{w_r + w_c + w} + \frac{k-2}{3w}\right) \tag{XII-45}$$
$$= \frac{3}{4}[3(0.782313) + (0.903266)] = 2.4377.$$

The efficiency of this design relative to the randomized complete block design is the ratio of the average effective error variances for the two designs; thus:

$$\frac{3.6558}{2.4377} \times 100 = 150 \text{ per cent.}$$

The coefficient of variation is the square root of the average effective error variance divided by the mean of the experiment:

$$\frac{\sqrt{2.4377}}{743.0/27} = 6 \text{ per cent.}$$

[1] Alternatively, one may multiply the $V_i - 3R_i$ values by $(w - w_r)/3k(w_r + w_c + w)$ and the $V_i - 3C_i$ values by $(w - w_c)/3k(w_r + w_c + w)$ to obtain the adjustments in each arrangement for the variety means. These six adjustments, two in each replicate, sum to the same value obtained for the nine adjustments described above.

The standard error of a difference between two adjusted means for a pair of varieties which appear together in a row and in a column (e.g., 000 and 011) is

$$\sqrt{\frac{2}{k}\left(\frac{2}{w_r + w_c + w} + \frac{k-2}{3w}\right)} = \sqrt{\frac{2}{3}[2(0.782313) + 0.903266]} = 1.28. \quad \text{(XII-46)}$$

The standard error of a difference between two adjusted means for a pair of varieties which do not appear together in a row and in a column (e.g., 000 and 112) is

$$\sqrt{\frac{2}{k}\left(\frac{3}{w_r + w_c + w} + \frac{k-3}{3w}\right)} = \sqrt{\frac{2}{3}(3)(0.782313)} = 1.25. \quad \text{(XII-47)}$$

Ordinarily the latter standard error will be larger than the former one, since the comparisons within rows and columns are more precise than between rows and columns. The above discrepancy is caused by the fact that w_c is larger than w; it should have been set equal to w for this case, but was not because it is desired to illustrate the computational procedure. The average standard error of a mean difference between any two adjusted variety means is

$$\sqrt{\frac{2}{k+1}\left(\frac{3}{w_r + w_c + w} + \frac{k-2}{3w}\right)} = \sqrt{\frac{2}{4}[3(0.782313) + 0.903266]} = 1.27. \quad \text{(XII-48)}$$

As a partial check the value of the last standard error should fall between the former two.

XII-2.3.4 Incomplete lattice square in $3q$ replicates. The arrangements in table XII-8 may be repeated to obtain 6, 9, $\cdots$, $3q$ replicates of an incomplete lattice square design. A different randomization is utilized for each set, as described earlier. The general form of the analysis of variance is

Source of variation	df	Mean square	
		Observed	Expected value
Replicate	$3q - 1$	—	—
Treatment (ignoring row and column)	$k^2 - 1$	—	—
Row (eliminating treatment; ignoring column)	$3(k - 1)$	—	—
Column (eliminating treatment and row)	$3(k - 1)$		
Column × replicate in which effects are confounded	$3(q - 1)(k - 1)$	E_c	$\sigma_e^2 + (2q - 1)k\sigma_\gamma^2/2q$
Row × replicate in which effects are confounded	$3(q - 1)(k - 1)$	E_r	$\sigma_e^2 + (2q - 1)k\sigma_\rho^2/2q$
Row (elim. treatment & col.)	$3(k - 1)$		
Column (elim. treat.; ign. row)	$3(k - 1)$		
Intrablock error	$3q(k - 1)^2 - k^2 + 1$	E_e	σ_e^2

The interaction of rows and replicates and of columns and replicates sums of squares are computed as described previously for the component (a) sum of squares. The expectations of the two mean squares are $\sigma_e^2 + k\sigma_\rho^2$ and $\sigma_e^2 + k\sigma_\gamma^2$, respectively.

Example XII-4. If an additional randomization of the three arrangements in table XII-9 is used for a second set of three replicates, the analysis follows that given in example XII-3 except for a few additional calculations. The computational procedure for nine replicates or three sets of the original three incomplete lattice squares follows directly from that given below.

Uniformity trial data on the pounds of ear corn from a 4 × 5 hill plot were used to construct the additional set of replicates (table XII-16). The arrangement is the same in replicates I and VI, II and IV, and in III and V. In replicates I and VI the first subscript remains the same throughout a row, the second subscript remains the same throughout a column, and the third subscript varies from 0, 1, to $2 = k - 1$ within a row and within a column. In terms of pseudo-effects, the levels of effect A are confounded with row differences and of effect B with column differences. The effects AB and AB^2 are unconfounded with row or column differences in replicates I and VI.

TABLE XII-16. Second set of 3 replicates of a 3 × 3 incomplete lattice square (see table XII-9 for the first 3 replicates) Pounds of ear corn per 4 × 5 hill plot

Replicate IV (Same basic arrangement as replicate II)				Row total	Replicate V (Same basic arrangement as replicate III)				Row total
	(210) 29.0	(112) 24.5	(011) 27.3	80.8		(112) 27.9	(202) 24.5	(022) 26.1	78.5
	(120) 28.3	(022) 27.9	(221) 27.5	83.7		(120) 26.4	(210) 25.4	(000) 25.1	76.9
	(000) 26.4	(202) 25.4	(101) 28.1	79.9		(101) 23.1	(221) 28.7	(011) 26.5	78.3
Col. total	83.7	77.8	82.9	244.4	Col. total	77.4	78.6	77.7	233.7

Replicate VI (Same basic arrangement as replicate I)				Row total
	(210) 23.8	(202) 23.8	(221) 24.7	72.3
	(011) 22.4	(000) 24.4	(022) 27.3	74.1
	(112) 23.1	(101) 24.8	(120) 25.6	73.5
Col. total	69.3	73.0	77.6	219.9

The sums and differences necessary to complete the analysis of variance (table XII-19) for two sets of the 3 × 3 incomplete lattice square are given in tables XII-16 to XII-18. The various sums of squares should present no difficulty, but if they do, complete details are given in reference 106.

The weights are

$$w = 1/E_e = .354258, \qquad \text{(XII-49)}$$

$$w_r = 3/(4E_r - E_e) = .509200, \qquad \text{(XII-50)}$$

and

$$w_c = 3/(4E_c - E_e) = .347673. \qquad \text{(XII-51)}$$

TABLE XII-17. Form for calculations

Rep. I and VI AB arrangement	Plot yields			Row total R_A	V_A	$c_r = V_A - 3R_A$	$d = V_A - 2R_A - C_A$	c_r'
	(000) 52.0	(011) 49.3	(022) 55.1	156.4	479.7	10.5	9.5	-.0746
	(101) 53.6	(112) 50.5	(120) 51.7	155.8	481.4	14.0	6.1	-.0995
	(202) 50.8	(210) 51.3	(221) 49.3	151.4	479.9	25.7	15.0	-.1827
Column total								
C_B	156.4	151.1	156.1	463.6			30.6	-.3568
V_B	475.3	474.8	490.9		1441.0			
$c_c = V_B - 3C_B$	6.1	21.5	22.6			50.2		
$f = V_B - 2C_B - R_B$	2.0	9.1	8.5	19.6				
c_c'	.0018	.0065	.0068	.0151				
Rep. II and IV BC arrangement				R_B	V_B	$c_r = V_B - 3R_B$	$d = V_B - 2R_B - C_B$	c_r'
	(000) 52.0	(101) 54.8	(202) 53.7	160.5	475.3	- 6.2	- 2.1	.0441
	(210) 57.5	(011) 55.1	(112) 50.9	163.5	474.8	-15.7	- 3.3	.1116
	(120) 56.2	(221) 55.2	(022) 58.8	170.2	490.9	-19.7	- 5.6	.1400
C_C	165.7	165.1	163.4	494.2			-11.0	.2957
V_C	487.3	477.5	476.2		1441.0			
$c_c = V_C - 3\,C_C$	- 9.8	- 17.8	- 14.0			-41.6		
$f = V_C - 2V_C - R_C$	- 10.7	- 12.9	- 7.0	- 30.6				
c_c'	-.0030	-.0054	-.0042	-.0126				
Rep. III and V CA arrangement				R_C	V_C	$c_r = V_C - 3R_C$	$d = V_C - 2R_C - C_C$	c_r'
	(000) 55.6	(120) 56.6	(210) 54.4	166.6	487.3	-12.5	-11.6	.0888
	(011) 50.9	(101) 52.2	(221) 57.1	160.2	477.5	- 3.1	- 8.0	.0220
	(022) 50.9	(112) 54.9	(202) 50.6	156.4	476.2	7.0	0.0	-.0498
C_A	157.4	163.7	162.1	483.2			-19.6	.0610
V_A	479.7	481.4	479.9		1441.0			
$c_c = V_A - 3C_A$	7.5	- 9.7	- 6.4			- 8.6		
$f = V_A - 2C_A - R_A$	8.5	- 1.8	4.3	11.0				
c_c'	.0023	-.0029	-.0019	-.0025				

TABLE XII-18. Row and column totals by replicate

Row totals

Row	Rep. I	Rep. VI	Total	Row	Rep. II	Rep. IV	Total
0..	82.3	74.1	156.4	.0.	80.6	79.9	160.5
1..	82.3	73.5	155.8	.1.	82.7	80.8	163.5
2..	79.1	72.3	151.4	.2.	86.5	83.7	170.2
Total	243.7	219.9	463.6	Total	249.8	244.4	494.2

Row	Rep. III	Rep. V	Total
..0	89.7	76.9	166.6
..1	81.9	78.3	160.2
..2	77.9	78.5	156.4
Total	249.5	233.7	483.2

Column totals

Column	Rep. I	Rep. VI	Total	Column	Rep. II	Rep. IV	Total
.0.	83.4	73.0	156.4	..0	82.0	83.7	165.7
.1.	81.8	69.3	151.1	..1	82.2	82.9	165.1
.2.	78.5	77.6	156.1	..2	85.6	77.8	163.4
Total	243.7	219.9	463.6	Total	249.8	244.4	494.2

Column	Rep. III	Rep. V	Total
0..	79.7	77.7	157.4
1..	86.3	77.4	163.7
2..	83.5	78.6	162.1
Total	249.5	233.7	483.2

TABLE XII-19. Analysis of variance

Source of variation	df	ss	ms
Total	53	200.4081	-
Correction for mean	1	38435.3519	-
Replicate	5	73.6525	14.7305
Variety (ign. row and column)	8	19.3814	2.4227
Error (r. c. b.)	40	107.3742	2.6844 = E_e'
Row (elim. variety; ign. column)	6	11.4750	-
Column (elim. variety and row)	6	8.5361	2.8629 = E_c
Column x replicate	6	25.8189	
Intrablock	16	45.1652	2.8228 = E_e
Row x replicate	6	16.3789	
Row (elim. variety and column)	6	9.7644	2.1786 = E_r
Column (elim. variety; ign. row)	6	10.2467	-

Since $w_r > w$, row adjustments should not be made, and E_r should be set equal to E_e. However, in order to illustrate the procedure, this is not done in the present example. The weighting factors for obtaining adjustments for the means are

$$\frac{w - w_r}{3qk(w + w_r + w_c)} = -.0071073, \tag{XII-52}$$

$$\frac{w - w_c}{3qk(w + w_r + w_c)} = .0003021, \tag{XII-53}$$

and

$$\frac{w_r - w_c}{3qk(w + w_r + w_c)} = .0074094. \tag{XII-54}$$

For this example the alternative procedure is followed in obtaining the adjustments for the various means. The values $V_i - 3R_i$ are each multiplied by equation (XII-52)

and the values $V_i - 3C_i$ by equation (XII-53). Two adjustments are obtained for a variety mean in each arrangement. Therefore, the total number of adjustments is six. The adjusted mean is equal to the unadjusted mean plus the sum of the corresponding six adjustments (table XII-20). For example, the adjusted mean for variety 022 is

$$\frac{164.8}{6} - .0746 + .0068 + .1400 - .0042 - .0498 + .0023 = 27.49.$$

TABLE XII-20. Unadjusted and adjusted variety means

Variety	Unadjusted Total	Unadjusted Mean	Sum of 6 adjustments	Adjusted means
000	159.6	26.6000	.0594	26.66
011	155.3	25.8833	.0624	25.95
022	164.8	27.4667	.0205	27.49
101	160.6	26.7667	-.0399	26.73
112	156.3	26.0500	-.0383	26.01
120	164.5	27.4167	.1302	27.55
202	155.1	25.8500	-.1927	25.66
210	163.2	27.2000	.0193	27.22
221	161.6	26.9333	-.0212	26.91
Total	1441.0	240.1667	-.0003	240.18

The standard error of a difference between two adjusted means for a pair of varieties appearing in the same row and column is

$$\sqrt{\frac{2}{kq}\left(\frac{2}{w_r + w_c + w} + \frac{k-2}{3w}\right)} = .93. \qquad \text{(XII-55)}$$

The standard error of a mean difference between a pair of varieties not appearing in the same row and column is

$$\sqrt{\frac{2}{kq}\left(\frac{3}{w_r + w_c + w} + \frac{k-3}{3w}\right)} = .91. \qquad \text{(XII-56)}$$

The average standard error of a difference for any pair of adjusted means is

$$\sqrt{\frac{2}{q(k+1)}\left(\frac{3}{w_r + w_c + w} + \frac{k-2}{3w}\right)} = .92. \qquad \text{(XII-57)}$$

The average effective error variance is

$$\frac{3}{k+1}\left(\frac{3}{w_r + w_c + w} + \frac{k-2}{3w}\right) = 2.563. \qquad \text{(XII-58)}$$

XII-3 k^3 Treatments in k Whole Plots, k Split Plots, and k Split Split Plots, A Two-Restrictional Design

As experimental conditions warrant, the following scheme of confounding may be utilized:

Arrangement	Whole plots	Split plots
I	A	B and A × B interactions
II	B	C and B × C "
III	C	A and A × C "

The above scheme of confounding is similar to that used in a cubic lattice. The only difference is that the pseudo-main effects are confounded with incomplete blocks of size k^2 instead of k. The randomization procedure differs in that the k levels of a pseudo-main effect are randomly allotted to the whole plots of size k^2. Then, the levels of the second pseudo-main effect are randomly allotted to the k incomplete blocks of size k within each whole plot. Finally, the treatments within a split plot are randomly allotted to the split split plots (the experimental units) within each split plot. (The randomization procedure in each replicate is the same as for a split split plot design.)

The sums of squares for the analysis of variance are computed in the same manner as for a cubic lattice. The expectations of some mean squares are different from those for the cubic lattice; thus:

Source of variation	df	Expected value of ms
Replicate	2	—
Treatment (ignoring whole and split plot effects)	$k^3 - 1$	—
Component (a)	$3(k-1)$	$\sigma_e^2 + k\sigma_\beta^2 + k^2\sigma_\alpha^2/2$
Component (b)	$3(k-1)$	$\sigma_e^2 + k\sigma_\beta^2/3 + k^2\sigma_\alpha^2/3$
Component (c)	$3(k-1)^2$	$\sigma_e^2 + 2k\sigma_\beta^2/3$
Intrablock	$2k^3 - 3k^2 + 1$	σ_e^2

The true weights are

$$w = 1/\sigma_e^2, \tag{XII-59}$$

$$w_s = 1/(\sigma_e^2 + k\sigma_\beta^2), \tag{XII-60}$$

and

$$w_p = 1/(\sigma_e^2 + k\sigma_\beta^2 + k^2\sigma_\alpha^2). \tag{XII-61}$$

The average variance of a difference between two adjusted means is

$$\frac{2}{k^2+k+1}\left(\frac{3}{w+w_s+w_p} + \frac{3(k-1)}{2w+w_s} + \frac{(k-1)^2}{3w}\right). \tag{XII-62}$$

In using such a design the experimenter should first determine the number of degrees of freedom associated with the various mean squares. If fewer than 12 to 14 are available, one may be well advised to use additional replicates or to use a one-restrictional lattice design instead of the present two-restrictional design, unless the amount of variation removed by whole plots is considerable.

XII-4 Three-Dimensional Three-Restrictional Designs

Various arrangements of k^3 treatments with three restrictions on the allocation of treatments within a complete block are available. Two of these schemes of confounding of the pseudo-effects are

Scheme I	k^2 whole plots		Whole plots
	Rows	Columns	
Replicate I	A	B	All A × B interactions
" II	B	C	All B × C "
" III	C	A	All A × C "

Scheme II	k whole plots	k^2 split plots	
		Rows	Columns
Replicate I	A	BC & A × BC	BC^2 & A × BC^2
" II	B	AC^2 & B × AC^2	AC & B × AC
" III	C	A & A × C	B & B × C

For 3^3 treatments the average error variance for Scheme I in $3q$ replicates is

$$\frac{2}{13q}\left(\frac{3}{w_r + w_c + w} + \frac{6}{w_s + 2w} + \frac{4}{3w}\right), \tag{XII-63}$$

and for Scheme II is equal to

$$\frac{2}{13q}\left(\frac{1}{w_p' + w_r' + w'} + \frac{1}{w_p' + w_c' + w'} + \frac{1}{w_p' + 2w'} + \frac{2}{3w'} + \frac{4}{w_r' + w_c' + w'} + \frac{2}{2w_r' + w'} + \frac{2}{2w_c' + w'}\right). \tag{XII-64}$$

Federer [104] and Kempthorne [175, sec. 24.5] discuss Scheme I; Yates [324] presents the design for Scheme II. Also, Kempthorne [175, sec. 24.5] treats a variation of Scheme II. A variation of Scheme I is to use kp^2 treatments in a $p \times p$ lattice square with split plots of k treatments. In addition, confounding in the split plots might be used [171].

XII-5 Four-Dimensional Lattices with Two or More Restrictions

Designs for k^4 treatments may have two, three, or four restrictions placed on the allotment of the treatments within the complete block. Lattice square designs of k^2 rows and k^2 columns may be used. Such a design may be analyzed as an ordinary lattice square. Also, one could use two-restrictional designs of the following nature:

Design	Number of		
	Whole plots	Split plots	Split split plots
1	k^2	k	k
2	k	k^2	k
3	k	k	k^2

The following three-restrictional designs are available for k^4 treatments:

Design	Number of			
	Whole plots	Split plots	Split split plots	Split split split plots
1	k	k	k	k
2	k^2	$k \times k$ lattice sq.	—	—
3	$k \times k$ lattice sq.	k^2	—	—

A four-restrictional design that may be useful for some experimental conditions is a $k \times k$ lattice square of the whole plots with the split plots in each whole plot arranged as a $k \times k$ lattice square.

XII-6 Missing Data

Cornish [67, 69] has developed missing plot formulae for the semi-balanced and balanced lattice squares; his formulae (without recovery of interblock information) for these designs are

$$X_{gij}' = \frac{k(k-1)(R+C) - (k+3)X_{g..} + 2k(k-2)X_{.ij} + 6X_{...} - 2k(B+V)}{(k-3)(k-1)^2} \tag{XII-65}$$

and

$$X_{gij}' = \frac{k(k-1)(R+C) - (k+1)X_{g..} + k(k-1)X_{.ij} + 3X_{...} - k(B+V)}{(k-2)(k-1)^2}, \tag{XII-66}$$

respectively. R, C, $X_{g..}$, and $X_{.ij}$ represent the row, column, replicate, and treatment totals, respectively, that contain the missing observation; $X_{...}$ is the total of all $rk^2 - 1$ observations; B is the sum of row and column totals in the other squares which contain the treatment whose value is missing in R

and C (i.e., B is the sum of the row and column totals containing the treatment); V is the sum of the $2(k-1)$ treatment totals for the treatments making up R and C. For each missing plot value computed, one degree of freedom is subtracted from the intrablock error degrees of freedom. Otherwise, after inserting the computed values, the analysis is completed in the same manner as for no missing values.

Missing plot formulae for most other designs discussed herein and for other situations have not been developed to date. The methods described by Cornish [67, 69] may be applied to obtain missing formulae for these designs.

XII-7 Tests of Significance

Tests of significance of the adjusted treatment means for the designs in this chapter are in a less advanced state than for those in the preceding chapter. It is recommended that the following test be used to approximate the correct test:

$$F\,(\text{treatment and intrablock error } df) = \frac{(\text{ms among adjusted treatment means})}{\text{average effective error variance}}. \tag{XII-67}$$

The above test was suggested by Bliss and Dearborn [25] for a semi-balanced lattice square. With such a test, it is possible to partition the treatment degrees of freedom and to test the resulting mean squares.

An alternative test would be to obtain the treatment (eliminating both row and column effects) mean square and compare it with the intrablock error mean square. Such a test is exact if the ordinary assumptions of additivity and normality hold, but it is inefficient, since the interrow and intercolumn information is ignored. Also, contrasts among groups of treatments are much more difficult to obtain by this method.

CHAPTER XIII

Other Incomplete Block Designs

XIII-1 Introduction

In addition to the designs discussed in Chapters IX through XII, there are other incomplete block designs which merit discussion because of their usefulness in certain experimental situations. The partially balanced incomplete block design and the balanced incomplete block design are two useful designs for controlling the variation among the b incomplete blocks of size k each, with the treatments not in complete replicates. Also, incomplete block designs with two-way elimination of variation may be used to control two sources of variability even if the treatments are not in complete replicates. In still other experimental situations a variation of the split plot design may be used to obtain more accurate comparisons on the treatments within a natural subgroup than on the treatments in different natural subgroups.

XIII-2 Incomplete Block Designs for v Treatments in b Incomplete Blocks of Size k

The designs in the present section were first proposed by Yates [320, 322] in 1936. He constructed several of the designs and suggested a large number of others. Since 1936, many useful designs of this type have been discovered and constructed [26, 27, 28, 30, 32, 175, 176, 226, 227, 255, 324, 326, 327, see 113 for additional references]. Two basic types of incomplete blocks are discussed in this section; i.e., the balanced incomplete block (b.i.b.) and the partially balanced incomplete block (p.b.i.b.) designs for v treatments in b incomplete blocks of size k with each treatment repeated r times.

The randomization procedure for these designs follows:

(i) Allot the entry numbers to the treatments at random unless there is a specific reason for not doing so.
(ii) Allot the groups to the b blocks at random.
(iii) Randomly allot the treatments to the k experimental units within each block.

In the event that it is possible and desirable to arrange the treatments in complete replicates (these designs are called *resolvable* incomplete block

designs by Bose and Nair [30]), the randomization procedure is the same as described in Chapter XI for any one-restrictional lattice design.

The analysis for resolvable incomplete block designs is the same as the analysis for the b.i.b. and p.b.i.b. designs described in the following two sections *except that* the replicate sum of squares is removed from the block (eliminating treatment) sum of squares. The expectation of E_b then becomes $\sigma_\epsilon^2 + k(r-1)\sigma_\beta^2/r$ instead of $\sigma_\epsilon^2 + (bk - v)\sigma_\beta^2/(b-1)$, as given below.

XIII-2.1 BALANCED INCOMPLETE BLOCK DESIGNS

XIII-2.1.1 One set. The particular lattice design composed of v treatments in b incomplete blocks of size k with the treatments not arranged in complete replicates is often referred to as the balanced incomplete block design [30, 60, 141, 175, 255, 320, 322] or as the symmetrical incomplete randomized block design [320]. This class of designs is useful for experimental situations requiring control of the variability only among the b incomplete blocks.

For v treatments in $b(b \geq v)$ incomplete blocks of size $k(k < v)$ with r replicates on each treatment the condition of balance is fulfilled if each pair of treatments occurs together in the incomplete blocks an equal number, λ, of times. Also, the following conditions must hold:

$$vr = kb. \tag{XIII-1}$$

$$(v-1)\lambda = r(k-1). \tag{XIII-2}$$

The efficiency factor, Eff, is defined [327] as the fraction of total information contained in intrablock comparisons when interblock and intrablock contrasts are of equal accuracy. The efficiency factor is equal to

$$\text{Eff} = \frac{1 - 1/k}{1 - 1/v} = \frac{v(k-1)}{k(v-1)} = \frac{v\lambda}{rk}. \tag{XIII-3}$$

The analysis of variance table for the balanced incomplete block design is [327]:

Source of variation	df	ss	Mean square Observed	Mean square Expected value
Treatment (ignoring block)	$v-1$	T_1	—	—
Block (elim. treatment)	$b-1$	B_1	E_b	$\sigma_\epsilon^2 + (kb - v)\sigma_\beta^2/(b-1)$
Intrablock	$kb - v - b + 1$	E	E_e	σ_ϵ^2
Total	$kb - 1$	—	—	—
Treatment (elim. block)	$v-1$	T_2	—	$\sigma_\epsilon^2 + r\sigma_\tau^2$
Block (ignoring treat.)	$b-1$	B_2	—	—
Intrablock	$kb - v - b + 1$	E	E_e	σ_ϵ^2

where

$$T_1 = \sum_{j=1}^{v} \frac{X_{.j}^2}{r} - \frac{X_{..}^2}{bk = rv}, \tag{XIII-4}$$

$$B_1 = ss \text{ among blocks} - \left(\sum_{j=1}^{v}(SB)_j^2 - \frac{k^2X_{..}^2}{v}\right)/k(r-\lambda)$$

$$+ \sum_{j=1}^{v} W_j^2/rv(v-k)(k-1), \tag{XIII-5}$$

$$\text{total } ss = \sum_{i=1}^{r}\sum_{j=1}^{v} X_{ij}^2 - \frac{X_{..}^2}{rv}, \tag{XIII-6}$$

and E is obtained by subtracting the sums of squares in formulae (XIII-4) and (XIII-5) from the sum of squares obtained in equation (XIII-6); $X_{.j}$ = treatment total; $X_{..}$ = total of all $rv = bk$ observations; $(SB)_j$ = total of all blocks in which treatment j appears (see example XI-4); and

$$W_j = (v-k)X_{.j} - (v-1)(SB)_j + (k-1)X_{..}. \tag{XIII-7}$$

Also,

$$T_2 = \sum_{j=1}^{v} Q_j^2/k^2r(\text{Eff}) = \sum Q_j^2/kv\lambda, \tag{XIII-8}$$

and

$$B_2 = ss \text{ among } b \text{ block totals}$$

$$= \sum_{u=1}^{b} \frac{B_{.u}^2}{k} - X_{..}^2/rv \tag{XIII-9}$$

where

$$Q_j = kX_{.j} - (SB)_j \tag{XIII-10}$$

and

$$B_{.u} = \text{total of } u\text{th incomplete block.} \tag{XIII-11}$$

In the event that $E_b \leq E_e$ the analysis for the completely randomized design is used, and the treatment means, $\bar{x}_j = X_{.j}/r$, are not adjusted. If the number of degrees of freedom associated with E_b is less than 12 and $E_b > E_e$, it is recommended that interblock information be ignored and the treatment means be adjusted only for intrablock information. If, on the other hand, $E_b > E_e$ and the number of degrees of freedom associated with E_b is greater than 12 to 14, it is recommended that both interblock and intrablock information be utilized.

The adjusted treatment mean utilizing only intrablock information is equal to the experimental mean, $\bar{x}$, plus a correction; thus:

$$\bar{x}_j'' = \bar{x} + Q_j/rk(\text{Eff}) = \bar{x} + (v-1)Q_j/rv(k-1) \tag{XIII-12}$$

with an average variance of a difference between two means of

$$\frac{2E_e}{r(\text{Eff})} = 2E_e\left(\frac{k(v-1)}{rv(k-1)}\right) = \frac{2E_e}{r}\left(1 + \frac{v-k}{v(k-1)}\right). \tag{XIII-13}$$

The variance of a difference between two adjusted means is obtained by multiplying by two. If interblock information is recovered, the adjusted treatment mean is equal to the unadjusted treatment mean plus a correction; thus:

$$\bar{x}_j' = \bar{x}_j + \mu W_j/r \tag{XIII-14}$$

with an average variance of a difference between two means of

$$\frac{2k(v-1)}{rwv(k-1) + rw'(v-k)} = \frac{2k(v-1)\mu}{r(w-w')}, \tag{XIII-15}$$

where

$$w = 1/E_e, \tag{XIII-16}$$

$$w' = \frac{v(r-1)}{k(b-1)E_b - (v-k)E_e}, \tag{XIII-17}$$

and

$$\begin{aligned} \mu &= \frac{w - w'}{wv(k-1) + w'(v-k)} \\ &= \frac{(b-1)(E_b - E_e)}{v(k-1)(b-1)E_b + (v-k)(b-v)E_e}. \end{aligned} \tag{XIII-18}$$

The average effective error variance is $r/2$ times the variance of a difference for mean (see formulae (XIII-13) and (XIII-15)). The efficiency of the incomplete block design relative to the completely randomized design is the ratio of the two average effective error variances for the two designs. The coefficient of variation is the square root of the average effective variance divided by the mean of the experiment, $\bar{x}$.

In the special case where $v = b$ and $r = k$, the above formulae may be simplified [60]. Some modifications of the formulae are necessary if the treatments are arranged in complete replicates or in groups of replicates. Yates [327] and others [30; 60, Ch. 11; 175, Ch. 26; 255] present formulae and discuss the calculational procedure for these cases.

It should be noted that the sum of squares for block (eliminating treatment effects), equation (XIII-5), may be obtained by another procedure; thus:

block (ignoring treatment) *ss*
− treatment (ignoring block) *ss*
+ treatment (eliminating block) *ss*
= block (eliminating treatment) *ss*. (XIII-19)

Also, it should be noted that when $v = b$, formula (XIII-5) reduces to

$$\sum_{j=1}^{v} W_j^2/rv(v-k)(k-1), \tag{XIII-20}$$

since the sum of squares among the block totals, $B_{\cdot u}$, is equal to the sum of squares among the totals of blocks, $(SB)_j$.

As with other lattice designs, tests of significance have not been fully

developed to date. The same approximate tests described previously are applicable here. When interblock information is not recovered, the F test,

$$F(v - 1 \text{ and } bk - v - b + 1df) = \frac{\text{treatment (elim. block) } ms}{E_e}, \quad \text{(XIII-21)}$$

follows the F distribution if the error deviations are independently and normally distributed with mean zero and variance σ_e^2. When interblock information is recovered, an *approximate* F test is

$$F(v - 1 \text{ and } bk - v - b + 1df) = \frac{ms \text{ among adjusted treatments}}{\text{average effective error variance}}. \quad \text{(XIII-22)}$$

Individual comparisons among means or groups of means are easily obtained by the above formula.

Cornish [64] presents a formula for obtaining the intrablock estimate for a missing value in a balanced incomplete block design. The formula is

$$X_{ju}' = \frac{vr(k-1)B_{.u} + k(v-1)Q_j - (v-1)Q_j'}{k(k-1)(bk - b - v + 1)}, \quad \text{(XIII-23)}$$

where the jth treatment in the uth block is missing; the uth block total is $B_{.u}$; Q_j is the value of $kX_{.j} - (SB)_j$ for the jth treatment; and Q_j' is the sum of the Q values for all other treatments in the uth block. If more than one value is missing, a guess-estimate is inserted for all missing values but one; formula (XIII-23) is used to compute the value for the remaining missing plot. The computed value is inserted for the missing experimental unit, formula (XIII-23) is used to compute the value for a second missing value, and the process is continued until the computed values stabilize. As usual, one degree of freedom is deducted from the intrablock error for each missing value computed. If several values are missing, it may be advisable to analyze the experiment as a completely randomized design. Also, one may introduce pseudo-variates, composed of zeros and ones, for the missing values and perform an analysis of covariance [255].

The methods for constructing b.i.b. designs have been described by Yates [322] and Kempthorne [175, Ch. 26]. A number of plans for various values of v, b, k, and r have been presented [26, 60; 175, Ch. 12; 322].

Example XIII-1. Hanson *et al.* [141] compared ten methods of preparing dried egg samples. A panel of seven judges tasted each sample. The average scores for each sample, the panel means, are given in table XIII-1. The $10 = v$ treatments were set up in $b = 15$ incomplete blocks of $k = 4$ treatments each. Every pair of samples occurred together $\lambda = 2$ times in the incomplete blocks. The number of replicates on any particular treatment was $r = 6$. The ten treatments consisted of two controls and a 2^3 factorial. Treatment 1 was the control for treatments 7 to 10, and treatment 2 was the control for treatments 3 to 6. Treatments 3 to 10 formed a $2 \times 2 \times 2$ factorial combination of powders, moisture contents, and methods of packing. The present analysis considers the ten treatments as a single set of treatments. The partition of

treatment degrees of freedom into single degree of freedom comparisons is left as an exercise for the reader. Precaution was taken to insure that color differences did not affect the judges' scores. Sodium vapor lights were used to mask any color differences; the dried egg samples were reconstituted and scrambled before presenting the samples to the judges [141].

TABLE XIII-1. Panel means of scores of 10 dried egg samples (score of 10 = no off-flavor and zero = extreme off-flavor)

Incomplete block (period)	Treatment number 1	2	3	4	5	6	7	8	9	10	Total = $B_{.u}$
1	9.7	8.7	-	5.4	5.0	-	-	-	-	-	28.8
2	-	9.6	8.8	-	-	5.6	-	-	-	3.6	27.6
3	-	9.0	-	7.3	-	3.8	4.3	-	-	-	24.4
4	9.3	-	8.7	-	6.8	-	3.8	-	-	-	28.6
5	10.0	-	-	7.5	-	-	-	4.2	-	2.8	24.5
6	-	9.6	-	-	-	-	5.1	4.6	3.6	-	22.9
7	-	9.8	-	-	7.4	-	-	4.4	-	3.8	25.4
8	-	-	-	-	9.4	-	6.3	-	5.1	2.0	22.8
9	9.3	9.3	8.2	-	-	-	-	-	3.3	-	30.1
10	-	-	-	8.7	9.0	6.0	-	-	3.3	-	27.0
11	9.7	-	-	-	-	6.7	6.6	-	-	2.8	25.8
12	-	-	9.3	8.1	-	-	-	-	3.7	2.6	23.7
13	9.8	-	-	-	-	7.3	-	5.4	4.0	-	26.5
14	-	-	9.0	8.3	-	-	4.8	3.8	-	-	25.9
15	-	-	9.3	-	8.3	6.3	-	3.8	-	-	27.7
Total = $X_{.j}$	57.8	56.0	53.3	45.3	45.9	35.7	30.9	26.2	23.0	17.6	391.7
Mean = x_j	9.6	9.3	8.9	7.6	7.6	6.0	5.2	4.4	3.8	2.9	6.53
Range	0.7	1.1	1.1	3.3	4.4	3.5	2.8	1.6	1.8	1.8	-

TABLE XIII-2. Calculation of adjusted treatment means

Treatment number	Treatment total = $X_{.j}$	Sum of blocks containing treatment j = $(SB)_j$	$Q_j = kX_{.j} - (SB)_j$	$W_j = (v - k)X_{.j} - (v - 1)(SB)_j + (k - 1)X_{..}$	Adjusted total = $X_{.j} + \mu W_j$	Adjusted mean
1	57.8	164.3	66.9	43.2	58.80	9.80
2	56.0	159.2	64.8	78.3	57.81	9.64
3	53.3	163.6	49.6	22.5	53.82	8.97
4	45.3	154.3	26.9	58.2	46.65	7.78
5	45.9	160.3	23.3	7.8	46.08	7.68
6	35.7	159.0	-16.2	-41.7	34.73	5.79
7	30.9	150.4	-26.8	6.9	31.06	5.18
8	26.2	152.9	-48.1	-43.8	25.19	4.20
9	23.0	153.0	-61.0	-63.9	21.52	3.59
10	17.6	149.8	-79.4	-67.5	16.04	2.67
Total	391.7 = $X_{..}$	1566.8 = $kX_{..}$	0	0	391.70 = $X_{..}$	65.30 = $X_{..}/r$

Before computing the analysis of variance, it should be noted that treatments with intermediate scores tend to have a larger range than do the remaining treatments (table XIII-1). However, the ranges of the deviations for a treatment rather than the ranges of the actual scores for the treatments should be considered. Since the ranges are small compared to the differences among the treatment means and since they are not exceedingly discrepant, separate analyses for the more variable treatments are

not computed. Also, no transformation of the data is made prior to analysis, although an arcsine transformation may be appropriate.

The various sums of squares are (see tables XIII-1 and XIII-2 for totals)

Treatment (ignoring block) with 9 df *(formula (XIII-4))*:

$$\frac{57.8^2 + \cdots + 17.6^2}{6} - \frac{391.7^2}{60} = 2871.86 - 2557.15 = 314.71.$$

Block (ignoring treatment) with 14 df *(formula (XIII-9))*:

$$\frac{28.8^2 + \cdots + 27.7^2}{4} - \frac{391.7^2}{60} = 2574.22 - 2557.15 = 17.07.$$

Treatment (eliminating block) with 9 df *(formula (XIII-8))*:

$$\frac{66.9^2 + \cdots + (-61.0)^2 + (-79.4)^2}{kv\lambda = 4(10)(2) = 80} = 321.51.$$

Block (eliminating treatment) with 14 df *(formulae (XIII-5) or (XIII-19))*:

$$17.07 - \left\{\frac{164.3^2 + \cdots + 149.8^2 - 1566.8^2/10}{k(r-\lambda) = 16}\right\} + \left\{\frac{43.2^2 + \cdots + (-67.5)^2}{6(10)(10-4)(4-1) = 1080}\right\}$$

$$= 17.07 - 15.69 + 22.50 = 23.88,$$

or

$$17.07 - (314.71 - 321.51) = 23.87.$$

Intrablock error with 36 df *(by subtraction)*:

$$356.46 - 314.71 - 23.88 = 17.87.$$

The weights are estimated to be

$$w = 1/.496 = 2.016;$$

$$w' = \frac{10(6-1)}{4(15-1)(1.706) - (10-4)(.496)} = 0.5402.$$

The weighting factor is equal to

$$\mu = \frac{2.016 - 0.5402}{2.016(10)(4-1) + 0.5402(10-4)} = \frac{1.4758}{63.7212} = 0.02316.$$

The adjusted total, recovering interblock information, is obtained by adding μW_j to the unadjusted treatment total, $X_{\cdot j}$. For example, the adjusted total for treatment 2 is $56.0 + (.02316)(78.3) = 57.81$. The other adjusted totals are obtained in a similar manner.

TABLE XIII-3. Analysis of variance

Source of variation	df	ss	ms
Total uncorrected	60	2913.61	-
Correction for mean	1	2557.15	-
Total	59	356.46	-
Treatment (ignoring block)	9	314.71	-
Within treatments	50	41.75	$0.835 = E_e'$
Block (eliminating treatment)	14	23.88	$1.706 = E_b$
Intrablock	36	17.87	$0.496 = E_e$
Treatment (eliminating block)	9	321.51	35.72
Block (ignoring treatment)	14	17.07	-
Intrablock	36	17.88	-

The efficiency of this design relative to the corresponding completely randomized design is

$$\frac{0.835(100)}{4(9)(.02316)/(1.4758)} = \frac{83.5}{.5650} = 148 \text{ per cent.}$$

The coefficient of variation is $(100)\sqrt{.5650}/6.53 = 11.5$ per cent. The standard error of a difference between two adjusted means is equal to $\sqrt{2(.5650)/6} = 0.434$ scoring units.

XIII-2.1.2 More than one basic set. It may happen that more than one set of the balanced lattice design is required to obtain sufficient replication on the treatments. For example, suppose that it is desired to compare $v = 13$ sweet potato varieties for quality, that treatment differences are small compared to the error variance, and that each taster or judge compares only $k = 4$ varieties at one time. For a b.i.b. design with $b = 13$ incomplete blocks, four replicates are required. It may be necessary to use sixteen replicates in order to detect differences of the order of magnitude likely to be encountered.[1] If each set of four replicates is completed prior to beginning the next set, the appropriate analysis of variance is given by Yates [327] and others [60, Ch. 11; 175, Ch. 26]. If, on the other hand, the fifty-two tasters are randomly allotted to the fifty-two incomplete blocks and if there is no valid reason for grouping by replicates, the analysis of variance for q sets of the basic design is of the form:

Source of variation	df	Mean square	
		Observed	Expected value
Treatment (ignoring block)	$v - 1$	—	—
Block (eliminating treatment)	$qb - 1$	E_b	$\sigma_e^2 + (qbk - v)\sigma_\beta^2/(qb - 1)$
Component (b)	$b - 1$		$\sigma_e^2 + (bk - v)\sigma_\beta^2/(b - 1)$
Component (a)	$b(q - 1)$		$\sigma_e^2 + k\sigma_\beta^2$
Intrablock	$qbk - qb - v + 1$	E_e	σ_e^2
Total	$qbk - 1$	—	—

The standard error of a difference between any two adjusted means is

$$\sqrt{\frac{2k(v-1)}{rq[vw(k-1) + w'(v-k)]}} \tag{XIII-24}$$

where

$$w = 1/E_e \tag{XIII-25}$$

and

$$w' = \frac{kbq - v}{k(qb-1)E_b - (v-k)E_e}. \tag{XIII-26}$$

[1] Drs. G. A. Johannessen and J. D. Hartman, Cornell University, conducted an experiment of this type.

The weighting factor is equal to

$$\mu = \frac{w - w'}{wv(k - 1) + w'(v - k)} \tag{XIII-27}$$

The adjusted treatment means are computed from formula (XIII-14), with r replaced by rq in the denominator of the correction factor.

XIII-2.2 PARTIALLY BALANCED INCOMPLETE BLOCK DESIGNS

As is obvious from formulae (XIII-1) and (XIII-2), large values of r are obtained for certain values of b, k, and v. In order to obtain incomplete block designs with smaller numbers of replicates, it is necessary to use incomplete block designs which are partially balanced [27, 30, 32, 175, 226, 320, 322, 324, 326]. Examples of resolvable p.b.i.b. designs (e.g., double, triple, cubic, and simple rectangular lattices) have already been discussed. P.b.i.b. designs with the treatments not in complete replicates are presented below.

Bose and Nair [30] give the following specifications for a p.b.i.b. design [32; 175, Ch. 27; 255]:

(i) The v treatments with r replicates per treatment are arranged in b incomplete blocks of k treatments each.

(ii) With regard to a particular treatment j, the remaining $(v - 1)$ treatments fall into m sets, the gth set of which occurs in λ_g blocks with treatment j and contains n_g treatments; the numbers n_g are the same regardless of the treatment specified.

(iii) $$\sum_{g=1}^{m} n_g = v - 1. \tag{XIII-28}$$

$$\sum_{g=1}^{m} \lambda_g n_g = r(k - 1). \tag{XIII-29}$$

(iv) If we denote the treatments occurring in an incomplete block λ_g times with a specified treatment j as the gth associates of j and if we denote the treatments occurring λ_g' times with a treatment $j'(\neq j)$as the fth associates of j', where j and j' are hth associates, then the number of treatments in common between the gth associates of j and the fth associates of j' is $p_{gf}{}^h$; this number is the same for any pair of hth associates.

(v) $$p_{gf}{}^h = p_{fg}{}^h. \tag{XIII-30}$$

$$n_h p_{gf}{}^h = n_g p_{fh}{}^g = n_f p_{gh}{}^f. \tag{XIII-31}$$

$$\sum_f p_{gf}{}^h = \begin{cases} n_g - 1, \text{ for } k = r \\ n_g \quad\ \ , \text{ for } k \neq r \end{cases}. \tag{XIII-32}$$

Bose and Nair denote r, v, b, k, λ_1, λ_2, $\cdots$, λ_m, n_1, n_2, $\cdots$, n_m as parameters of the first kind and the $p_{gf}{}^h$ as parameters of the second kind.

The methods of constructing p.b.i.b. designs are not discussed herein.[1] The analysis for $m = 2$ is illustrated with an example. For the analysis of a p.b.i.b. design with $m = 3$, the reader is referred to Chapter XI and to the paper by Nair [221]. Nair also discusses the analysis for the simple rectangular lattice which is an $m = 4$ associate class design.

If $E_b \leq E_e$, Rao [255] recommends that the experiment be analyzed as a completely randomized design. When less than 12 degrees of freedom are associated with E_b and when $E_b > E_e$, it is suggested that recovery of interblock information be dispensed with and only intrablock adjustments be made.

Rather than repeat the basic design, it is recommended that attempts be made to balance the design or to approach a balanced design in order to approach equal precision on treatment comparisons. If this is undesirable, then the basic plan of the p.b.i.b. design with a different randomization could be repeated. The form of the analysis follows that described in the previous section. For a more complete discussion of these designs the reader is referred to Bose and Nair [30], Kempthorne [175, Ch. 27], Nair [221], and Rao [255]. The analysis for the p.b.i.b. design given in example XIII-2 closely parallels the discussions and examples given by these authors.

Example XIII-2. An example of a partially balanced incomplete block design was constructed from the data presented in table XIII-1. All incomplete blocks containing treatment 1 were deleted. This procedure of constructing a partially balanced incomplete block design from a balanced incomplete block design is known as "variety cutting" [255; see 113]. The resulting design may not always be the simplest one available for the particular values of b, v, and k. In order to obtain a relatively simple design, the experimental results in incomplete blocks 10, 12, and 14, which contained the following sets of treatments: (4, 5, 6, 9), (3, 4, 9, 10), and (3, 4, 7, 8), were arranged into incomplete blocks containing the following sets of treatments: (4, 6, 8, 9), (3, 4, 5, 9), and (3, 4, 7, 10) (table XIII-4). These shifts resulted in a p.b.i.b. design with $m = 2$ associate classes, which is similar to the simple lattice analysis (see example XI-1). The notation used herein is somewhat different from that used in example XI-1 and by Rao [255]; it is more in agreement with that used by Bose and Nair [30], Kempthorne [175, Ch. 27], and Nair [221].

The parameters of the first kind for this particular p.b.i.b. design are

$$\left.\begin{array}{c} v = 9,\ \ b = 9,\ \ r = 4,\ \ k = 4 \\ \\ \lambda_1 = 1,\ n_1 = 4,\ \lambda_2 = 2,\ n_2 = 4 \end{array}\right\}. \tag{XIII-33}$$

The treatments which appear together once in the incomplete blocks are denoted as first associates, and those which appear together twice in the incomplete blocks as second associates. The treatments occurring together once in one of the incomplete blocks have the same type of association with other treatments. Hence, this design is a two associate class design.

[1] All known p.b.i.b. designs for $m = 2$, $r \leq 10$, and $3 \leq k \leq 10$ have been tabulated by Bose, Clatworthy, and Shrikhande [26].

TABLE XIII-4. Partially balanced incomplete block design constructed from the balanced incomplete block design in table XIII-1

Incomplete block (period)	Treatment number									Incomplete block total $= B_{.u}$
	2	3	4	5	6	7	8	9	10	
2	9.6	8.8	-	-	5.6	-	-	-	3.6	27.6
3	9.0	-	7.3	-	3.8	4.3	-	-	-	24.4
6	9.6	-	-	-	-	5.1	4.6	3.6	-	22.9
7	9.8	-	-	7.4	-	-	4.4	-	3.8	25.4
8	-	-	-	9.4	-	6.3	-	5.1	2.0	22.8
10	-	-	8.7	-	6.0	-	3.8	3.3	-	21.8
12	-	9.3	8.1	9.0	-	-	-	3.7	-	30.1
14	-	9.0	8.3	-	-	4.8	-	-	2.6	24.7
15	-	9.3	-	8.3	6.3	-	3.8	-	-	27.7
Total $= X_{.j}$	38.0	36.4	32.4	34.1	21.7	20.5	16.6	15.7	12.0	227.4
Mean $= \bar{x}_j$	9.5	9.1	8.1	8.5	5.4	5.1	4.2	3.9	3.0	6.317
Range	0.8	0.5	1.4	2.0	2.5	2.0	0.8	1.8	1.8	-

TABLE XIII-5. Data for computing the analysis of variance and the adjusted means

Treatment number	$X_{.j}$	$(SB)_j$	$Q_j = kX_{.j} - (SB)_j$	First associates	ΣQ_{jl}	Adj. mean effect $= \hat{\tau}_j$	P_j	ΣP_{jl}	Adj. mean $= \bar{x}_j'$
2	38.0	100.3	51.7	3, 4, 5, 9	59.7	3.402	120.184	152.035	9.60
3	36.4	110.1	35.5	2, 7, 8, 9	-27.3	2.716	93.719	- 80.057	9.07
4	32.4	101.0	28.6	2, 5, 8, 10	- 1.8	2.076	66.903	- 3.825	8.23
5	34.1	106.0	30.4	2, 4, 6, 7	52.8	1.902	76.974	115.851	8.40
6	21.7	101.5	-14.7	5, 7, 9, 10	-69.7	-0.674	- 33.920	-169.522	5.52
7	20.5	94.8	-12.8	3, 5, 6, 8	19.8	-1.034	- 37.316	59.409	5.19
8	16.6	97.8	-31.4	3, 4, 7, 10	- 1.2	-2.261	- 77.364	- 0.313	4.11
9	15.7	97.6	-34.8	2, 3, 6, 10	20.0	-2.624	- 85.561	56.364	3.82
10	12.0	100.5	-52.5	4, 6, 8, 9	-52.3	-3.501	-123.619	-129.942	2.92
Total	227.4 $= X_{..}$	909.6 $= rX_{..}$	0		0	0.002	0.000	0.000	56.86 $= v\bar{x}$

The parameters of the second kind are

$$p_{ef}^1 = \begin{pmatrix} 1 & 2 \\ 2 & 2 \end{pmatrix} \quad \text{and} \quad p_{ef}^2 = \begin{pmatrix} 2 & 2 \\ 2 & 1 \end{pmatrix}. \tag{XIII-34}$$

The p_{ef}^1 matrix refers to the number of associates in common for pairs of treatments which are first associates; e.g., treatments 2 and 3 are first associates; they have one first associate in common, two second associates in common, and two of the treatments in common which are first associates of one and second associates of the other. The p_{ef}^2 matrix refers to the number of various types of associates in common for a pair of treatments which are second associates. The two kinds of parameters for a p.b.i.b. design fully determine the incomplete block design, although there is some leeway in the selection of treatments appearing together in an incomplete block.

The analysis of the reduced design is described below. The incomplete block, the

treatment, and the grand totals are presented in table XIII-4. It should be noted that the ranges of panel scores by treatments are more alike than for the complete data in table XIII-1. The inclusion of the periods containing treatment 1 increased the range of panel scores.

The next step is to construct table XIII-5 in order to complete the analysis of variance table (table XIII-6). The first column of table XIII-5 contains the treatment numbers; the second column contains the treatment totals; the third column contains the sum of the incomplete block totals, $B_{.u}$, which contain a given treatment; thus, treatment 2 appears in incomplete blocks 2, 3, 6, and 7, and $(SB)_2$ is equal to $B_{.2} + B_{.3} + B_{.6} + B_{.7} = 27.6 + 24.4 + 22.9 + 25.4 = 100.3$. The values in the fourth column are obtained from the formula,

$$Q_j = kX_{.j} - (SB)_j, \tag{XIII-35}$$

which for $j = 2$ is equal to $4(38.0) - 100.3 = 51.7 = Q_2$. The first associates of the various treatments are listed in column 5. The sixth column contains the sum of the Q values for treatments which are first associates of treatment j; to illustrate, treatments 3, 4, 5, and 9 are first associates of treatment 2, and the sum of the Q values for these $n_1 = 4$ treatments is equal to $35.5 + 28.6 + 30.4 + (-34.8) = 59.7 = \sum Q_{21} = Q_3 + Q_4 + Q_5 + Q_9$.

Before proceeding further with table XIII-5, it is necessary to calculate a number of constants associated with the design; thus:

$$A_{12} = r(k-1) + \lambda_2 = 4(4-1) + 2 = 14, \tag{XIII-36}$$

$$A_{22} = (\lambda_2 - \lambda_1)p_{12}^2 = (2-1)(2) = 2, \tag{XIII-37}$$

$$B_{12} = \lambda_2 - \lambda_1 = 1, \tag{XIII-38}$$

$$B_{22} = A_{12} + (\lambda_2 - \lambda_1)(p_{11}^1 - p_{11}^2) = 14 + (1-2) = 13, \tag{XIII-39}$$

$$A_{11} = r(k-1) + \lambda_1 = 4(4-1) + 1 = 13, \tag{XIII-40}$$

$$A_{21} = (\lambda_1 - \lambda_2)p_{12}^1 = (1-2)(2) = -2, \tag{XIII-41}$$

$$B_{11} = \lambda_1 - \lambda_2 = -1, \tag{XIII-42}$$

$$B_{21} = A_{11} + B_{11}(p_{22}^2 - p_{22}^1) = 13 + (-1)(1-2) = 14, \tag{XIII-43}$$

and

$$\Delta = A_{12}B_{22} - A_{22}B_{12} = A_{11}B_{21} - A_{21}B_{11} = 180. \tag{XIII-44}$$

The seventh column contains the adjusted treatment effects without recovery of interblock information. These values, $\hat{t}_j$, plus the over-all mean, $\bar{x} = 6.317$, result in the adjusted treatment means. The adjusted treatment effects without recovery of interblock information are obtained from the formula,

$$\hat{t}_j = \frac{\begin{vmatrix} Q_j & B_{12} \\ \sum Q_{j1} & B_{22} \end{vmatrix}}{\Delta} = \frac{B_{22}Q_j - B_{12}\sum Q_{j1}}{\Delta} = \frac{(B_{22} + B_{12})Q_j - B_{12}(\sum Q_{j1} + Q_j)}{\Delta} \tag{XIII-45}$$

For example, $\hat{t}_2 = [13(51.7) - (1)(59.7)]/180 = 3.402$.

After completing the computations in the first seven columns of table XIII-5, table XIII-6 is constructed. The various sums of squares are obtained below.

Total (*35* df):

$$9.6^2 + \cdots + 3.8^2 - 227.4^2/36 = 1649.10 - 1436.41 = 212.69.$$

TABLE XIII-6. Analysis of variance for the experiment in table XIII-4

Source of variation	df	ss	ms
Total uncorrected	36	1649.10	-
Correction for mean	1	1436.41	-
Total	35	212.69	-
Treatment (ignoring block)	8	198.27	-
Within treatments	27	14.42	0.534
Block (eliminating treatment)	8	6.298	0.787 = E_b
Intrablock	19	8.122	0.427 = E_e
Treatment (eliminating block)	8	189.688	23.711
Block (ignoring treatment)	8	14.88	-
Intrablock	19	8.122	0.427

Treatment (*ignoring block*) (*8* df):

$$\frac{38.0^2 + \cdots + 12.0^2}{4} - \frac{227.4^2}{36} = 1634.68 - 1436.41 = 198.27.$$

Block (*ignoring treatment*) (*8* df):

$$\frac{\sum B._u{}^2}{k} - \frac{X..^2}{bk} = \frac{27.6^2 + \cdots + 27.7^2}{4} - \frac{227.4^2}{36} = 1451.29 - 1436.41 = 14.88. \tag{XIII-46}$$

Treatment (*eliminating block*) (*8* df):

$$\sum_{j=2}^{10} \hat{\tau}_j Q_j/k = 189.688 = \frac{B_{22}}{k\Delta}\sum_{j=2}^{10} Q_j{}^2 - \frac{B_{12}}{k\Delta}\sum Q_j Q_{1j}. \tag{XIII-47}$$

Block (*eliminating treatment*) (*8* df):

$$\frac{\sum B._u{}^2}{k} - \frac{X..^2}{kb} - \left\{\frac{\sum X._j{}^2}{r} - \frac{X..^2}{kb} - \sum \hat{\tau}_j Q_j/k\right\} = \sum B._u{}^2/k - \sum X._j{}^2/r + \sum \hat{\tau}_j Q_j/k$$
$$= 1451.29 - 1634.68 + 189.688 = 6.298. \tag{XIII-48}$$

Intrablock (bk − v − b + 1 = 19 df):

$$212.69 - 198.27 - 6.298 = 212.69 - 189.688 - 14.88 = 8.122.$$

The estimated amount of intrablock information is

$$w = 1/E_e = 1/0.427 = 2.342, \tag{XIII-49}$$

and the estimated amount of interblock information is

$$w' = \frac{bk - v}{k(b-1)E_b - (v-k)E_e} = \frac{27}{32(.787) - 5(.427)} = 1.171. \tag{XIII-50}$$

In order to complete the calculations in table XIII-5 and to obtain the adjusted treatment means with recovery of interblock information, the following quantities are required:

$$R = r[w + w'/(k-1)] = 4(2.342 + 1.171/3) = 10.929. \qquad \text{(XIII-51)}$$

$$\Lambda_1 = \lambda_1(w - w') = 1.171. \qquad \text{(XIII-52)}$$

$$\Lambda_2 = \lambda_2(w - w') = 2(1.171) = 2.342. \qquad \text{(XIII-53)}$$

$$A_{12}' = R(k-1) + \Lambda_2 = 3(10.929) + 2.342 = 35.129. \qquad \text{(XIII-54)}$$

$$A_{22}' = (\Lambda_2 - \Lambda_1)p_{12}^2 = (1.171)(2) = 2.342. \qquad \text{(XIII-55)}$$

$$B_{12}' = \Lambda_2 - \Lambda_1 = 1.171. \qquad \text{(XIII-56)}$$

$$B_{22}' = A_{12}' + B_{12}'(p_{11}^1 - p_{11}^2) = 33.958. \qquad \text{(XIII-57)}$$

$$A_{11}' = R(k-1) + \Lambda_1 = 33.958. \qquad \text{(XIII-58)}$$

$$A_{21}' = (\Lambda_1 - \Lambda_2)p_{12}^1 = -2.342. \qquad \text{(XIII-59)}$$

$$B_{11}' = \Lambda_1 - \Lambda_2 = -1.171. \qquad \text{(XIII-60)}$$

$$B_{21}' = A_{11}' + B_{11}'(p_{22}^2 - p_{22}^1) = 35.129. \qquad \text{(XIII-61)}$$

$$\begin{aligned} \Delta' &= A_{12}'B_{22}' - A_{22}'B_{12}' = A_{11}'B_{21}' - A_{21}'B_{11}' \qquad \text{(XIII-62)} \\ &= 35.129(33.958) - 2.342(1.171) \\ &= 33.958(35.129) - (-2.342)(-1.171) = 1190.168. \end{aligned}$$

The eighth column of table XIII-5 is computed using the formula,

$$P_j = wQ_j + w'(SB)_j - rkw'\bar{x}, \qquad \text{(XIII-63)}$$

which for $j = 2$ is equal to $2.342(51.7) + 1.171(100.3) - 4(4)(1.171)(227.4/36) = 121.0814 + 117.4513 - 118.3491 = 120.184$. The ninth column contains the sum of the P_j values for the treatments which are first associates of treatment j; thus, for $j = 2$ the $\sum P_{21} = 93.719 + 66.903 + 76.974 - 85.561 = 152.035$. In the tenth and last column the adjusted treatment means with recovery of interblock information are presented. The formula for obtaining the adjusted treatment means is

$$\bar{x}_j' = \bar{x} + \frac{1}{\Delta'}\begin{vmatrix} P_j & B_{12}' \\ \sum P_{j1} & B_{22}' \end{vmatrix} = \bar{x} + \frac{1}{\Delta'}(B_{22}'P_j - B_{12}'\textstyle\sum P_{j1}), \qquad \text{(XIII-64)}$$

which is similar to formula (XIII-45). The adjusted mean for treatment 2 is equal to $6.3167 + \frac{1}{1190.168}[33.958(120.184) - 1.171(152.035)] = 6.3167 + 3.2795 = 9.60$. The remaining adjusted means are obtained in a similar manner.

The variances for differences of treatment effects derived from intrablock comparisons are

$$V_1 = 2kB_{21}E_e/\Delta = \frac{2(4)(14)(0.427)}{180} = .2657 \qquad \text{(XIII-65)}$$

and

$$V_2 = 2kB_{22}E_e/\Delta = \frac{2(4)(13)(0.427)}{180} = .2467 \qquad \text{(XIII-66)}$$

for pairs of treatments which are first and second associates, respectively. The average variance of a mean difference is equal to

$$V = \frac{vn_1V_1 + vn_2V_2}{2v(n_1 + n_2)/2} = \frac{n_1V_1 + n_2V_2}{v - 1} = \frac{4(.2657) + 4(.2467)}{9 - 1} = .2562. \qquad \text{(XIII-67)}$$

The same form of the above formulae holds for the differences between adjusted treatment means when interblock information is recovered; thus:

$$V_1' = \frac{2kB_{21}'}{\Delta'} = \frac{2(4)(35.129)}{1190.168} = .2361 \qquad \text{(XIII-68)}$$

and

$$V_2' = \frac{2kB_{22}'}{\Delta'} = \frac{2(4)(33.958)}{1190.168} = .2283 \qquad \text{(XIII-69)}$$

for pairs of treatments which are first and second associates, respectively. The average variance of a difference is equal to

$$V' = \frac{1}{v - 1}(n_1V_1' + n_2V_2') = .2322. \qquad \text{(XIII-70)}$$

The recovery of interblock information did not appreciably alter the variance of a difference between two treatments. Since E_b is not too much larger than E_e, this is what would be expected.

The efficiency factor for this design is equal to

$$\text{Eff} = \frac{(v-1)}{rk}\frac{\begin{vmatrix} A_{12} & B_{12} \\ A_{22} & B_{22} \end{vmatrix}}{\begin{vmatrix} v-1 & B_{12} \\ -n_1 & B_{22} \end{vmatrix}} = \frac{(v-1)\Delta}{rk[(v-1)B_{22} + n_1B_{12}]} \qquad \text{(XIII-71)}$$

$$= \frac{8(180)}{4(4)[8(13) + 4(1)]} = \frac{5}{6}.$$

XIII-3 Incomplete Block Designs for the Two-Way Elimination of Heterogeneity

Another incomplete block design is the incomplete latin square. For example, one or more rows or columns, or one or more rows and columns, of a $k \times k$ latin square may be omitted [321, 333]. The Youden square [28, 264, 266, 333, 336, 337] and the semi-latin square [201, 284, 319], in some instances, represent special cases of a latin square design with one or more rows omitted. Also, rows or columns may be added to a $k \times k$ latin square [60, 264], and other situations are possible [28, 241, 242, 264, 269]. The examples discussed below illustrate some of the incomplete block designs with a two-way elimination of heterogeneity.

The designs discussed in this section are of the single latin square type. The source of variability controlled is similar to that controlled by rows and columns in the latin square. Designs such as the one-restrictional lattice in complete replicates are not considered as belonging to this group. The lattice

square design, a two-restrictional lattice, is of the latin square type within each replicate but not over the entire experimental area.

XIII-3.1 ONE OR MORE MISSING ROWS OR COLUMNS IN A LATIN SQUARE

In latin squares the yields for one or more rows, columns, or treatments may be omitted from the analysis. Also, the error variances of some treatments may be different from other treatments, and it may be desired to run the analysis on only a portion of the treatments in a $k \times k$ latin square. The omission of a treatment in a $k \times k$ latin square results in an incomplete latin square. The omission of one or more rows or columns in a $k \times k$ latin square results in an incomplete block design, since all treatments do not appear in the same row or column. Any omission results in some nonorthogonality; despite this, some of the resulting incomplete latin squares are relatively simple computationally. For a complete discussion of these designs the reader is referred to the papers by Yates [321] and Yates and Hale [333]. Dr. G. S. Beavans' "chessboard" system of square-yard plots [284; Chapter I] with proper randomization fits into the scheme of analysis proposed by Yates and Hale.

XIII-3.2 THE SEMI-LATIN SQUARE AND SIMILAR DESIGNS

The semi-latin square design (also called a balanced or equalized block design) consists of a $k \times k$ latin square design with p split plots per whole plot. There are $v = pk$ treatments compared in the $k \times k$ latin square. For example, with ten treatments arranged in a 5×5 latin square, Student [284] suggests the following arrangement for treatments $A, B, C, D, E, F, G, H, I,$ and J:

	Col. 1	Col. 2	Col. 3	Col. 4	Col. 5
Row 1	G F	H D	E J	C B	A I
Row 2	H B	J G	D I	F A	E C
Row 3	E J	I B	A C	G H	D F
Row 4	C A	F E	B G	I D	J H
Row 5	D I	A C	F H	J E	B G

The order within a square is important; each treatment appears in the top and bottom rows an equal or nearly equal number of times. As Fisher states [319, p. 231], the correct error for any particular contrast "would be exceedingly laborious to estimate." Yates [319] points out that the "usual method" of analysis for the semi-latin square leads to biased estimates of the error variance. The design has been used by a number of workers [113]. Ma and Harrington [201] report that 133 semi-latin squares (or modified latin squares) were used at the University of Saskatchewan and associated stations during the years 1936 to 1944. While recognizing the bias in the error variance for these designs, they estimated that the average error variance for comparable randomized complete block designs conducted over the same period was approximately equal to that for the semi-latin squares.

Yates [324, sec. 16i and 16j] has proposed a modification of the semi-latin square; he suggests arranging the $2k$ treatments in such a way that they correspond to the Latin and Greek letters in a graeco-latin square. He describes the arrangement and analysis for fourteen treatments arranged in a 7×7 graeco-latin square. He also gives a limited discussion of designs of this type for $3k$ treatments in a $k \times k$ hyper-graeco-latin square. Yates suggests that these designs may be suitable for treatment numbers between ten and twenty-four.

XIII-3.3 YOUDEN SQUARES

Youden [336, 337] introduced a set of incomplete latin squares which have become known as the *Youden square* design. This design combines features of the latin square and the balanced incomplete block designs. The Youden square design was proposed to control the variability due to position or order within an incomplete block. In studies on tobacco mosaic virus, Youden found that the position of a leaf on the plant affected the response. The number of leaves was insufficient to allow use of a $k \times k$ latin square. The design proposed by Youden allows for a two-way elimination of heterogeneity, such as that due to plants and position of leaves on a plant. All treatments appear in any given leaf position (rows) and only a fraction of the treatments appear on any given plant (columns or incomplete blocks). For example, if $v = 7$ treatments were to be compared and if there were only $k = 3$ available leaf positions, the appropriate Youden square design is

Row or position	Column or plant 1	2	3	4	5	6	7
1	7	1	2	3	4	5	6
2	1	2	3	4	5	6	7
3	3	4	5	6	7	1	2

If, on the other hand, $k = 4$ leaf positions were available to compare the $v = 7$ treatments, the appropriate Youden square design is[1]

Position	Column or plant 1	2	3	4	5	6	7
1	5	6	7	1	2	3	4
2	2	3	4	5	6	7	1
3	4	5	6	7	1	2	3
4	6	7	1	2	3	4	5

In the first Youden square above, $v = b = 7$, $k = r = 3$, and $\lambda = 1$, while in the second, $v = b = 7$, $k = r = 4$, and $\lambda = 2$. Each treatment appears with every other treatment once within the incomplete block (column) in the first Youden square, whereas each treatment appears twice with every other treatment in the second design.

Although the above method of constructing a Youden square from a specific latin square is suitable for some cases, other methods of construction are needed. Smith and Hartley [266] give a practical procedure for converting balanced incomplete block designs with $b = v$ into Youden square designs. Also, Cochran and Cox [60, Ch. 13] and Youden [337] list experimental plans for Youden squares for various values of v.

The randomization procedure for a Youden square design is

(i) to randomly assign the treatments to the numbers,
(ii) to randomly assign the columns to the incomplete blocks, and
(iii) to randomly assign the rows to the replicates.

If the Youden square is replicated, repeat steps (ii) and (iii) using another randomization. A complete latin square design (or sets of latin squares) is preferable to repetitions of the Youden square design, but if, owing to the nature of the experimental material, it is impossible to use a complete latin square design, repetitions of the Youden square design are used.

The analysis of variance for q repetitions of a Youden square is

Source of variation	df	Mean square: Observed	Mean square: Expected
Row (replicate)	$qk - 1$	—	—
Column or block (elim. treat.)	$q(b-1) = q(v-1)$	E_b	$\sigma_\epsilon^2 + \left(\frac{kq(v-1) - v + k}{q(v-1)}\right)\sigma_\beta^2$
Component (a)	$(q-1)(v-1)$	—	$\sigma_\epsilon^2 + k\sigma_\beta^2$
Component (b)	$(v-1)$	—	$\sigma_\epsilon^2 + (vk - v)\sigma_\beta^2/(v-1)$
Treatment (ignoring block)	$v - 1$		
Intrablock error	$(v-1)(qk - q - 1)$	E_e	σ_ϵ^2
Total	$qvk - 1$	—	—

[1] It may be noted that the above two Youden squares form a 7 × 7 latin square. Use may be made of this fact in setting up experiments on certain types of material. Separate and combined analyses are then possible.

The row or replicate, the total, and the treatment (ignoring block) sums of squares are obtained in the usual manner. The block (eliminating treatment) sum of squares is obtained as the sum of the sums of squares for components (*a*) and (*b*). The interaction of groupings of k treatments with repetitions of the basic plan yields a sum of squares with $(q - 1)(b - 1)$ degrees of freedom and is computed in the same manner as for previous component (*a*) sums of squares. The component (*b*) sum of squares is equal to

$$\sum_{j=1}^{v} W_j^2/qkv(v - k)(k - 1), \tag{XIII-72}$$

where

$$W_j = (v - k)X_{.j} - (v - 1)(SB)_j + (k - 1)X_{..}\,. \tag{XIII-73}$$

The above symbols are the ones used in the previous sections of this chapter.

The estimated weights are equal to

$$w = 1/E_e \tag{XIII-74}$$

and

$$w' = \frac{kq(v - 1) - v + k}{kq(v - 1)E_b - (v - k)E_e}. \tag{XIII-75}$$

The weighting factor is equal to

$$\mu = \frac{w - w'}{v(k - 1)w + (v - k)w'} = \frac{q(E_b - E_e)}{qv(k - 1)E_b + (v - k)(q - 1)E_e}. \tag{XIII-76}$$

The adjusted treatment mean with recovery of interblock information is equal to the unadjusted mean plus an adjustment; thus:

$$\bar{x}_j' = \bar{x}_j + \mu W_j/qk. \tag{XIII-77}$$

The standard error of a difference between two adjusted means is equal to

$$\sqrt{\frac{2E_e}{kq}\{1 + (v - k)\mu\}}. \tag{XIII-78}$$

The formula for a missing experimental unit is given by Cochran and Cox [60, p. 375]. Also, one may use the method for treating missing data as described by Rao [255] (a one is inserted for the missing observation and a zero for all other observations, and analysis of covariance is performed on the pseudo-variates composed of zeros and a one).

Youden [336, 337] and Cochran and Cox [60, Ch. 13] present the analysis for Youden squares without recovery of interblock information as well as with recovery.

XIII-3.4 OTHER DESIGNS

Cochran and Cox [60, Ch. 13] present a series of designs for small numbers of treatments which consist of a Youden square design added to a complete latin square design. A numerical example is also given. In connection with

these designs, it should be noted that it is simpler in some cases to use two or more latin squares than to use one of their designs. However, these designs are important and useful if only a fixed amount of material is available and this amount is less than required for sets of complete latin squares.

Pearce [241, 242] developed a number of designs of the latin square type. He considered the analysis and layout for latin squares with an additional column, with an additional row and an additional column, and with a column added and a row omitted. Shrikhande [264] considers some general classes of designs for the two-way elimination of heterogeneity. He presents balanced incomplete block designs for $b = mv$ where m is an integer. Shrikhande also considers the balanced incomplete blocks where b is not an integral multiple of v and shows that it is possible to obtain designs which have a two-way elimination of heterogeneity. For the latter designs, some pairs of treatments have one error variance while others have another error variance, whereas in a balanced incomplete block design with only one-way elimination of heterogeneity the error variance is the same for any pair. Bose and Kishen [28] and Shrikhande [264] developed a class of designs which they call partially balanced Youden squares. These designs are useful for larger treatment numbers than those generally suited to the Youden square design.

XIII-4 Split Plot Designs for Nonfactorial Experiments

For a large number of treatments, v, various experimental designs (see Chapters XI and XII and preceding sections of this chapter) may be utilized to control variability over the experimental area. If the experimental material falls into g natural groupings, it may be desirable to obtain more precise comparisons on the k individual items within each natural group than on comparisons between individual entries not in the same grouping. If only the contrasts on the k items within each natural grouping are desired, then each natural group may be tested in a separate experiment. If the comparison of individuals not in the same natural group is equal in importance to the comparison of individual entries in the same group, then the $gk = v$ entries are compared in a single experiment (e.g., in an incomplete block design). However, the actual situation is usually somewhere between these two extremes in that some information is desired on all comparisons but with greater precision on contrasts among individuals within a natural group.

Examples of material in natural groups are available from various fields. In plant breeding, one might be interested in g crosses and k selections from each cross; contrasts among selections within crosses may be of more importance than contrasts among selections from different crosses. Also, the nature of the material may make it advisable to group the material from each cross. In an animal breeding study, one might be interested in the characteristics of the g crosses with k litters of size r each. In a baking study, one might wish

to study characteristics on g recipes with k combinations of the ingredients of each recipe. Also, one might be interested in g teaching methods with k teachers using each method; a total of gk teachers participate in the experiment, and each teacher teaches r classes. These illustrations represent but a few of the examples and fields that contain material of this nature. Yates [322], Cochran [47], and others [165, 227, 255] have proposed designs suitable for these conditions. The construction and analysis of two of the designs are discussed below.

XIII-4.1 GROUPS OF SIMILAR TREATMENTS IN EACH WHOLE PLOT

If g crosses with k selections per cross represent the v treatments, a "split plot design" with the g crosses randomly allotted to the g whole plots in each replicate and the k selections per cross randomly allotted to the k experimental units or "split plots" within each whole plot may be used. For r replicates the analysis of variance is of the following form:

Source of variation	df	Mean square
Replicate	$r - 1$	—
Crosses or whole plots	$g - 1$	—
Error (a)	$(r - 1)(g - 1)$	E_a
Selections within cross 1	$k - 1$	—
Selections within cross 2	$k - 1$	—
.		.
.		.
.		.
Selections within cross g	$k - 1$	—
Error (b)	$g(k - 1)(r - 1)$	E_b

In the above analysis the interaction of the k selections with replicates for each cross may be computed; this interaction has $(k - 1)(r - 1)$ degrees of freedom. If the individual interaction mean squares are all estimates of the same quantity, say σ_β^2, they may be pooled into the single mean square E_b with $g(k - 1)(r - 1)$ degrees of freedom. Also, the above split plot design may be analyzed as g separate randomized complete block experiments if desired. This is possible despite the fact that the separate replicates for one cross are intermingled with the corresponding replicates for the other crosses.

The standard error of a difference between the means of two selections from the *same* cross is

$$\sqrt{2E_b/r}, \qquad \text{(XIII-79)}$$

of two selections from different crosses is

$$\sqrt{\frac{2}{kr}(E_a + (k - 1)E_b)}, \qquad \text{(XIII-80)}$$

and of two cross means is

$$\sqrt{2E_a/rk}. \tag{XIII-81}$$

The above design has the advantages that the natural groupings are retained and that comparisons on the selections within a group or cross are more precise than without grouping. The disadvantage of this design is that less precision is available on comparisons of selections not in the same cross.

Instead of having g randomized complete block designs intermingled in such a way that complete replicates are formed, g incomplete block designs could be used. That is, an incomplete block design suitable for k treatments in r replicates could be selected. The same design with different randomizations would be used for the g crosses. Also, the g whole plots could be designed as a $g \times g$ latin square. Formulae (XIII-79) to (XIII-81) would still hold with the corresponding effective error mean squares replacing E_a and E_b.

XIII-4.2 INCLUSION OF RANDOM CONTROLS IN THE SPLIT PLOTS

With the idea of increasing the precision on selections from different crosses, controls have often been added to the k treatments comprising the split plot. The addition of c controls within each whole plot increases the size of the whole plot from k to $k + c$ experimental units. The total additional experimental units required is grc. The use made of the additional experimental units is to make comparisons with the controls and to compare treatments in different randomized complete block designs through their common controls. If two treatment means, say $\bar{x}_1$ and $\bar{x}_2$, not in the same group are compared through the controls in these two groups, say $\bar{x}_{c1}$ and $\bar{x}_{c2}$, the comparison of $(\bar{x}_1 - \bar{x}_{c1})$ with $(\bar{x}_2 - \bar{x}_{c2})$ has the following standard error:

$$\sqrt{s_{\bar{x}_1}{}^2 + s_{\bar{x}_{c_1}}{}^2 + s_{\bar{x}_2}{}^2 + s_{\bar{x}_{c_2}}{}^2}, \tag{XIII-82}$$

which is equal to $\sqrt{4s^2/r}$ if all means are subject to the same error variance and if all means have the same number of replications.

In the event that more than one experimental unit per whole plot receives the check treatment, Yates [322] has shown that the optimum number of checks to include in the block for a given number of experimental units is

$$c = -1 + \sqrt{1 + k}, \tag{XIII-83}$$

where the greatest error variance of any comparison is used as the criterion for assessing the accuracy of an experiment. The integer nearest to the value of the above expression is used for the value of c.

Yates [322] states that the use of random controls is less efficient than the systematic spacing of controls in a randomized complete block experiment. He points out that with smaller groups a latin square design may be used; both he and Cochran [47] state that there is not much to recommend these designs and the methods of comparison discussed in this section. They state that the

design of the preceding section or some other incomplete block design is preferable to those discussed in the present section.

XIII-5 Linked Block Designs

In a brief dissertation, Youden [338] and Eisenhart [101] discuss the design and analysis for the partially balanced incomplete block designs designated as Youden linked block designs. It is stated [338] that these designs possess "symmetry, ease of analysis, reasonable block size and a small number of replications." Examples of the linked block designs are listed below.

k	b	v	r
4	5	10	2
5	6	15	2
4	9	12	3
6	7	14	3
6	13	26	3
7	15	35	3

Bose and Shimamoto [32] discuss the relationship of linked block designs to other p.b.i.b. designs. Additional work on these designs has been reported by Youden and Conner [339] and by Mandel [206].

CHAPTER XIV

Balanced Designs

XIV-1 Introduction

Experimental designs discussed thus far pertain, for the most part, to situations where the total number of experimental units is equal to the number of treatments times the number of replicates and where each experimental unit is used only once. Designs discussed in the present chapter pertain to situations in which the treatments are applied in sequence over several periods to a group of individual items and in which the number of experimental units may be less than the number of observations. Each period of observation represents a cycle, and several cycles are required to complete the requirements of balance in the designs. Since the aspect of balance through time or over cycles is important in the designs discussed in the present chapter, the term *balanced designs* is used to include all such designs. Particular balanced designs have been discussed under various names; e.g., cross-over [57, 117, 198], switch-back [273], switch-over [38, 273], reversal [38, 273], change-over [57, 60, 175, 198-200], double change-over [57, 175], double-reversal [38, 198, 235, 263, 273], double change-over with the last period repeated [157, 198], rotation [46, 77, 331], etc. [236, 325]. A brief discussion of some of the designs is presented in the following sections, and the similarities of the designs are noted.

In certain types of experiments (e.g., rotation) the length of the period is fixed. In others, the length of the period of observation is determined by the experimenter. Care should be taken to insure that the period is of sufficient duration to allow expression of the treatment effect. Also, the period should be long enough so that the effect of the preceding treatment does not extend beyond the next period.

A treatment effect that continues after the treatment has been discontinued is called a *carry-over* or *residual* effect. The carry-over effect from a previous treatment affects the measurement of the effect of the present treatment. This carry-over effect may be taken care of by experimental design or by inserting a "rest period" between the treatment periods. The rest period may consist of a period of no observation on the present treatment or of a period of no treatment. The lengths of the rest period and of the

period of observation on the treatment need not be of the same duration and are determined by the nature of the experimental material and by the character measured.

XIV-2 Change-Over Designs

XIV-2.1 THE SIMPLE CHANGE-OVER DESIGN

A design combining the features of the latin square and the randomized complete block designs has been used for comparing two to four treatments in some dairy husbandry and other biological studies. This design may also be used to advantage in psychological and marketing research. Like a latin square the cross-over design has two restrictions imposed on the randomization of the "treatments" to the "plots" or individual units. The treatments are all included in each replicate or group. The individual units are rated with regard to time of application in each replicate or group. The second restriction is that each treatment must be applied an equal number of times in each period or time in the replicates.

In the simplest case, consider two treatments, A = supplemental feeding and B = no supplemental feeding, administered to six dairy cows. Each cow is to receive treatments A and B in periods 1 and 2. The treatments A and B are allotted to the periods at random with the *restriction* that *half* of the cows receive treatment A and the other half receive treatment B in period 1, and the cows receiving treatment A (or B) in period 1 receive treatment B (or A) in period 2. The experimental design for the six replicates (six cows) is of the following nature:

	Columns (cows or replicates)					
Rows	I	II	III	IV	V	VI
Period 1	B	B	A	A	B	A
Period 2	A	A	B	B	A	B

If the experiment is conducted as three 2×2 latin squares, the design is of the following nature:

	Square I		Square II		Square III	
Rows	Columns					
Row 1	A	B	A	B	B	A
Row 2	B	A	B	A	A	B

where the six cows are grouped into three pairs. The difference between the cross-over design and sets of latin squares is illustrated by the following breakdown of degrees of freedom in the respective analyses of variance:

Cross-over design Source of variation	df	Three 2 × 2 latin squares Source of variation	df
Column or pair	5	Square	2
Row (period 1 vs period 2)	1	Columns in squares	3
Treatment	1	Rows in squares	3
Residual = error	4	Treatment	1
		Tr. × square = error	2
Total	11	Total	11

The main difference lies in the fact that there are more degrees of freedom associated with the error sum of squares in the cross-over design and that there is a less complete elimination of row effects. This design is suitable when row differences are approximately equal in all replicates, and, therefore, most of the row effects are removed by the single degree of freedom for rows, and the error mean square is no larger than for the latin square. Also, there are more degrees of freedom available for error.

The cross-over design may be used for any number of treatments with the condition that the number of replicates must be a multiple of the number of treatments, but it is inadvisable to use a cross-over design in place of a latin square if there are more than four treatments.

If the above design were applied to an experimental situation which requires a separate experimental unit for each treatment in each replicate, the analysis would be the same as that given above. For example, suppose that two treatments, A and B, are applied to dairy cows, that one treatment period is used, that twelve dairy cows are available for the experiment, that the twelve cows are paired into six pairs with each member of the pair being rated as "superior" or "inferior," and that one-half of the superior and one-half of the inferior cows receive treatment A and the remaining cows receive treatment B. The experimental design might be of the following form:

Rows	Cows or replicates					
Superior	B	B	A	A	B	A
Inferior	A	A	B	B	A	B

Pearce [242] has denoted this type of design as a "tied latin square" to distinguish it from the simple change-over design. In the latter design, different

treatments are applied to the same experimental unit in two or more time periods; i.e., there is a changing of the treatments on a unit. In the tied latin square design, no changing of the treatments is involved, and there are as many experimental units as there are observations.

For three treatments the simple change-over design is of the following form (other arrangements are possible):

	Columns or replicates					
Rows (period)	1	2	3	4	5	6
1	A	A	B	C	C	B
2	C	B	C	B	A	A
3	B	C	A	A	B	C

Multiples of three replicates are required in order to have each treatment appear in each row an equal number of times. An alternative design for the above is to use two 3 × 3 latin squares. The breakdown of the degrees of freedom in the analysis of variance for the simple change-over design and for two 3 × 3 latin squares is

Change-over design Source of variation	df
Replicate (column)	5
Row	2
Treatment	2
Error	8
Total	17

Two 3 × 3 latin squares Source of variation	df
Square or set	1
Columns within square	4
Rows " "	4
Treatment	2
Treatment × square	2
Error within square	4

If the last two mean squares in the analysis for the 3 × 3 latin squares may be pooled justifiably, 6 degrees of freedom are associated with the error for treatments, while 8 degrees of freedom are associated with the error mean square in the change-over design. Thus, if the rows with two degrees of freedom control approximately the same amount of variation as the rows within squares with four degrees of freedom, the change-over design is more efficient than two 3 × 3 latin squares. Even if the error mean square in the change-over design is not appreciably smaller than the error mean square for the 3 × 3 latin squares, the extra degrees of freedom associated with the error mean square in the simple change-over design make it the preferred design.

Simple change-over designs for four or more treatments may be set up in the manner described above for two and three treatments. The analysis follows that outlined in example XIV-1.

Fieller [117] and Cochran and Cox [60, sec. 4.4] illustrate the method of analysis for a missing datum, and Fieller [117] describes the covariance analysis for the simple change-over design.

Example XIV-1. Suppose that two merchandising practices, C and E, are compared in a store for a period of eight weeks. Furthermore, suppose that the display counter is such that the customers approach from one end of the counter. With regard to the product and to the practices used in this experiment, it is suspected that customers will tend to select items from the practice that is observed first. Therefore, the display counter is divided into two parts, first and second positions, and the design is such that both practices appear in the store in every week and each practice appears in the first position during four of the eight weeks and in the second position during the remaining four weeks. The procedure is to allot the practices randomly to the weeks with the restriction that treatments C and E must each appear in the first position in four of the weeks. The arrangement, the total sales in pounds per week (artificial), and the various totals are given in table XIV-1. The total, replicate (week), row (position), treatment, and residual sums of squares are computed as follows:

TABLE XIV-1. Total sales (pounds/week) for two merchandising practices

Position	Week or replicate number								Total
	1	2	3	4	5	6	7	8	
1	E-71	C-66	E-49	C-59	E-50	E-50	C-48	C-59	452
2	C-50	E-42	C-49	E-50	C-49	C-43	E-31	E-48	362
Total	121	108	98	109	99	93	79	107	814

Total for C = 423; total for E = 391.

Analysis of variance

Source of variation	df	ss	ms
Replicate	7	552.75	78.96
Row (position)	1	506.25	506.25
Treatment	1	64.00	64.00
Residual	6	208.75	34.79

Correction term (1 *df*) $= 814^2/16 = 41412.25$.
Total (15 *df*) $= 71^2 + 50^2 + \cdots + 48^2 - 41412.25 = 1331.75$.
Replicate (7 *df*) $= (121^2 + \cdots + 107^2)/2 - 41412.25 = 552.75$.
Treatment (1 *df*) $= (423 - 391)^2/16 = 64.00$.
Row (1 *df*) $= (452 - 362)^2/16 = 506.25$.
Residual (6 *df*) $= 1331.75 - (552.75 + 64.00 + 506.25) = 208.75$.

The standard error of a difference between two treatment means is $\sqrt{2(34.79)/8} = 2.95$. The 95 per cent confidence interval on the difference in pounds sold per week from the two merchandising practices is $\frac{423 - 391}{8} - (2.95)(2.447) = -3.2$ to $4.0 + 7.2 = 11.2$ pounds per week. The coefficient of variation is equal to $100(16)\sqrt{34.79}/814$

= 12 per cent. The error variance, 34.79, is much smaller than it would have been without the stratification into rows. The single degree of freedom for rows is associated with a relatively large amount of the total sum of squares. The efficiency of the design relative to the comparable randomized complete block design is

$$\frac{506.25 + 34.79(6+1)}{8(34.79)}\left(\frac{6+1}{6+3}\right)\left(\frac{8+3}{8+1}\right) \times 100 = 256 \text{ per cent.}$$

XIV-2.2 DOUBLE-REVERSAL DESIGNS WITH EXTENSIONS—NO RESIDUAL EFFECT FROM PREVIOUS TREATMENT

Brandt [38] describes the analysis for a group of designs known as the double-reversal, switch-back, or double change-over designs. Basically the design involves two treatments and two sequences of treatments over the three periods of observation; thus:

Period	Sequence of treatments	
I	A	B
II	B	A
III	A	B

If four periods are used, the sequences are

Period	Sequence of treatments	
I	A	B
II	B	A
III	A	B
IV	B	A

The above system may be used for five or more periods. However, it is unlikely that the total usable period will be composed of more than three or four experimental periods.

The analysis for the double-reversal design is somewhat different in that use is made of the quadratic time trend on each subject, animal, or item for three periods, of the cubic time trend on each subject for four periods, etc. To illustrate, suppose that ten subjects are available to compare two treatments over three time periods. (Presumably the subjects are homogeneous with respect to the character measured in the absence of any treatment effect.) Then, the five subjects are randomly allotted to each of the two sequences of treatments. The allotment of treatments is

Period	Sequence ABA					Sequence BAB				
	1	2	3	4	5	6	7	8	9	10(=0)
I	A_{11}	A_{21}	A_{31}	A_{41}	A_{51}	B_{61}	B_{71}	B_{81}	B_{91}	B_{01}
II	B_{12}	B_{22}	B_{32}	B_{42}	B_{52}	A_{62}	A_{72}	A_{82}	A_{92}	A_{02}
III	A_{13}	A_{23}	A_{33}	A_{43}	A_{53}	B_{63}	B_{73}	B_{83}	B_{93}	B_{03}

where the symbol with the subscript represents the yield, the first subscript refers to the subject, and the second subscript refers to the period. The difference between the two treatments is estimated to be

$$\frac{1}{5}\left[\sum_{i=1}^{5}(A_{i1} - 2B_{i2} + A_{i3}) - \sum_{i=6}^{10}(B_{i1} - 2A_{i2} + B_{i3})\right], \qquad \text{(XIV-1)}$$

with a variance equal to

$$\left(\frac{2}{5}\right)\left(\frac{1}{2(5-1)}\right)\left[\sum_{i=1}^{5}(A_{i1} - 2B_{i2} + A_{i3})^2 - \left[\sum_{i=1}^{5}(A_{i1} - 2B_{i2} + A_{i3})\right]^2/5\right.$$
$$\left. + \sum_{i=6}^{10}(B_{i1} - 2A_{i2} + B_{i3})^2 - \left[\sum_{i=6}^{10}(B_{i1} - 2A_{i2} + B_{i3})\right]^2/5\right]. \qquad \text{(XIV-2)}$$

The above variance has $2(5 - 1) = 8$ degrees of freedom. Both Brandt [38] and Snedecor [273, sec. 15.8] give illustrative examples of the above.

Seath [263] devised a double-reversal design for a 2×2 factorial set of treatments; thus:

	Sequence			
Period	1	2	3	4
I	00	11	01	10
II	11	00	10	01
III	00	11	01	10

The first subscript refers to the level of the first factor and the second subscript to the level of the second factor. The quadratic term (formula (XIV-2)) is used to test main effects; a larger error (interaction of groups of sequences and levels of AB) is used to test the AB interaction. This design overcomes, to some extent, the difficulty of comparing only two treatments and still retains the high precision usually associated with the designs [198].

XIV-2.3 CHANGE-OVER DESIGNS TO MEASURE RESIDUAL EFFECTS

In experiments where the treatments are applied in sequence to the same animal, the same store, the same field plot, etc., the effect of certain treatments continues after the application of the treatment is discontinued. The effect of a given treatment is influenced by the carry-over or residual effect of the previous treatment. One method of eliminating the effect of the previous treatment is to insert a rest period between the treatment periods. In this way the treatment effects are freed of most of the residual effects. However, it is not always possible or desirable to use such a procedure. The alternative is to use an experimental design which yields a measurement of both *residual* and *direct* effects of treatments and an adjustment of the direct effects for residual

effects, and *vice versa*. Cochran *et al.* [57] discuss the design and analysis for an experimental design which meets these requirements. Their results are given below for three treatments and for four treatments.

For three treatments (A, B, C) the following design is used:

Sequence number

Period	Square 1			Square 2		
	1	2	3	4	5	6
I	A	B	C	A	B	C
II	B	C	A	C	A	B
III	C	A	B	B	C	A

In square 1, treatment B follows treatment A on items 1 and 3. In square 2, treatment A follows treatment B on items 5 and 6. The same balance is achieved for other combinations of treatments in the two orthogonal latin squares. (The particular randomization of the last design in section XIV-2.1 contains the six sequences of treatments given above. In that design, no attention is paid to the groups or squares.) The six items should be grouped into two groups in such a way that the groups are as different as possible and that the items within groups are as homogeneous as possible. Then, a square is randomly allotted to the groups, and the items within a group are randomly allotted to the three sequences of treatments.

With four treatments the following three 4×4 orthogonal latin squares yield a double change-over design for measuring residual effects:

Sequence number

Period	Square 1				Square 2				Square 3			
	1	2	3	4	5	6	7	8	9	10	11	12
I	A	B	C	D	A	B	C	D	A	B	C	D
II	B	A	D	C	D	C	B	A	C	D	A	B
III	C	D	A	B	B	A	D	C	D	C	B	A
IV	D	C	B	A	C	D	A	B	B	A	D	C

Thus, twelve items are sufficient to obtain the important features of the design, although there are twenty-four possible sequences. Lucas [198] points out that a single 4×4 latin square also satisfies the requirement of symmetry. The above design with twelve sequences of treatments is the one discussed below. The randomization procedure follows that for three treatments.

For other numbers of treatments (e.g., 5, 7, etc.) the $k - 1$ orthogonal[1] squares of a $k \times k$ latin square are selected. The randomization procedure

[1]These are the orthogonal squares defined in Chapter XV and not necessarily those described by Bose and Nair [31].

follows that described above for three and four treatments. Also, residual effects may sometimes be estimated from less than $k - 1$ orthogonal $k \times k$ latin squares, but the effective number of replicates may be too small for the purposes of the experiment. Even with $k - 1$ orthogonal latin squares the ratio of the effective number of replicates of residual effects relative to direct effects is 2 to 3 for three treatments and 3 to 4 for four treatments [198].

The advantages of the double change-over design are:

(i) The design allows estimation of residual effects as well as direct effects of the treatments.
(ii) A high degree of precision is usually attained with this design [198].
(iii) The design is suitable for small numbers of treatments.

The disadvantages of the double change-over design are:

(i) The effective number of replicates for residual effects is less than for direct effects.
(ii) The design is usually limited to three or four treatments.

Lucas [198] states that carry-over effects may be estimated from certain Youden square and lattice square designs but that the large number of items (animals, stores, etc.) required is too large for most experimental setups.

The sums of squares for group, item within group, period, period $\times$ group, and for the total are computed in the same manner as for groups of latin squares (see Chapter VI). The new features in the analysis for the double change-over design are indicated in the following analysis of variance table:

Source of variation	3 treatments df	4 treatments df
Total corrected for the mean	$18q - 1$	$48q - 1$
Group	$2q - 1$	$3q - 1$
Period	2	3
Item within group	$4q$	$9q$
Period $\times$ group	$2(2q - 1)$	$3(3q - 1)$
Treatment (direct and residual)	4	6
Direct (ignoring residual)	2	3
Residual (eliminating direct)	2	3
Direct (eliminating residual)	2	3
Residual (ignoring direct)	2	3
Error	$8q - 4$	$27q - 6$

The sums of squares for the direct and residual effects of treatments are obtained from the following formulae. (The residual or carry-over effects are partially confounded with items. The sum of squares for residual effects is computed eliminating the effect of items, but the sum of squares for items is computed ignoring the residual effects.)

For three treatments:

(i) Direct effect of treatments *ignoring* residual effects:

$$\sum_{h=1}^{3} X_{..h.}^2/6q - X_{....}^2/18q. \tag{XIV-3}$$

(ii) Direct effect of treatments *eliminating* residual effects:

$$\frac{[5X_{..A.} + 2a - b - c + s_2 + s_6 - 2X_{....}/3]^2}{120q} + \frac{[5X_{..B.} + 2b - a - c + s_3 + s_4 - 2X_{....}/3]^2}{120q} + \frac{[5X_{..C.} + 2c - a - b + s_1 + s_5 - 2X_{....}/3]^2}{120q} - \frac{[4X_{....}]^2}{360q}. \tag{XIV-4}$$

(iii) Residual effect of treatments *ignoring* direct effects:

$$\frac{[3a + s_2 + s_6]^2 + [3b + s_3 + s_4]^2 + [3c + s_1 + s_5]^2}{30q} - \frac{[3(a + b + c) + X_{....}]^2}{90q}. \tag{XIV-5}$$

(iv) Residual effect of treatments *eliminating* direct effects:

$$\frac{[3a + X_{..A.} + s_2 + s_6]^2 + [3b + X_{..B.} + s_3 + s_4]^2 + [3c + X_{..C.} + s_1 + s_5]^2}{24q} - \frac{[3(a + b + c) + 2X_{....}]^2}{72q}. \tag{XIV-6}$$

For four treatments:

(i) Direct effect of treatments *ignoring* residual effects:

$$\sum_{h=1}^{4} X_{..h.}^2/12q - X_{....}^2/48q. \tag{XIV-7}$$

(ii) Direct effect of treatments *eliminating* residual effects:

$$\frac{[11X_{..A.} + 3a - b - c - d + s_4 + s_7 + s_{10} - X_{....}/2]^2}{1320q} + \cdots + \frac{[11X_{..D.} + 3d - a - b - c + s_1 + s_8 + s_{11} - X_{....}/2]^2}{1320q} - [10X_{....}]^2/4q(1320). \tag{XIV-8}$$

(iii) Residual effect of treatments *ignoring* direct effects:

$$\frac{[4a + s_4 + s_7 + s_{10}]^2 + \cdots + [4d + s_1 + s_8 + s_{11}]^2}{132q} - \frac{[4(a + b + c + d) + X_{....}]^2}{528q}. \tag{XIV-9}$$

(iv) Residual effect of treatments *eliminating* direct effects:

$$\frac{[4a + X_{..A.} + s_4 + s_7 + s_{10}]^2 + \cdots + [4d + X_{..D.} + s_1 + s_8 + s_{11}]^2}{120q} - [4(a + b + c + d) + 2X_{....}]^2/480q. \tag{XIV-10}$$

The error sum of squares is obtained by subtraction.

The linear model for the gth repetition of the double change-over design is

$$X_{ijhg} = \mu + \alpha_i + \beta_{jg} + (\alpha\beta)_{ijg} + \delta_h + \rho_p + \epsilon_{ijhg}, \tag{XIV-11}$$

where μ = mean effect, α_i = effect of ith period, β_{jg} = effect of jth sequence in gth repetition, $(\alpha\beta)_{ijg}$ = effect of the jth sequence in the gth repetition in the ith period, δ_h = direct effect of the treatment, ρ_p = residual effect of the preceding treatment, and ϵ_{ijhg} = random component of error; $g = 1, 2, \cdots, q$; $i = 1, 2, \cdots, k$; $j = 1, 2, \cdots, k(k-1)$; $h = 1, 2, \cdots, k$; and $p = 1, 2, \cdots, k$. In the sums of squares in formulae (XIV-3) to (XIV-10), $X_{....}$ = grand total of all observations; $X_{..h.}$ = treatment totals; $h = A = 1, B = 2, C = 3, D = 4$; q = number of repetitions of the basic design; s_j = sequence total; $j = 1, 2, \cdots, 6$ for three treatments and $j = 1, 2, \cdots, 12$ for four treatments; a = sum of yields of treatments which received treatment A in the preceding period; b = sum of yields of treatments which received treatment B in the preceding period; c = sum of yields of treatments which received treatment C in the preceding period; and d = sum of yields of treatments which received treatment D in the preceding period.

For three treatments the treatment means adjusted for residual effects are[1]

$$[5X_{..A.} + 2a - b - c + s_2 + s_6 - 2X_{....}/3]/24q, \tag{XIV-12}$$

$$[5X_{..B.} + 2b - a - c + s_3 + s_4 - 2X_{....}/3]/24q, \tag{XIV-13}$$

and

$$[5X_{..C.} + 2c - a - b + s_1 + s_5 - 2X_{....}/3]/24q; \tag{XIV-14}$$

for four treatments the adjusted means are

$$[11X_{..A.} + 3a - b - c - d + s_4 + s_7 + s_{10} - X_{....}/2]/120q, \tag{XIV-15}$$

$$[11X_{..B.} + 3b - a - c - d + s_3 + s_8 + s_9 - X_{....}/2]/120q, \tag{XIV-16}$$

$$[11X_{..C.} + 3c - a - b - d + s_2 + s_5 + s_{12} - X_{....}/2]/120q, \tag{XIV-17}$$

and

$$[11X_{..D.} + 3d - a - b - c + s_1 + s_6 + s_{11} - X_{....}/2]/120q. \tag{XIV-18}$$

For three treatments the formulae for the residual effects adjusted for direct effects of treatments are

$$[3a + X_{..A.} + s_2 + s_6]/8q, \tag{XIV-19}$$

$$[3b + X_{..B.} + s_3 + s_4]/8q, \tag{XIV-20}$$

and

$$[3c + X_{..C.} + s_1 + s_5]/8q; \tag{XIV-21}$$

for four treatments the adjusted residual effects are

$$[4a + X_{..A.} + s_4 + s_7 + s_{10}]/30q, \tag{XIV-22}$$

$$[4b + X_{..B.} + s_3 + s_8 + s_9]/30q, \tag{XIV-23}$$

$$[4c + X_{..C.} + s_2 + s_5 + s_{12}]/30q, \tag{XIV-24}$$

and

$$[4d + X_{..D.} + s_1 + s_6 + s_{11}]/30q. \tag{XIV-25}$$

[1]Note that sequences 2 and 6 contain treatment A in period III, that treatment B is followed by treatment C in sequence 2, and that treatment C is followed by treatment B in sequence 6. Similar relationships among the treatments are found in the other pairs of sequences, 3 and 4 and 1 and 5.

Alternatively, the treatment mean adjusted for residual effects may be obtained by subtracting the mean residual deviations for the other treatments from the unadjusted treatment mean. For example, the treatment mean adjusted for residual effects for A is equal to

$$\frac{X_{..A.}}{6q} - \left[\text{formula (XIV-20)} + \text{formula (XIV-21)} - \frac{6(a+b+c)+4X_{....}}{3q(24)}\right] \tag{XIV-26}$$

for a design with three treatments and is equal to

$$\frac{X_{..A.}}{12q} - \left[\text{(XIV-23)} + \text{(XIV-24)} + \text{(XIV-25)} - \frac{12(a+b+c)+6X_{....}}{4q(120)}\right] \tag{XIV-27}$$

for a design with four treatments. The other adjusted treatment values may be computed in the same manner; the adjusted treatment means will usually be computed from formulae (XIV-19) to (XIV-21) or from formulae (XIV-22) to (XIV-25).

The standard error for an adjusted treatment mean for three treatments is

$$\sqrt{\frac{1.25 \text{ error mean square}}{6q}}, \tag{XIV-28}$$

and for four treatments is

$$\sqrt{\frac{1.1 \text{ error mean square}}{12q}}. \tag{XIV-29}$$

The design and analysis for a double change-over design with three treatments are illustrated in the following example. The example is particularly useful in illustrating the computation of the quantities a, b, and c.

Example XIV-2. Henderson [157] conducted an experiment in retail grocery stores in the fall of 1951 to determine the effect on McIntosh apple sales (in pounds) due to packaging the apples in 4-pound (treatment A), 6-pound (treatment B), and 8-pound (treatment C) Polythene bags (table XIV-2). Six retail grocery stores in central New York were used. Each treatment was left in a store for one week. It was assumed that the effect of an apple purchase in one week did not extend beyond the succeeding week. With bags of the size used in this experiment the assumption may be regarded as valid. For larger bags of apples a longer period of observation for each treatment is recommended.

The six stores were divided into two groups, with group I (stores 1, 2, 3) containing the smallest stores and group II (stores 4, 5, 6) containing the largest stores. For the purposes of the experiment, another store doing approximately the same volume of business as stores 4 and 5 should have been used in place of store 6. This is not always possible, as it is usually difficult to enlist the aid of store managers, and the experimenter must use the stores whose managers will cooperate in the experiment. The store numbers were assigned at random to the stores within each group, and each group of stores was randomly assigned to the particular 3 × 3 latin square.

TABLE XIV-2. Pounds of McIntosh apples sold per week in 4(A), 6(B), 8(C) pound Polythene bags in six stores

Period	Store and treatment (pounds of apples) 1	2	3	Total	4	5	6	Total	Total
I	A-720	B-1020	C-640	2380	A-1640	B-2080	C- 960	4680	7060
II	B-900	C- 820	A-720	2440	C-1560	A-1560	B-1120	4240	6680
III	C-720	A- 920	B-800	2440	B-2090	C-1784	A- 900	4774	7214
Total Set total	2340 $= s_1$	2760 $= s_2$	2160 $= s_3$	7260	5290 $= s_4$	5424 $= s_5$	2980 $= s_6$	13694	20954

Data for adjusting yields

Treatment	Total	Sum of yields of treatments in previous period	Sum of set totals	2/3 grand total
A	$6460 = X_{..A}$	$a = 5044$	$s_2 + s_6 = 5740$	
B	$8010 = X_{..B}$	$b = 4000$	$s_3 + s_4 = 7450$	
C	$6484 = X_{..C}$	$c = 4850$	$s_1 + s_5 = 7764$	13969

Treatment means (per week)

Treatment	Unadjusted Mean lbs.	Increase over A lbs.	per cent	Adjusted Mean lbs.	Increase over A lbs.	per cent
A	1077	-	-	1055	-	-
B	1335	258	24.0	1318	263	24.9
C	1081	4	0.4	1120	65	6.2
Total	3493			3493		

Estimate of carry-over effect per treatment per store (total per week)

Treatment	Total carry-over effect per treatment	Deviations from mean in pounds per week
A	$(3a + X_{..A} + s_2 + s_6)/24 = 27332/24 = 1139$	-66
B	$(3b + X_{..B} + s_3 + s_4)/24 = 27460/24 = 1144$	-51
C	$(3c + X_{..C} + s_1 + s_5)/24 = 28798/24 = 1200$	117
Mean	$(27332 + 27460 + 28798)/72 = 1161$	0

Analysis of variance

Source of variation	df	ss	ms
Group	1	2299798	-
Period	2	25182	12591
Period x group	2	29795	14898
Stores within group	4	1321777	-
Direct (ign. carry-over)	2	262875	-
Carry-over (elim. direct)	2	54941	27470
Direct (elim. carry-over)	2	181106	90553
Carry-over (ign. direct)	2	136710	-
Error	4	6804	1701
Total	17	4001172	-
Correction for mean	1	24392784	-
Total uncorrected	18	28393956	-

In performing the analysis of a double change-over design [57] the first step is to obtain the store totals (s_i) over the three periods, the period totals both within and over groups, the group totals, and the grand total (table XIV-2). The second step is to obtain the treatment totals over all stores, the sum of the yields in the previous period, and the sum of the set totals. The sum of the yields receiving that particular treatment in the previous period for treatment A is equal to

$$a = 900 + 800 + 1560 + 1784 = 5044,$$

for treatment B is equal to

$$b = 720 + 820 + 1560 + 900 = 4000,$$

and for treatment C is equal to

$$c = 920 + 720 + 2090 + 1120 = 4850.$$

The unadjusted treatment mean in pounds of apples per week is merely the unadjusted total divided by 6, the number of stores. The adjusted means in pounds of apples per week are

For treatment A (*from formula* (*XIV-12*)):

$$\begin{aligned}\bar{x}_A &= \tfrac{1}{24}\{5(6460) + 2(5044) - 4000 - 4850 + 2760 + 2980 - 13969\}\\ &= 25309/24 = 1055.\end{aligned}$$

For treatment B (*from formula* (*XIV-13*)):

$$\begin{aligned}\bar{x}_B &= \tfrac{1}{24}\{5(8010) + 2(4000) - 5044 - 4850 + 2160 + 5290 - 13969\}\\ &= 31637/24 = 1318.\end{aligned}$$

For treatment C (*from formula* (*XIV-14*)):

$$\begin{aligned}\bar{x}_C &= \tfrac{1}{24}\{5(6484) + 2(4850) - 5044 - 4000 + 2340 + 5424 - 13969\}\\ &= 26871/24 = 1120.\end{aligned}$$

The estimates of the carry-over effects per treatment per store in total pounds of apples sold in one week are given in table XIV-2 (formulae (XIV-19) to (XIV-21)). The method of computation is indicated in the table. For example, the carry-over effect for treatment A is $[3(5044) + 6460 + 2760 + 2980]/24 = 27332/24 = 1139$. The difference in carry-over effect between treatments B and A is $3432 - 3417 = -51 - (-66) = 15$ pounds of apples per week, between C and A is $3600 - 3417 = 117 - (-66) = 183$ pounds, and between C and B is $3600 - 3432 = 168$ pounds. The corresponding differences for the direct effects are $1318 - 1055 = 263$ pounds for B and A, $1120 - 1055 = 65$ pounds for C and A, and $1120 - 1318 = -198$ pounds for C and B. The carry-over effect from treatment C to treatments A and B is large; i.e., 183 and 168 pounds, respectively. The carry-over effect from treatment B to A is relatively small. From a practical standpoint, this means that a purchase of 4 or 6 pounds of apples in one week did not affect the purchases of apples in the succeeding week. However, the purchase of 8-pound bags of apples in a given week did affect the purchases of apples in the next week. Evidently, 8 pounds of apples are more than enough for one week's supply for the average household buying apples in Central New York. Also, the total sales of apples were increased by using the 6-pound bag,

the differences between treatment B and treatments A and C being 263 and 198 pounds, respectively. These differences are rather large in comparison with the difference, 65, in amount of apples sold in 8-pound and 4-pound bags.

Also, the adjustments to the three treatment means may be obtained from the deviations of means for carry-over effect from the average mean of the carry-over effects [57] (see formula (XIV-26)); thus,

$$\begin{aligned} \text{Adjusted mean for } A &= 1077 - (-17 + 39) = 1055. \\ \text{"} \quad \text{"} \quad \text{"} \quad B &= 1335 - (-22 + 39) = 1318. \\ \text{"} \quad \text{"} \quad \text{"} \quad C &= 1081 - (-22 - 17) = 1120. \end{aligned}$$

The group (or square), period, period by group, stores within group, and total sums of squares presented in the bottom part of table XIV-2 are obtained by the procedure appropriate for the analysis of two latin squares (see example VI-2). The new features in the analysis of the double change-over design relative to the analysis of two latin squares are illustrated in the computation of the following sums of squares (formulae (XIV-3) to (XIV-6)):

Direct effects of treatments ignoring residual effects (2 df):

$$\frac{6460^2 + 8010^2 + 6484^2}{6} - \frac{20954^2}{18} = 262875.$$

Direct effects of treatments eliminating residual effects (2 df):

$$\frac{25309^2 + 31637^2 + 26871^2}{120} - \frac{83817^2}{360} = 181106.$$

Residual effects ignoring direct effects (2 df):

$$\frac{20872^2 + 19450^2 + 22314^2}{30} - \frac{62636^2}{90} = 136710.$$

Residual effects eliminating direct effects (2 df):

$$\frac{27332^2 + 27460^2 + 28798^2}{24} - \frac{83590^2}{72} = 54941.$$

As a partial check the sum of the first and last sums of squares above should equal the sum of the second and third sums of squares given above; thus:

$$262875 + 54941 = 317816,$$

and

$$181106 + 136710 = 317816.$$

The error sum of squares is obtained by subtraction; thus, 4001172 − (2299798 + 25182 + 29795 + 1321777 + 317816) = 6804.

The standard error of a treatment mean adjusted for residual effects is $\sqrt{120qE_e/24q} = \sqrt{1.25E_e/6q} = \sqrt{1.25(1701)/6} = 18.8$ pounds of apples per week. For three treatments the $hsd = 18.8(q_{.05,3,4df}) = 18.8(5.00) = 94.0$[1]. Thus, the difference between the means adjusted for residual effects for treatments A and C does not exceed this value, but the differences for treatments B and A and for B and C do exceed this value. Treatment B is considered superior to the other two. The 95 per cent confidence intervals on the various differences of direct effects are

$$\bar{x}_{..B} - \bar{x}_{..A} \pm hsd = 263 \pm 94.0,$$
$$\bar{x}_{..B} - \bar{x}_{..C} \pm hsd = 198 \pm 94.0,$$

and

$$\bar{x}_{..C} - \bar{x}_{..A} \pm hsd = 65 \pm 94.0.$$

[1]The value of $q_{.05,3,4df}$ was obtained from May's table of ranges [208].

XIV-2.4 OTHER DESIGNS

Some additional designs exhibiting symmetrical properties are discussed in the present section. Other designs are possible and will arise with new experimental situations [236].

In the ordinary double change-over design with three treatments the replication on direct effects relative to residual effects is in the ratio of 3 to 2 [198]. Also, the carry-over or residual effects are partially confounded with items. The partial confounding makes the effective replication less than actual. Lucas [198] further points out that the direct and residual effects are positively correlated and that the interest of the experimenter may be in the direct effect plus the residual effect [also, see 200, 235]. The latter quantity estimates the effect of the treatment in the second period if the given treatment had been applied for two consecutive periods instead of for one period. Lucas states that this quantity may "be of more practical importance than either the direct or the carry-over effects *per se*" and that the variance of the "direct plus residual effect is disconcertingly high" in the double change-over design. By the introduction of a single new feature to the double change-over design, Lucas found that the covariance between direct and residual effects can be eliminated and that the variances of direct and residual effects can be made more nearly equal. The additional feature consists of doubling the last period in each square of the double change-over design. The design presented by Lucas is

	Square 1			Square 2		
Period	1	2	3	4	5	6
I	A	B	C	A	B	C
II	B	C	A	C	A	B
III	C	A	B	B	C	A
IV	C	A	B	B	C	A

The doubled period is considered as two periods in the analysis.

A variant of the preceding design is the following:

	Sequence number											
Period	1	2	3	4	5	6	7	8	9	10	11	12
I	A	A	A	A	B	B	B	B	C	C	C	C
II	B	C	B	C	A	C	A	C	A	B	A	B
III	C	B	B	C	C	A	A	C	B	A	A	B
IV	A	A	C	B	B	B	C	A	C	C	B	A

The above design requires twelve like items and four periods of observation for the three treatments. For four treatments, seventy-two like items are re-

quired to obtain the same type of symmetry described above. The eighteen sequences with treatment A applied in period I are

Sequence number

Period	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
I	A	A	A	A	A	A	A	A	A	A	A	A	A	A	A	A	A	A
II	B	C	D	B	C	D	B	B	C	D	C	C	B	D	D	D	B	C
III	C	D	C	D	B	B	B	B	B	B	C	C	C	C	D	D	D	D
IV	D	B	B	C	D	C	C	D	B	B	B	D	C	C	B	C	D	D
V	A	A	A	A	A	A	D	C	D	C	D	B	D	B	C	B	C	B

The other fifty-four sequences may be obtained in the same manner. The above sequences do not contain consecutive applications of a treatment in the first and second periods or in the last and next to the last periods. If this were desired, additional sequences would need to be set up. Since a relatively large number of homogeneous items is required, the design does not appear to be suitable for most experimental situations. Also, the analysis for the above design has not been worked out.

The following variant of a double change-over design was set up to compare three marketing procedures in six stores over seven periods of one week each:

Store number

Period	Group 1			Group 2		
	1	2	3	4	5	6
I	A	B	C	A	B	C
II	B	C	A	C	A	B
III	C	A	B	B	C	A
IV	A	B	C	A	B	C
V	C	A	B	B	C	A
VI	B	C	A	C	A	B
VII	A	B	C	A	B	C

It will be noted that the first three periods form a double change-over design. This is also true for the last three periods and for periods IV, V, and VI Residual effects enter into the fourth and fifth periods but not in the first period. If only three items and four, seven, or ten periods are available, an analysis is still possible. The design is useful for situations in which several periods of observation are available. For example, the market demand for a certain commodity may be relatively stable over a period of four, seven, ten, etc. weeks, and the period of observation might be one week.

The analysis for the above design with three treatments and seven periods

of observation has been developed by M. L. Richards.[1] The various sources of variation and sums of squares in the analysis of variance are given below.

Source of variation	df	ss
Total (corrected for the mean)	41	T
Store (ignoring direct)	5	S
Period	6	P
Direct (ignor. res.; elim. store)	2	D
Residual (eliminating direct)	2	R′
Direct (elim. residual and store)	2	D′
Residual (ignor. direct; elim. period)	2	R
Error	26	by subtraction

In the above analysis

$$T=\sum_{i=1}^{6}\sum_{j=1}^{7} X_{ij h}^2 - X_{...}^2/42, \tag{XIV-30}$$

$$S=\sum_{i=1}^{6}\frac{X_{i..}^2}{7}-\frac{X_{...}^2}{42}, \tag{XIV-31}$$

$$P=\sum_{j=1}^{7}\frac{X_{.j.}^2}{6}-\frac{X_{...}^2}{42}, \tag{XIV-32}$$

$$\begin{aligned} D&=\frac{[7X_{..A}-X_{1..}-X_{4..}-2X_{...}=X_A{}^*]^2+[7X_{..B}-X_{2..}-X_{5..}-2X_{...}=X_B{}^*]^2}{672} \\ &\quad+\frac{[7X_{..C}-X_{3..}-X_{6..}-2X_{...}=X_C{}^*]^2}{672} \\ &=\frac{[(X_A{}^*)^2+(X_B{}^*)^2+(X_C{}^*)^2]}{672}, \end{aligned} \tag{XIV-33}$$

$$\begin{aligned} R'&=\frac{[16(3a+X_{.1.}-X_{...}=a^*)+3X_A{}^*]^2}{21600} \\ &\quad+\frac{[16(3b+X_{.1.}-X_{...}=b^*)+3X_B{}^*]^2+[16(3c+X_{.1.}-X_{...}=c^*)+3X_C{}^*]^2}{21600} \\ &=\frac{[16a^*+3X_A{}^*]^2+[16b^*+3X_B{}^*]^2+[16c^*+3X_C{}^*]^2}{21600}, \end{aligned} \tag{XIV-34}$$

$$D'=\frac{[6X_A{}^*+7a^*]^2+[6X_B{}^*+7b^*]^2+[6X_C{}^*+7c^*]^2}{18900}, \tag{XIV-35}$$

$$R=\frac{(a^*)^2+(b^*)^2+(c^*)^2}{108}, \tag{XIV-36}$$

$X_{i..}$ = store total, $X_{.j.}$ = period total, $X_{..h}$ = treatment total for $h = A, B,$ and C, $X_{...}$ = grand total of all observations, a = sum of yields for treatments receiving A in the previous period, b = sum of yields for treatments receiving B in previous period, c = sum of yields for treatments receiving C in previous period, and the starred quantities are defined in equations (XIV-33) and

[1]North Carolina State College, written correspondence.

(XIV-34). In addition to the above sums of squares the sum of squares for "permanent" effects (direct plus the residual effects) is

$$\frac{[9X_A^* + 23a^*]^2 + [9X_B^* + 23b^*]^2 + [9X_C^* + 23c^*]^2}{59400}, \quad \text{(XIV-37)}$$

with 2 degrees of freedom. As an additional feature in the analysis the treatment by period interaction could be computed to check the consistency of the treatment responses in the experiment. This is done by comparing the treatment totals in the first three periods and in the second three periods. The resulting interaction sum of squares has 2 degrees of freedom. The above sums of squares are a little out of the ordinary. This is occasioned by the fact that there is partial confounding between direct and residual effects, between store and direct effects, and between period and residual effects.

The above designs indicate the diversity of types of designs possible for various situations. The analysis of other designs may be developed along the lines described for the above design [see 236, 242].

XIV-3 Designs For Long-Term Experiments

Certain agronomic experiments are conducted on the same experimental site for a number of years. The design, the analysis, and the interpretation of such experiments are not in an advanced stage. Much work still needs to be done on the above three phases of long-term experiments. However, enough work has been done on such experiments to clarify the procedures for certain types of experiments. Some of these are discussed below. Papers by Cochran [46], Crowther and Cochran [77], Dutton [99], Fisher [123, 124], and Yates [331] represent worthy contributions to the complex subject of long-term experimentation. Other papers have been written on this subject, but the ones listed above contain most of the pertinent information on such experiments. The comprehensive paper by Cochran is particularly enlightening in that he treats the general subject and presents several illustrative examples; the paper by Yates clearly defines the various effects of treatments and the general principles of long-term experimentation.

In the preceding chapters, only the direct effects of the treatments are considered, while both direct and residual effects of treatments are discussed thus far in this chapter. Long-term experimentation results in additional treatment effects, viz. *cumulative* effects and *limiting value of cumulative* effects of treatments. The various treatment effects are defined by Yates [331] as follows (the definitions pertain to the application of a single treatment but may be extended directly to consider differences between treatments):

"The *direct effect* of a treatment may be defined as the response to that treatment in the year of application, or more exactly the difference in the yield where the treatment is applied over that which would be obtained if the treatment were omitted.

"The first-year, second-year, etc. *residual effects* may be defined as the differences in yield in the first, second, etc. years after application, when a treatment is applied in a particular year, over the yield if the treatment were omitted in that year.

"The *cumulative effect* may be defined as the difference in yield in a particular year resulting from repeated application of a treatment or cycle of treatments, over the yield that would result if the treatment were not applied."

The *limiting value of the cumulative effect* may be defined as the stable value to which the cumulative effect tends if the experiment is carried on for a very long time.

Yates further states that none of these definitions is absolute, since the response to a fertilizer treatment may be different on two different types of soils or on two areas which had different treatment previous to the time of application of the fertilizer. However, they are workable definitions in agricultural experimentation. In other areas of experimentation, it may be necessary to develop new concepts and new definitions.

Various types of long-term experiments [46] are presented in figure XIV-1. In this breakdown, Cochran considers the treatments (cultural methods, fertilizers, varieties, time of harvesting, etc.) as different from the various crops (corn, oats, wheat, sugarbeets, swedes, cotton, etc.). Thus far in the present manuscript the term "treatments" refers to the items being tested, but in this section the term is used in the restricted sense described above. The various crops are not considered as treatments.

Treatments			Information supplied on		Crops
Fixed	Applied on the same plots	Every year	Cumulative effects	×	Single crop: annual, perennial
		First year only	Residual effects		Fixed rotation
		At fixed intervals	Direct and residual effects		
Rotating	Applied on different plots in successive years		Direct and residual effects		Effects of different crops

Figure XIV-1. Types of long-term experiments.

Long-term experimentation affords the only evidence on certain biological phenomena and, hence, is a very important type of experimentation. Since such experiments are usually expensive, the number of treatments and crops tested and the size of the experiment are regulated by available resources. Such conditions make it mandatory to use good designs, to use efficient analyses, and to make a complete interpretation of the data. The data from such experiments should be studied at the end of *each year*, and the results should be combined after a few years [46]. Useful knowledge will be provided

from such analyses, and it may be possible to anticipate the final results several years in advance of the conclusion of a complete cycle. If it is found that the wrong selection of treatments has been made after the experiment has been in progress for one or more years, Yates [331] suggests that it may be wise to abandon that experiment and to start a new one.

In designing a long-term experiment a design should be used which permits the inclusion of additional treatments at a later stage, and provision should be made to include every course of the rotation in each period [46, 331].

XIV-3.1 SAME TREATMENT APPLIED ON A SINGLE CROP EVERY YEAR

Certain agronomic experiments on a single crop are conducted over a period of several years. A treatment is applied to the same field plot year after year. Several such plots may be laid out side by side with the different treatments on different plots but with the same crop being used year after year on all plots.

In an attempt to obtain information on the various treatment effects under a system of continuous cropping, Fisher [123, 124] applied the method of orthogonal polynomials to an experiment on wheat and involving various fertilizer treatments. He fitted the following polynomial to a long series of yield data:

$$Y_i = A + BX_i + CX_i^2 + DX_i^3 + EX_i^4 + FX_i^5, \qquad \text{(XIV-38)}$$

where Y_i = yield in year i, the X's are the components of the rainfall curve, and the coefficients of the X's are regression coefficients. Fisher's method has been applied to numerous sets of data by various workers. However, since the equation is of limited usefulness in predicting the limiting value of yield and since the biological explanation of a mechanism which will generate a polynomial does not appear reasonable, some other function of time should be used to explain the yield from a plot in the ith year. For example, the following equation does approach a limiting value for given functions of A, B, and C:

$$Y_i = A + BC^{i-1}. \qquad \text{(XIV-39)}$$

Dutton [99] used this approach to describe the yields from continuous cropping with fixed treatments over a period of several years.

Other approaches (e.g., harmonic analyses) to the problem are possible but require further investigation from both the statistical and the biological viewpoints. After the analyses by the various methods and the interpretation of the results on several sets of data are performed, sufficient empirical evidence should be available to formulate some general principles. Even with present methods of analysis, much knowledge is to be gained from the analysis of data from long-term experiments.

XIV-3.2 EXPERIMENTS ON A SINGLE ROTATION OF CROPS

An established agronomic practice is to rotate various field crops on a given plot. The particular rotation of crops might consist of corn, oats, red clover, red clover with one-fourth of the field planted to corn, one-fourth to oats, and one-half to red clover. A particular area would be planted to corn one year in four, to oats one year in four, and to red clover for the remaining two years. Such a rotation of crops as described above is called a four-course rotation. Two-course, three-course, etc. rotations are in common use in farming areas around the world. One of the courses in a rotation might be a period of no-cropping. Various types of rotation experiments are discussed by Cochran [46], Crowther and Cochran [77], Yates [331], and in the references listed.

Cochran [46] states that the most important rule about rotation experiments is that each crop in the rotation should be grown in each period. This should be accomplished even at the expense of replication on a single crop. Some replication on a crop should be made in order to perform an analysis on each crop for each year. If replication within a year is not possible, then the years may be used as replicates. Also, in order to facilitate cultural operations, all treatments in a replicate on a crop should be laid out in adjoining plots. This is called a *series*. (In a single year a series might be called a whole plot, since the design is comparable to a split plot design with the crops constituting the whole plots and the treatments constituting the split plots within a whole plot.) From a practical standpoint, it is advisable to have the replicates on a crop close together in order to facilitate cultural operations, but from the statistical viewpoint it is recommended that adjacent whole plots or series in a replicate be used. The latter layout usually allows observations over a wider range of conditions, since replicates which are farther apart tend to be more unlike than replicates closer together. Also, more precise comparisons among crops, if desired, are obtained with such a design.

Example XIV-3. A long-term experiment in which the fertilizer treatments were discontinued is described by Cochran [46]. Previously, four treatments—A = organic manure from cotton cake, B = organic manure from corn meal, and C and D = artificial manures with equivalent amounts of nitrogen, potash, and phosphate as found in treatments A and B, respectively—had been applied to four plots on each of four series with one crop of a four-course rotation being grown on each series. The four-course rotation consisted of mangolds, barley, peas, and wheat grown on a plot in that order. The cycle was repeated in the next four years. The field plan for five years is given in table XIV-3.

The object of the analysis of the yield data in table XIV-3 is to examine the yields of barley (grain) to determine the persistence of effects from previous fertilizer treatments. In other words, the residual effects of treatments are compared over three complete cycles of a rotation after discontinuing previous fertilizer treatments, and the rate of deterioration in yields is examined.

The systematic placement of the treatments (see field plan in table XIV-3) invalidates the estimate of the error variance of the average effects. Also, the comparisons

TABLE XIV-3. **Yields of barley grain in lbs per ¼ acre, totals, and plan**

Yield data

Year	Series	Previous treatments A	B	C	D	
First cycle						
1886 = 1	II	207	215	229	210	
1887 = 2	IV	149	156	184	180	
1888 = 3	I	155	154	170	153	
1889 = 4	III	141	148	169	164	
Second cycle						
1890 = 5	II	214	193	201	181	
1891 = 6	IV	128	116	136	155	
1892 = 7	I	142	126	123	130	
1893 = 8	III	131	125	139	108	
Third cycle						
1894 = 9	II	167	180	150	137	
1895 = 10	IV	76	98	102	104	
1896 = 11	I	82	80	68	72	
1897 = 12	III	92	93	101	85	
Total treatments		1684	1684	1772	1679	6819
Total for series	II	588	588	580	528	2284
	IV	353	370	422	439	1584
	I	379	360	361	355	1455
	III	364	366	409	357	1496

Field plan of crops in various years

Year	Series I Treatments D C B A	Series II Treatments D C B A	Series III Treatments D C B A	Series IV Treatments D C B A
1886	Wheat	Barley	Peas	Mangolds
1887	Mangolds	Peas	Wheat	Barley
1888	Barley	Wheat	Mangolds	Peas
1889	Peas	Mangolds	Barley	Wheat
1890	Wheat	Barley	Peas	Mangolds

etc.

Linear trend = differences between yields on same plots in first and third cycles (pounds per ¼ acre) × (−1) = decrease in yield

Series	Previous treatment A	B	C	D	Total
II	40	35	79	73	227
IV	73	58	82	76	289
I	73	74	102	81	330
III	49	55	68	79	251
Total	235	222	331	309	1097

Quadratic component of yields on same plot = 2(second cycle yield) − (first + third cycle yields)

Series	Previous treatment A	B	C	D	Total
II	54	−9	23	15	83
IV	31	−22	−14	26	21
I	47	18	8	35	108
III	29	9	8	−33	13
Total	161	−4	25	43	225

among series are partially confounded with comparisons among years. Nevertheless, the data are used to illustrate the analysis of a long-term experiment.

The totals directly under the original yield data are merely the sums of yields for the three yields of barley grain in the years that barley occupied that particular plot; for example, $588 = 207 + 214 + 167$. The other totals in this table are obtained similarly. The first analysis of variance in table XIV-4 is for these data. The third set of figures in table XIV-3 represents the decrease in yield between the first and third years; for example, on the plot previously receiving treatment A in series II the decrease in barley grain yields per $\frac{1}{4}$ acre between the years 1886 and 1894 is $207 - 167 = 40$ pounds of grain per $\frac{1}{4}$ acre. There is a downward trend in yields after stopping the previous fertilizer treatment. The analysis of variance on these data is presented in the second part of table XIV-4. The divisor for the individual squares is 2, since two original yields make up each difference. The last set of differences represents the curvilinear (quadratic) effect of the decrease. This is estimated by 2 (yield from the second cycle) − (yield in the first and third cycles); for example, the first difference is equal to $54 = 2(214) - (207 + 167)$. The other differences are obtained similarly. The analysis of variance on these differences is presented in the last part of table XIV-4. The divisor for the squares of each of these differences is equal to $(2^2 + (-1)^2 + (-1)^2) = 6$. The analyses on quadratic and linear regression on years follows the usual form described in standard textbooks on statistical methodology [273]. Also, the standard errors of the individual figures or means in table XIV-3 are computed in the usual manner.

Owing to the nature of the design the estimated error for average treatment effects is to be questioned. However, if the error variance is considered as an estimate of the true error variance, there is no indication of real differences among the average yields. The totals for treatments A, B, and D seem surprisingly close together.

Inspection of the linear trends indicates that the yields following inorganic manures deteriorated faster than those following organic manures. An examination of the quadratic regressions indicates that yields following treatments A and C were particularly well maintained during the second cycle of the experiment (1890 to 1893).

Cochran [46] discusses a more accurate estimate of the deterioration in yields than the one given immediately above. Let (1), (2), $\cdots$, (12) denote the yields from a plot receiving a specified treatment in the first, second, $\cdots$, twelfth years. If the plot errors in the twelve years are independent and have equal variances, σ_e^2, the best estimate of the average decrease in yield per year is given by

$$L = \tfrac{1}{286}[11(12) + 9(11) + 7(10) + \cdots - 7(3) - 9(2) - 11(1)],$$

with a variance equal to $\sigma_e^2/143$. Likewise, the above equation may be rewritten in the following form:

$$L = \tfrac{1}{286}[(3d_0 + c_0 - b_0 - 3a_0) + 8(a_1 + b_1 + c_1 + d_1)],$$

where the following relationships hold:

Totals	Linear components
$a_0 = (1) + (5) + (9)$	$a_1 = (9) - (1)$
$b_0 = (2) + (6) + (10)$	$b_1 = (10) - (2)$
$c_0 = (3) + (7) + (11)$	$c_1 = (11) - (3)$
$d_0 = (4) + (8) + (12)$	$d_1 = (12) - (4)$

TABLE XIV-4. Analyses of variance (unit = ¼ acre per year)

Average effects of treatments

Source of variation	df	ss	ms
Series	3	38005.2	-
Treatment	3	503.9	168.0
Error	9	2685.0	298.3
Total	15	41194.1	-

Linear regression on years

Source of variation	df	ss	ms
Series	3	762.4	-
Treatment	3	1087.4	362.5
Error	9	368.2	40.9
Total	15	2218.0	-

Quadratic regression on years

Source of variation	df	ss	ms
Series	3	271.1	-
Treatment	3	656.4	218.8
Error	9	539.3	59.9
Total	15	1466.8	-

The quantity $L' = (a_1 + b_1 + c_1 + d_1)/32$ estimates the decrease in yield per year and a_1, b_1, c_1, and d_1 are the quantities used in the analysis of variance for linear regression on years. The variance of L' is equal to $\dfrac{\sigma_e^2(1+1+1+1+1+1+1+1)}{32 \times 32}$ $= \sigma_e^2/128$, given that the plot errors are independent. Therefore, the fractional loss in information due to using L' instead of L is $(143 - 128)/143 = 15/143 = .105$, or a loss of about 10 per cent. Since the two quantities L' and $L'' = (3d_0 + c_0 - b_0 - 3a_0)/30$ are subject to different variances the loss is somewhat less than 10 per cent. In fact, Cochran states that little additional information is to be obtained in this experiment if L is used instead of L'. He presents a similar discussion for the quadratic regression on years.

XIV-3.3 COMPARISON OF SEVERAL ROTATIONS

There are many types of rotations that are in agronomic usage. The comparison of the direct and residual effects from various rotations may be evaluated in various ways. For example, uniform or continuous cropping could be followed for a time [331]; then, the various rotations could be started and

conducted through one or more complete cycles, after which continuous cropping of the test crop could be followed to evaluate the residual effects of the various rotations. Yates [331] and Cochran [46] discuss this and other examples. The reader is referred to these excellent papers for a clear and concise discussion of comparisons among rotation experiments and of the conduct of long-term experiments.

The field layout for the comparison of two three-course rotations is discussed below. Since the year-to-year variation is usually considerably larger than the plot-to-plot variation within a year, it is desirable to grow every crop in the rotation each year. Thus, if the two rotations consist of C = corn, O = oats, S = soybeans and C = corn, R_1 = first-year red clover, R_2 = second-year red clover, the following field plan for a single replicate might be used:

Plot number

Year	1	2	3	4	5	6
1	C	O	S	C	R_1	R_2
2	O	S	C	R_1	R_2	C
3	S	C	O	R_2	C	R_1
4	C	O	S	C	R_1	R_2

etc.

The cycle is completed in three years. The crops on a given plot in the fourth year are the same as in the first year.

A comparison of a two-course and a three-course rotation would require multiples of five plots and six years to complete a cycle. Thus, for two rotations, C = corn, O = oats and C = corn, O = oats, S = soybeans, one replicate of the field plan might be represented as follows:

Plot number

Year	1	2	3	4	5
1	C	O	S	C	O
2	O	S	C	O	C
3	S	C	O	C	O
4	C	O	S	O	C
5	O	S	C	C	O
6	S	C	O	O	C
7	C	O	S	C	O

etc.

It is recommended that more than one replicate and more than one location be used for rotations of the above type. The sequences should be randomly allotted to the plots. Also, additional information on cultural operations, fertilizer treatments, etc. may be obtained by splitting the plots and applying the various treatments.

CHAPTER XV

Some Additional Designs

A few additional designs are included in the present chapter. These designs plus those in the preceding chapters include most of the experimental designs encountered in experimental work. Although the designs discussed below are seldom used in experimental work, they are included for completeness, for the infrequent occasions when they are encountered, and for use in understanding the design and analysis of other designs. A more complete account of the designs discussed herein may be found in the references cited.

XV-1 Three-Way and Higher-Way Grouping of the Treatments

In the randomized complete block design, only a single grouping of the treatments is used. In the latin square design, two-way grouping of the treatments is utilized. Further stratification of the set of treatments is possible by grouping the treatments in other ways. The purpose of additional stratification is to control sources of variation that cannot be controlled by less stratification.

In making use of the designs in this section, caution should be used in selecting the experimental material and in using experimental material subject to several major sources of variation. If the treatments interact with one of the sources of variation (say, rows or columns), the tests of significance for treatment differences may be invalidated because of the confounding of effects. Also, if there is an interaction between the types of stratification, tests of significance and estimates of effects are invalidated because of the confounding of effects.

Fisher [126, sec. 35.1] states that the principal use of graeco-latin and higher squares consists in clarifying complex combinatorial situations, but that they are occasionally useful directly in experimental work [98, 126, 205, 289, 324]. Likewise, the main use of latin cubes, graeco-latin cubes, and hyper-graeco-latin cubes [126, 184, 186] is in the clarification of concepts associated with the more complex incomplete block designs and with complex combinatorial situations.

XV-1.1 GRAECO-LATIN SQUARES

In an ordinary latin square design, each treatment occurs once in each row and once in each column. The treatments are usually designated by Latin letters, A, B, C, etc. A second set of variates, say Greek letters, α, β, γ, etc. may be superimposed on the Latin letters (treatments) in the latin square in such a manner that the Greek letters appear once in each row, once in each column, and once with each Latin letter (treatment). For example, the only 3×3 graeco-latin square possible is [126, sec. 35]

$A\alpha$	$B\beta$	$C\gamma$
$B\gamma$	$C\alpha$	$A\beta$
$C\beta$	$A\gamma$	$B\alpha$

The partitioning of the 8 degrees of freedom for the nine cells follows:

Source of variation	df
Row	2
Column	2
Latin letter	2
Greek letter	2

In order to observe the relationship of the above to confounding in factorial experiments, let $A = 0$, $B = 1$, $C = 2$, $\alpha = 0$, $\beta = 1$, $\gamma = 2$; then, the graeco-latin square becomes

00	11	22
12	20	01
21	02	10

The partitioning of the 8 degrees of freedom for the two pseudo-factors a and b is

Source of variation	df
Row = comparison among $(AB^2)_0$, $(AB^2)_1$, and $(AB^2)_2 = AB^2$	2
Column = " " $(AB)_0$, $(AB)_1$, and $(AB)_2 = AB$	2
Latin letter = comp. among $(A)_0$, $(A)_1$, and $(A)_2 = A$	2
Greek letter = " " $(B)_0$, $(B)_1$, and $(B)_2 = B$	2

Thus, the AB^2 effect is completely confounded with row differences and the AB effect with column differences in the above graeco-latin square. The A effect is the Latin letter effect, and the B effect is the Greek letter effect.

Nothing is left unspecified in the 3 × 3 graeco-latin square; this results in zero degrees of freedom and zero sum of squares for residuals. The above illustrates the orthogonality[1] of the two latin squares involved in the 3 × 3 graeco-latin square and the analogy between the separate components of variation in the graeco-latin square and in the factorial experiment.

Since there is no error term for a 3 × 3 graeco-latin square, it is necessary to use sets of 3 × 3 graeco-latin squares and to use the square by treatment interaction as the error for treatments. The analysis of variance for *s* 3 × 3 graeco-latin squares is

Source of variation	df
Square	$s-1$
Column within square	$2s$
Row " "	$2s$
Greek letter within square	$2s$
Latin " (treatment)	2
Treatment × square	$2(s-1)$
Total	$9s-1$

Though there is only one 3 × 3 graeco-latin square, there are seventy-two possible arrangements. The twelve possible arrangements for a 3 × 3 latin square (Chapter VI) and the three Greek letters may be permuted among themselves in six ways, resulting in seventy-two arrangements. In using the 3 × 3 graeco-latin square, one of the seventy-two arrangements should be selected at random. An alternative method is to select at random one of the twelve arrangements of the 3 × 3 latin squares and then assign the Greek letters to the variates at random.

As stated in Chapter VI, there are four standard 4 × 4 latin squares and a total of 576 arrangements [126]. Of the four standard squares, only one,

A	B	C	D
B	A	D	C
C	D	A	B
D	C	B	A

[1]At this point the concept of *orthogonal latin squares* needs to be defined. (The term has been used in previous chapters but was not defined.) Fisher and Yates [129] give the following definition of orthogonal latin squares and their relationship to graeco-latin squares: "Two latin squares are orthogonal to each other if, when they are superimposed, every letter of one square occurs once and only once with every letter of the other. Such a pair of squares (one square being written with Greek letters and the other with Latin letters) form a graeco-latin square."

yields a graeco-latin square [126, sec. 35]. There are two 4 × 4 graeco-latin squares from among the three orthogonal 4 × 4 latin squares [129]; thus:

Aα	Bβ	Cγ	Dδ
Bδ	Aγ	Dβ	Cα
Cβ	Dα	Aδ	Bγ
Dγ	Cδ	Bα	Aβ

Aα	Bβ	Cγ	Dδ
Bγ	Aδ	Dα	Cβ
Cδ	Dγ	Aβ	Bα
Dβ	Cα	Bδ	Aγ

There are 144 = 4!3! arrangements for a 4 × 4 standard latin square, twenty-four ways of permuting the Greek letters among themselves, and two orthogonal graeco-latin squares, resulting in a total of 6912 = 144 × 2 × 24 arrangements. In laying out a 4 × 4 graeco-latin square, select one of the 144 arrangements of the latin square at random. Then, select one of the two types of graeco-latin squares at random, and assign the Greek letters to the variates at random. This process assigns equal probability of selection to each arrangement. The analysis of variance breakdown of the degrees of freedom for the 4 × 4 graeco-latin square design is:

Source of variation	df
Row	3
Column	3
Greek letter	3
Treatment (Latin letter)	3
Residual	3
Total	15

Of the fifty-six standard squares [129] for the 5 × 5 latin square, only six standard squares yield graeco-latin squares; each of the six yields three different squares which differ in ways other than a randomization of the Greek letters [126, sec. 35]. There are $3 \times 6 \times 24 \times 120^2$ 5 × 5 graeco-latin square arrangements. The randomization procedure follows that for the 4 × 4 graeco-latin square.

By enumerating all the actual types of squares, Fisher and Yates [128] have established the fact that no 6 × 6 graeco-latin square exists. They [129] list the two 3 × 3 orthogonal latin squares, the three 4 × 4 orthogonal latin squares, the four 5 × 5 orthogonal latin squares, the six 7 × 7 orthogonal latin squares, the seven 8 × 8 orthogonal latin squares, and the eight 9 × 9 orthog-

onal latin squares. Graeco-latin squares have been constructed for all numbers of treatments from three to thirteen with the exception of six and ten; Cochran and Cox [60] include plans for the 3×3, 4×4, 5×5, 7×7, 8×8, 9×9, 11×11, and 12×12 graeco-latin squares in their book. Graeco-latin squares exist for all odd numbers. It is suspected from the nature of the modulo notation that no 14×14 graeco-latin square is possible, since 14 is composed of mixed primes (i.e., $2 \times 7 = 14$) in the same manner as are the numbers $2 \times 3 = 6$ and $2 \times 5 = 10$.

XV-1.2 HYPER-GRAECO-LATIN SQUARES

More restrictions are placed on the grouping of the treatments (Latin letters) in the hyper-graeco-latin square than in the graeco-latin square design. In the latter design each Latin letter appears once with each Greek letter. In the hyper-graeco-latin square design suffices (more alphabets [126]) are added and each of the letters appears once with each of the numbers. If no interactions among the treatments and sources of variation exist, the hyper-graeco-latin square design may be used to control the additional sources of variation.

Fisher [126] forms the following 4×4 hyper-graeco-latin square by letting one of the sets of Greek letters in the two 4×4 graeco-latin squares be numbers or suffices 1, 2, 3, and 4:

A1α	B2β	C3γ	D4δ
B4γ	A3δ	D2α	C1β
C2δ	D1γ	A4β	B3α
D3β	C4α	B1δ	A2γ

Again, it is possible to set up the analogy between the hyper-graeco-latin square and confounding in factorial experiments. The sixteen cells may be considered as the combinations obtained from four pseudo-factors each at two levels, or a 2^4 factorial arrangement, with the following scheme of association between the factorial arrangement and the 4×4 hyper-graeco-latin square:

Effects in 2^4 factorial	Item of hyper-graeco-latin square	Degrees of freedom
A, BC, ABC	Row	3
B, AD, ABD	Column	3
C, BD, BCD	Latin letter	3
D, AC, ACD	Number	3
AB, CD, ABCD	Greek letter	3

Among the four row totals, there are 3 degrees of freedom which correspond to the 3 degrees of freedom associated with A, BC, and ABC; analogous relations exist between the other sources of variation. In laying out the 4×4 hyper-graeco-latin square, one of the $2 \times 144 \times 24 \times 24$ possible arrangements is selected at random. Alternatively, one of the graeco-latin squares may be selected at random, and then the numbers are randomly assigned. Since there is no residual variation in the 4×4 hyper-graeco-latin square, it is necessary to use more than one square and to use the square by treatment interaction as the error variance to test treatment differences. The breakdown of the total degrees of freedom follows that outlined for sets of 3×3 graeco-latin squares.

The 5×5 hyper-graeco-latin square design of Greek letters and one suffix may be likened to the Knut Vik square [126, p. 76], where each treatment (Latin letter) appears once in a row, once in a column, once in a diagonal in one direction, and once in a diagonal in the opposite direction; or it may be likened to a 5^2 factorial with the following association among the effects and the Latin letters, the Greek letters, and the numbers:

Effect of 5^2 factorial	Item in hyper-graeco-latin square	Degrees of freedom
A	Latin letter	4
B	Greek letter = first diagonal	4
AB	Row	4
AB^2	Column	4
AB^3	Number = second diagonal	4
AB^4	Residual	4

Other associations among the effects and the categories of the hyper-graeco-latin square may be substituted for the one above, since the effects are pseudo-effects. The association presented above illustrates the relationship between the factorial experiment and the hyper-graeco-latin square. If a second set of numbers (or suffices) is used, it corresponds to the AB^4 effect. Thus, all items in the 5×5 latin square are specified; there are zero degrees of freedom and zero sum of squares for the residual, necessitating the use of more than one square in order to obtain an error mean square for comparing treatment differences.

Nissen [232] has shown that there are two 5×5 Knut Vik squares for a given assignment of the Latin letters to the treatments. If the Latin letters are randomly allotted to the treatments, there are 2(120) arrangements for the Knut Vik square. A random selection of one of the arrangements is made.

There are a total of $6 \times 6 \times 24 \times 120^2$ 5×5 hyper-graeco-latin squares with Latin letters, Greek letters, and numbers; there are $6 \times 6 \times 24 \times 120^4$ different hyper-graeco-latin squares for Latin letters, Greek letters, first-number suffix, and second-number suffix. In setting up an experimental design, one of the possible arrangements is randomly selected.

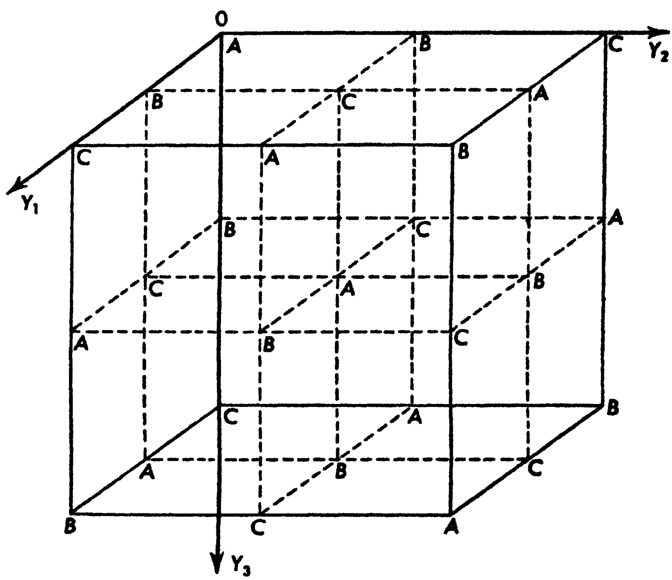
In all graeco-latin squares and hyper-graeco-latin squares, randomizations may be obtained by randomizing all rows, all columns, all Latin letters, all Greek letters, etc. of a given square. For larger squares, some of the arrangements will be excluded by this method, but the number of arrangements possible will probably be large enough unless these designs are used extensively and unless it is desired to summarize the results from all such designs.

Graeco-latin square and hyper-graeco-latin square designs may be particularly effective in controlling and locating sources of variation in various fields.

XV-1.3 LATIN AND HYPER-GRAECO-LATIN CUBES AND HYPERCUBES

Although the latin squares and hyper-graeco-latin squares (including the graeco-latin squares) were first studied in the latter part of the eighteenth

Diagrammatic representation



Schematic representation

I			II			III		
A	B	C	B	C	A	C	A	B
B	C	A	C	A	B	A	B	C
C	A	B	A	B	C	B	C	A

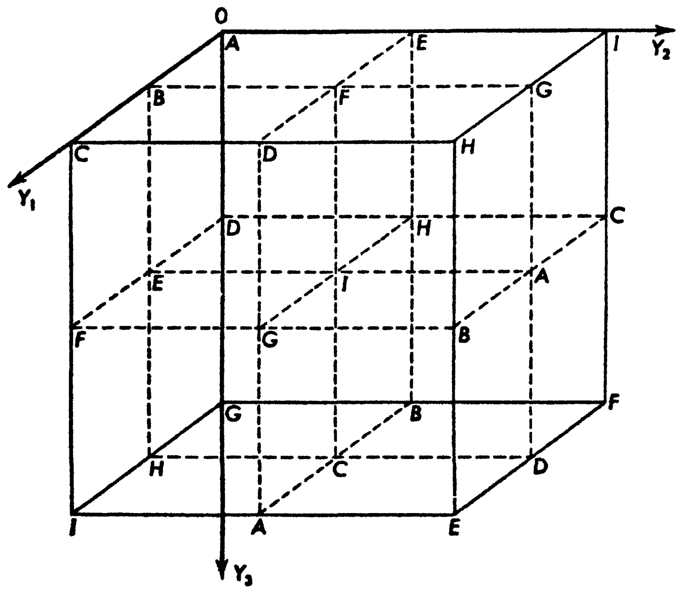
Figure XV-1. $3 \times 3 \times 3$ latin cube of first order.

century by Professor L. Euler, a famous German mathematician, the concept of latin and hyper-graeco-latin cubes and hypercubes is quite new. Kishen [184, 186] first introduced these designs in 1942. In 1945, Fisher [126] inde-

pendently published on the completely orthogonalized hyper-graeco-latin cubes and hypercubes of first order; he was investigating systems of confounding in the general symmetrical factorial design at the time. Shortly after this (1946-47), Rao defined certain combinatorial arrangements, which he called hypercubes of strength *d*, and showed that for $d = 2$ the hypercubes are exactly equivalent to latin and hyper-graeco-latin cubes and hypercubes of the first order [186].

The natural extension of the latin square and the hyper-graeco-latin square is to a latin cube and to a hyper-graeco-latin cube. The extension from

Diagrammatic representation



Schematic representation

I			II			III		
A	E	I	D	H	C	G	B	F
B	F	G	E	I	A	H	C	D
C	D	H	F	G	B	I	A	E

Figure XV-2. $3 \times 3 \times 3$ latin cube of second order.

cubes is to higher dimensions, or to the hypercubes. Instead of considering k^2 elements as in a latin square, k^3 elements are considered. There are k layers of k^2 elements with the three sets of such layers intersecting at right angles. Thus, any two layers of the different sets will intersect in k common elements (figure XV-1). Kishen [186] defines a latin cube of first order of edge k as a

cube arrangement of k letters, each repeated k^2 times, such that each letter occurs exactly k times in each of the three sets of planes parallel to the three coordinate planes OY_2Y_3, OY_1Y_3, and OY_1Y_2. The diagrammatic and schematic representations of this are given in figure XV-1. The schematic form of presenting the $3 \times 3 \times 3$ latin cube of first order is more convenient and less cumbersome than the former. The analogy between the rows (or columns) in the squares and the sides of the cube is evident from an examination of figure XV-1.

A latin cube of second order of side k is defined to be a cube arrangement of k^2 letters, each repeated k times, such that each letter occurs exactly once in each of its three sets of planes parallel to the coordinate planes OY_1Y_2, OY_1Y_3, and OY_2Y_3. Thus, in a latin cube of second order, there are k^2 letters instead of k letters as there are in a latin cube of first order. The diagrammatic and schematic arrangements for a $3 \times 3 \times 3$ latin cube of second order are given in figure XV-2.

A further discussion of rth order latin cubes and latin hypercubes and of their use in the theory of general symmetrical factorial arrangements and in balanced incomplete block designs may be found in Kishen's paper [186]. This reference plus the others listed in this section discuss hyper-graeco-latin cubes and hypercubes as well as orthogonality among the cubes [see 113].

XV-2 Quasi-Latin Squares

XV-2.1 CONFOUNDING IN LATIN SQUARES

The subject of confounding in the randomized complete block design has been discussed in previous chapters. Only brief mention of confounding in the latin square design has been made thus far (sections IX-3.2, X-1.4, and XIII-3; problems X-7 and X-9). This subject is important in experimental design because it may be necessary to use two-way elimination of experimental heterogeneity and to use more treatments than there are rows and columns in the given latin square. (In some experimental situations the lattice square design may be appropriately used.) For certain treatment combinations, it is possible to arrange the treatments in a latin square design in such a way that the less important interactions are confounded with row and column differences. Since these designs resemble ordinary latin squares in that row and column differences are eliminated from the experimental error and since they differ from latin squares in that each treatment does not appear once in a row and once in a column, Yates [324] proposed the name *quasi-latin squares* for these designs. He constructed a number of the designs and presented appropriate analyses. In laying out the designs, *all rows and all columns must be rearranged at random in order to obtain an unbiased estimate of the error variance.* This is true for all quasi-latin squares including the half-plaid and plaid latin squares discussed in the following sections.

Prior to selecting a particular quasi-latin square design the experimenter should note which effects are confounded with row differences. Also, an outline of the procedure for computing the sums of squares in the analysis of variance should be made prior to conducting the experiment. The latter information points up the difficulties associated with the designs, the effects confounded, etc. The procedure for computing the various sums of squares may be obtained from the results presented in Chapters VI through IX. Plans for the various quasi-latin squares and key-outs for the degrees of freedom in the analysis of variance are tabulated in various places [see 60, 140, 175, 253, 324].

The design for and the analysis of a 2^3 factorial in two 4 × 4 latin squares with the columns of the two squares interlaced is discussed in section IX-3.2. The three-factor interaction is completely confounded with square differences. Various alternative schemes of confounding the effects from a 2^3 factorial in 4×4 latin squares may be used. For example, the following plan of partially confounding the interactions with row and column differences might be utilized:

	Square I					Square II			
	$(BC)_0$	$(BC)_1$	$(AB)_0$	$(AB)_1$		$(ABC)_0$	$(ABC)_1$	$(AC)_0$	$(AC)_1$
$(ABC)_0$	011	110	000	101	$(BC)_0$	011	111	000	100
$(ABC)_1$	111	010	001	100	$(BC)_1$	110	010	101	001
$(AC)_0$	000	101	111	010	$(AB)_0$	000	001	111	110
$(AC)_1$	100	001	110	011	$(AB)_1$	101	100	010	011

The key-out for the degrees of freedom in the above design is

Source of variation	df
Square	1
Rows within square	6
Columns " "	6
Main effects (A, B, C)	3
AB′	1
AC′	1
BC′	1
ABC′	1
Residual variance	11
Total	31

All interactions are partially confounded with row and column differences. Only one-half of the relative information is retained on the partially confounded effects. Consequently, unless the interactions are of little importance or unless the added stratification controls a considerable amount of the residual

variation, it may be well to use an incomplete block design with plots of four treatments; if each interaction is confounded with incomplete block differences in one of the four replicates, three-fourths of the relative information is retained on each interaction (section IX-4.1).

If three 4 × 4 latin squares were to be used with 2^3 treatments, the following additional square could be added to the above two:

	Square III			
	$(A)_0$	$(A)_1$	$(ABC)_0$	$(ABC)_1$
$(B)_0$	001	101	000	100
$(B)_1$	010	110	011	111
$(C)_0$	000	100	110	010
$(C)_1$	011	111	101	001

The above design allows for five-sixths relative information on the main effects, two-thirds relative information on the two-factor interactions, and one-half information on the three-factor interaction. The key-out of the degrees of freedom in the above design is

Source of variation	df
Square	2
Rows within square	9
Columns within square	9
Main effects (A′, B′, C′)	3
Two-factor interactions (AB′, AC′, BC′)	3
Three-factor interaction (ABC′)	1
Residual	20
Total	47

Other schemes of confounding could be used. The three-factor interaction could be completely confounded with rows in all three squares, *A* and *BC* with columns in square I, *B* and *AC* with columns in square II, and *C* and *AB* with columns in square III. Also, the three-factor interaction may be completely confounded with rows in all three squares and one of the two-factor interactions may be completely confounded with columns in one of the squares. The former design allows for no information (i.e., intrablock) on the *ABC* effect and for five-sixths information on the other effects. The latter design allows for full information on the main effects and for two-thirds relative information on the two-factor interactions.

Similar schemes of confounding in latin squares with 3^n, 4^n, 5^n, etc. treatments are possible. The reader is referred to Yates [324] for a more comprehensive discussion of confounding in latin squares.

XV-2.2 HALF-PLAID LATIN SQUARES

The half-plaid latin square is a quasi-latin square in which a main effect, instead of an interaction, is confounded with row differences. The levels of the factor, which are applied to the rows of the latin squares, should be allotted at random. No grouping of the treatments is permissible if an unbiased estimate of the error variance is to be obtained. Thus, the design for the treatments applied to entire rows in the latin square corresponds to a completely randomized design. The additional degrees of freedom in the error variance for the treatments applied to the rows somewhat compensate for the inefficient design [324].

The following example is used to illustrate the design and analysis for the half-plaid latin square. Suppose that four levels (v_1, v_2, v_3, and v_4) of the factor v, or four varieties, are applied to the rows of an 8×8 latin square. The varieties are randomly allotted to the eight rows of the square. The $2^3 = 8$ treatments within the 8×8 latin square are randomly allotted in the manner described in Chapter VI. The following field arrangement represents one of the possible layouts for a $4 \times (2^3)$ half-plaid latin square (the notation $p \times (k)$ is used to denote the fact that the p levels of a factor are applied to the whole rows and the $k = 2^3$ treatments are the treatments within the $k \times k$ latin square):

v_2	001	010	110	101	111	100	000	011
v_1	111	100	000	011	001	010	110	101
v_4	010	001	101	110	100	111	011	000
v_2	110	101	001	010	000	011	111	100
v_3	011	000	100	111	101	110	010	001
v_1	000	011	111	100	110	101	001	010
v_3	100	111	011	000	010	001	101	110
v_4	101	110	010	001	011	000	100	111

If E is set equal to the comparison $-v_1 - v_2 + v_3 + v_4$, if D is set equal to the comparison $-v_1 + v_2 - v_3 + v_4$, if DE is set equal to the comparison $v_1 - v_2 - v_3 + v_4$, and if the levels of the three factors a, b, c correspond to the first, second, and third numbers, respectively, the ABE, BCD, and $ACDE$ effects

are confounded with column differences. The key-out for the degrees of freedom in the analysis of variance is

Source of variation	df			ms
Row	7			
Variety		3		
Residual		4		E_v
Column	7			
Confounded effects (ABE, BCD, ACDE)		3		
Residual		4		E_c
Sub-plots	49			
Treatment		7		
A			1	
B			1	
AB			1	
C			1	
AC			1	
BC			1	
ABC			1	
Variety × treatment		18		
Variety × A			3	
Variety × B			3	
Variety × C			3	
Variety × AB′			2	
Variety × AC′			2	
Variety × BC′			2	
Variety × ABC			3	
Error		24		E_e
Total	63			

The error mean square E_e may be used to test the treatment and the treatment × variety effects if only these varieties and levels of a, b, and c are concerned in making inferential statements about the treatments. Likewise, E_c and E_v represent the error mean squares for comparison with the mean squares for confounded effects (ABE, BCD, and $ACDE$) and for varieties, respectively.

If two such squares are used with different randomizations, the following breakdown of the degrees of freedom in the analysis of variance is made:

Source of variation	df		ms
Square	1		
Rows within square	14		
Variety		3	
Variety × square		3 }	
Residual		8 }	E_v
Columns within square	14		
Effects confounded with columns		3	
Effect × square		3 }	
Residual		8 }	E_c
Sub-plots within squares	98		
Treatment		7	
Treatment × square		7	
Treatment × variety		18′	
Treatment × square × variety		18′	
Residual within square		48	E_e
Total	127		

The above designs are useful in applying additional factors to a latin square design that has already been laid out in the field or laboratory. For example,[1] an 8×8 latin square design on eight cabbage varieties might be laid out. Later in the season, information might be desired on four fertilizers (or four insect sprays). With proper precautions in applying the fertilizers to specific rows in the latin square, additional information is obtained on the effectiveness of fertilizers as well as on the relative yields of the eight cabbage varieties.

XV-2.3 PLAID LATIN SQUARES

If one set of factors is applied to the rows and a second set of factors is applied to the columns of a latin square design, the resulting quasi-latin square is called a *plaid latin square.* Yates [324] used this term because the resulting square after randomization resembles a "typical Scotch plaid." The relationship of the half-plaid latin square to the plaid latin square is evident and is the reason for the name, half-plaid.

The notation for the plaid latin square represents an extension of that for the half-plaid latin square. For example, a $2 \times 2 \times (2^2)$ plaid latin square is one with two levels of one factor, say a, applied to the columns, two levels of the second factor, say b, applied to the rows, and the 2^2 treatments, say two levels each of the factors c and d in all combinations, arranged in a 4×4 latin square; the schematic arrangement is

	a_0		a_1	
b_0	00	01	10	11
	11	10	00	01
b_1	10	11	01	00
	01	00	11	10

or using the complete notation the schematic arrangement is

	$(A)_0$		$(A)_1$	
$(B)_0$	0000	0001	1010	1011
	0011	0010	1000	1001
$(B)_1$	0110	0111	1101	1100
	0101	0100	1111	1110

[1]M. T. Vittum, Geneva Experiment Station, N. Y., used such a design.

The effects confounded with the various columns and rows are (indicated by the symbol x) given below.

Effect	Row number 1	2	3	4	Column number 1	2	3	4	df confounded
A					x	x	x	x	1
B	x	x	x	x					1
AC	x	x	x	x					1
BD							x	x	½
ABC	x	x	x	x					1
ABD							x	x	½
BCD					x	x			½
ABCD					x	x			½
Total df confounded									6

Thus, effects B, AC, and ABC are completely confounded with row differences and A with column differences. Effects BD, ABD, BCD, and $ABCD$ are partially confounded with column differences with one-half relative information on each. The degrees of freedom confounded add up to the 6 degrees of freedom for rows and columns. In order to obtain an error variance for some of the effects, it is necessary to use more than one 4×4 square. The reader is referred to the paper by Yates [324] for a more comprehensive treatment of these designs.

XV-3 Magic Latin Squares and Super Magic Latin Squares[1]

In certain instances it may be desirable to place additional restrictions on the grouping of the treatments within a latin square design. For example, in a $k \times k$ latin square, the k treatments might be grouped into $r \times s = k$ smaller squares or rectangles. Also, the grouping might be in more than one direction.

The 6×6 magic latin square listed below illustrates the arrangement of treatments.[2]

A	B	C	D	E	F
D	E	F	A	B	C
C	A	B	E	F	D
F	D	E	C	A	B
B	C	A	F	D	E
E	F	D	B	C	A

[1]The analyses for these designs were developed by G. M. Cox, Univ. of North Carolina, unpublished results.

[2]This square has been used by R. J. Borden, Hawaiian Sugar Planters' Assoc., and by M. T. Vittum, Geneva Experiment Station, in field experimentation.

In the above square, each treatment appears once in a row and once in a column, and all treatments appear together in a given row or a given column. Thus far the plan is a typical latin square arrangement. In addition, all six treatments appear within each 3 × 2 rectangle marked by the dotted lines. This further restriction on the allocation of treatments must be taken into consideration when making an analysis.

The randomization procedure is to assign the treatments to the letters at random, to assign the sets of three columns to the left part and to the right part of the experimental area at random, to randomize the three columns within each set, to randomize the sets of two rows, and then to randomize within the sets of rows. All treatments then fall together in a row, in a column, and in a 3 × 2 rectangle.

The breakdown of the total degrees of freedom in the analysis of variance for a 6 × 6 magic latin square is

Source of variation	df
Row	5
Column	5
Treatment	5
Rectangle (elim. row and col. effects)	2
Error	18
Total	35

The total, row, column, and treatment sums of squares are computed in the same manner as previously described for the latin square design. The remaining sum of squares with 20 degrees of freedom is partitioned into two parts, one with 2 degrees of freedom representing the sum of squares among rectangles (eliminating row and column effects) and the other with 18 degrees of freedom representing the sum of squares for the experimental error.

An examination of the following design is helpful in formulating the analysis of variance for the design:

Column number

Row no.	1 2 3	4 5 6
1	Rectangle 1	Rectangle 2
2	Total = $X_{\ldots 1}$	Total = $X_{\ldots 2}$
3	Rectangle 3	Rectangle 4
4	Total = $X_{\ldots 3}$	Total = $X_{\ldots 4}$
5	Rectangle 5	Rectangle 6
6	Total = $X_{\ldots 5}$	Total = $X_{\ldots 6}$

The sum of squares among the totals, $X_{\ldots g}$ (let X_{ijkg} = yield of a single observation), for the six rectangles may be partitioned into the following five orthogonal contrasts:

$$R_1 = (X_{\ldots 1} + X_{\ldots 3} + X_{\ldots 5}) - (X_{\ldots 2} + X_{\ldots 4} + X_{\ldots 6}), \qquad \text{(XV-1)}$$

$$R_2 = (X_{\ldots 1} + X_{\ldots 2}) - (X_{\ldots 3} + X_{\ldots 4}), \qquad \text{(XV-2)}$$

$$R_3 = (X_{\ldots 1} + X_{\ldots 2} + X_{\ldots 3} + X_{\ldots 4}) - 2(X_{\ldots 5} + X_{\ldots 6}), \qquad \text{(XV-3)}$$

$$R_4 = (X_{\ldots 1} + X_{\ldots 4}) - (X_{\ldots 2} + X_{\ldots 3}), \qquad \text{(XV-4)}$$

and

$$R_5 = (X_{\ldots 1} + X_{\ldots 3} + 2X_{\ldots 6}) - (X_{\ldots 2} + X_{\ldots 4} + 2X_{\ldots 5}). \qquad \text{(XV-5)}$$

The contrast R_1 represents a column contrast; R_2 and R_3 represent contrasts among row totals. Only the contrasts R_4 and R_5 represent comparisons which are not row or column comparisons. Since the contrasts are orthogonal and since each is associated with a single degree of freedom, there are only two degrees of freedom associated with the sum of squares,

$$\frac{R_4^2}{6(1+1+1+1) = 24} + \frac{R_5^2}{6(1+1+4+1+1+4) = 72}, \qquad \text{(XV-6)}$$

among rectangles (eliminating row and column comparisons). The divisors are obtained as the sum of the squares of the coefficients times the number of items making up each total $X_{\ldots g}$.

The error variance is obtained by subtraction; thus, total *ss* − (row *ss* + column *ss* + treatment *ss* + rectangle (eliminating row and column) *ss*) = error *ss*. Computation of standard errors and of tests of significance proceed in the same manner as for the ordinary latin square design.

Still further grouping of the treatments may be made; thus:

A	B	C	D	E	F
D	E	F	A	B	C
F	C	B	E	A	D
E	A	D	C	F	B
B	D	E	F	C	A
C	F	A	B	D	E

In the above *super magic latin square* the six treatments all appear in a row, in a column, in a 3 × 2 rectangle (indicated by ....), and in a 2 × 3 rectangle

(indicated by •••••). The additional restriction on the allocation of the treatments must be accounted for in the analysis. With proper randomization the breakdown of the degrees of freedom in the analysis of variance is

Source of variation	df	
Row	5	
Column	5	
Treatment	5	
Residual	20	
3 × 2 rectangles (ign. 2 × 3 rect.)		2
2 × 3 rectangles (elim. 3 × 2 rect.)		1
Error		16
Total	35	

The sum of squares for the 3 × 2 rectangles is given by equation (XV-6). Contrasts similar to those given in equations (XV-1) to (XV-5) may be set up for the 2 × 3 rectangles; thus:

$$S_1 = (X_{....1} + X_{....2} + X_{....3}) - (X_{....4} + X_{....5} + X_{....6}), \qquad \text{(XV-7)}$$

$$S_2 = (X_{....1} + X_{....4}) - (X_{....2} + X_{....5}) \qquad \text{(XV-8)}$$

$$S_3 = (X_{....1} + X_{....2} + X_{....4} + X_{....5}) - 2(X_{....3} + X_{....6}), \qquad \text{(XV-9)}$$

$$S_4 = (X_{....1} + X_{....5}) - (X_{....2} + X_{....4}), \qquad \text{(XV-10)}$$

and

$$S_5 = (X_{....1} + X_{....2} + 2X_{....6}) - (2X_{....3} + X_{....4} + X_{....5}), \qquad \text{(XV-11)}$$

where $X_{....f}$ represents the total for the six individual yields, X_{ijkgf}, in the 2 × 3 rectangles:

Row no.	Column number 1 2	3 4	5 6
1 2 3	$X_{....1}$	$X_{....2}$	$X_{....3}$
4 5 6	$X_{....4}$	$X_{....5}$	$X_{....6}$

The contrasts S_4 and S_5 are the only ones which do not represent row or column comparisons. Therefore, the sum of squares due to the 2 × 3 rectangles (eliminating row and column comparisons) is given by the expression,

$$\left(\frac{S_4}{2} - \frac{S_5}{6}\right)^2 \qquad \text{(XV-12)}$$

with one degree of freedom. The total, row, column, treatment, and rectangle (3 × 2) sums of squares are computed in the same manner as for the magic

latin square. The error sum of squares with 16 degrees of freedom is obtained by subtraction.

Other groupings of the treatments, such as graeco-magic-latin squares, are possible. It is doubtful if these designs will attain prominence in experimental work, since the additional stratification of the treatments over the latin square will not control any considerable amount of the heterogeneity in most experimental situations. Unless the experimenter is certain that a relatively large amount of the extraneous variation will be controlled by the additional grouping, it is recommended that the latin square design be used instead of the magic or super magic latin square designs.

XV-4 Weighing Designs

Several papers [7-10, 162, 174, 185, 211, 249, 254, 319] have been written on the problem of weighing several light objects on a chemical balance, spring balance, or other weighing device. This problem was first introduced by Yates [319]. He states that the information obtained by weighing each of seven objects separately and then making an eighth weighing with no object to determine the zero correction is only one-fourth that obtained by weighing the seven objects according to the following design:

Weighing	Objects weighed
1	a, b, c, d, e, f, g
2	a, b, d
3	a, c, e
4	a, f, g
5	b, c, f
6	b, e, g
7	c, d, g
8	d, e, f

This illustrates the great gain in efficiency that may be made by using the appropriate design. In the weighing design described by Yates, every object is weighed four times and every object is weighed twice with every other object. The difference between the first four and the last four weighings is an estimate of four times the weight of object a.

The last seven weighings represent a balanced incomplete block design with $v = 7$, $b = 7$, $r = 3$, $k = 3$, and $\gamma = 1$. Also, the above weighing design may be related to the estimation of effects in the 2^3 factorial when the factors do not interact. If the weighings 1, 2, 3, $\cdots$, 8 are numbered 111, 110, 101, 100, 011, 010, 001, and 000, respectively, then the weight of the object a is represented by the comparison giving the A effect. Also, the estimates of the weights of the objects b, c, d, e, f, and g are given by the estimates of the effects B, C, AB, AC, BC, and ABC, respectively.

The reader is referred to the above cited references for a more comprehensive discussion of weighing designs and of weighing designs for other situations.

CHAPTER XVI

Covariance

XVI-1 Introduction

The methods for reducing or controlling experimental variation are [126, 127, 218, 273]:

(i) Selection of homogeneous material and (or) of a uniform environment.
(ii) Stratification (grouping) of the material and (or) of the environment into homogeneous subgroups.
(iii) Refinement of experimental techniques.
(iv) Measurement of related variates and use of covariance analysis.

If the above procedures fail to furnish adequate control of the experimental variability, then the number of replicates will need to be increased to the point where adequate precision is attained.

Local control (methods (i) and (ii)), randomization, and replication form the basis for valid experimental designs. If the material and the environment are uniform, then a completely randomized design is used. If the material and the environment are grouped into homogeneous subgroups with enough material in each subgroup to compare all treatments, a randomized complete block design is used. If the subgroups are not large enough to compare all treatments, an incomplete block design is used. Other situations may require the use of additional grouping of treatments in order to attain homogeneity of the material in the subgroups (e.g., the latin square design).

Some discussion on the refinement of experimental technique (method (iii)) is given in Chapter III and elsewhere in the text. In most instances the variability associated with a given technique is relatively small in experimental work. However, cases do arise in which technique errors add considerably to the variability. Technique errors should be guarded against, but they should not be overemphasized.

The use of concomitant observations or covariates may be effective in reducing the experimental variability over and above the reduction obtained by stratification of the experimental material. Alternatively, the use of related variates may be substituted for one or more of the other methods in certain experimental situations. A rule to follow here might be: *if the experimental variation cannot be controlled by stratification, then measure related*

variates and use covariance. Also, it may be more advantageous to use covariance than to use stratification, since fewer degrees of freedom are usually required to control the variation. On the other hand, stratification may be used to control variability even when no measurement of variability is available, whereas covariance could not be used.

The covariates in covariance analyses should be *selected* with care, and the results should be *interpreted* in light of the particular covariates used and of the particular values of the covariates obtained [13]. The effects eliminated should be irrelevant to the objectives of the experiment, and the tests of significance of treatment comparisons should not be invalidated [126, sec. 55]. In a large number of experimental situations the differences among treatment means for the covariates should be random differences and not treatment differences. If the treatment means for the covariates represent real treatment differences, then it may not be advisable to use covariance analyses. Instead, a multivariate analysis may be required to assess the true treatment differences in the observed characters. The particular analysis to be used depends upon the nature of the experiment.

In order to use the average within regression, the individual regressions must be estimates of the same regression. Also, the individual treatment variances adjusted for covariation with the independent variates must form a homogeneous set of variances if the pooled variance is to be used in tests of significance. In addition, the deviations from regression should (theoretically) be normally and independently distributed in order for the resulting t, F, and χ^2 tests to be valid [126, 127, 273].

In some experimental situations the regression of Y on X will vary from treatment to treatment [158]. The use of an average regression in such cases is often misleading. In the analysis of data in which the treatment regressions differ, the standard errors for treatment differences become rather complex; also, the use of covariance is to be questioned. The fact that the treatment regressions differ may be sufficient reason for conducting additional experiments on the subgroups of treatments which do have common regressions.

Covariance analyses for a number of the designs discussed in previous chapters are illustrated below. The covariance analysis for other designs is a straightforward extension of the methods presented herein. In addition to covariance analyses for various designs, examples illustrating the control of variation by covariance instead of by stratification, and other selected topics are included.

XVI-2 Completely Randomized Design

The covariance analysis for a completely randomized design is described in most textbooks in statistics as the covariance analysis for a one-way classification or as an "among groups and within groups covariance analysis."

Snedecor [272, 273] presents several examples and discusses the results for several covariance analyses [also see 58, 80, 84, 86, 119, 126, 127, 158, 218, 252, 308, 314].

The completely randomized design consists of v treatments randomly allotted to the $r. = \sum_{i=1}^{v} r_i$ experimental units with r_i replicates on the ith treatment. The linear model for covariance is[1]

$$Y_{ij} = \mu + \tau_i + \beta(X_{ij} - \bar{x}) + \epsilon_{ij}, \tag{XVI-1}$$

where Y_{ij} = the yield of the jth observation on the ith treatment, μ = population mean, τ_i = effect of ith treatment, $X_{ij} - \bar{x}$ = deviation of the ijth covariate X from the mean of the covariate, β = common regression for all treatments, and ϵ_{ij} = random error component.

The key-out for the degrees of freedom and for the sums of squares is presented in table XVI-1. The sums of squares for both the variate Y and

TABLE XVI-1. Covariance analysis for a completely randomized design

Source of variation	Sum of products				Adjusted ss	
	df	y^2	xy	x^2	df	ss
Total	$r.-1$	T_{yy}	T_{xy}	T_{xx}	$r.-2$	$T_{yy}' = T_{yy} - T_{xy}^2/T_{xx}$
Group	$v-1$	G_{yy}	G_{xy}	G_{xx}	-	-
Within	$r.-v$	E_{yy}	E_{xy}	E_{xx}	$r.-v-1$	$E_{yy}' = E_{yy} - E_{xy}^2/E_{xx}$
Group adjusted for the average within group regression					$v-1$	$G_{yy}' = T_{yy}' - E_{yy}'$

the covariate X are obtained in the usual manner for a completely randomized design. The sum of the cross products of Y and X is obtained as follows:

$$T_{xy} = \sum_{i=1}^{v} \sum_{j=1}^{r_i} X_{ij} Y_{ij} - X..Y../r., \tag{XVI-2}$$

$$G_{xy} = \sum_{i} \frac{X_i.Y_i.}{r_i} - X..Y../r., \tag{XVI-3}$$

and

$$E_{xy} = T_{xy} - G_{xy} = \sum_{i} \left\{ \sum_{j=1}^{r_i} X_{ij} Y_{ij} - \frac{X_i.Y_i.}{r_i} \right\}. \tag{XVI-4}$$

The average within group linear regression coefficient is estimated by

$$b_{yx} = E_{xy}/E_{xx}, \tag{XVI-5}$$

and the average within group correlation is estimated by

$$r_{yx} = E_{xy}/\sqrt{E_{yy}E_{xx}}. \tag{XVI-6}$$

[1] If desired, the constant $\beta\bar{x}$ may be included with μ, and an estimate of $\mu' = \mu - \beta\bar{x}$ may be obtained in place of the estimate for μ.

The reduction in the error sum of squares due to fitting a linear regression coefficient is equal to

$$E_{xy}^2/E_{xx} = r_{yx}^2 E_{yy} = b_{yx}^2 E_{xx}, \tag{XVI-7}$$

with one degree of freedom.

The F test for the comparison of the group mean square adjusted for within-group regression is

$$F(v - 1, r. - v - 1df) = \frac{G'(r. - v - 1)}{E'(v - 1)}. \tag{XVI-8}$$

The group (treatment) means adjusted for the average within-group regression are obtained from the formula,

$$\bar{y}_i' = \bar{y}_i - b(\bar{x}_i - \bar{x}). \tag{XVI-9}$$

The adjustment $b_{yx}(\bar{x}_i - \bar{x})$ is subtracted from the unadjusted mean, $\bar{y}_i$, to obtain the adjusted treatment mean.

The estimated variance of a difference between two adjusted means is [119, 218, 314]

$$\frac{E_{yy}'}{f_e'}\left\{\frac{1}{r_1} + \frac{1}{r_2} + \frac{(\bar{x}_1 - \bar{x}_2)^2}{E_{xx}}\right\}, \tag{XVI-10}$$

where r_1 = number of replicates on treatment one, r_2 = number of replicates on treatment two, $\bar{x}_1$ = mean of X values for treatment one, $\bar{x}_2$ = mean of X values for treatment two, and f_e' = degrees of freedom for E_{yy}'.

The above covariance analysis pertains to a single group of treatments. In many situations the treatments may be partitioned into subgroups, or the treatments may form a factorial arrangement. The covariance analysis for such situations is presented in table XVI-2. In the table,

$$(A_{yy} + E_{yy})' = A_{yy} + E_{yy} - (A_{xy} + E_{xy})^2/(A_{xx} + E_{xx}). \tag{XVI-11}$$

The other two sums of squares, $(B_{yy} + E_{yy})'$ and $(R_{yy} + E_{yy})'$, are similarly obtained. The tests of significance and adjustment of the means proceed in the same manner as described previously.

When several lines in the analysis of variance are to be adjusted for the error regression, the adjusted sum of squares may be evaluated as $A_{yy} - 2bA_{xy} + b^2A_{xx}$, where $b = E_{xy}/E_{xx}$ and the other values are defined in table XVI-2 [60, sec. 3.86; 127, sec. 49.1]. An adjusted sum of squares for A, say, obtained in this manner is equivalent to the sum of squares among adjusted means; thus:

$$A_{yy} - 2bA_{xy} + b^2A_{xx} = k\sum[\bar{y}_{i..} - \bar{y} - b(\bar{x}_{i..} - \bar{x})]^2, \tag{XVI-12}$$

where k = number of individuals in $\bar{y}_{i..}$ and the other symbols are as defined above. Since this sum of squares contains a component due to error of adjustment, it is not strictly comparable to $E_{yy}' = E_{yy} - E_{xy}^2/E_{xx}$ for testing the null hypothesis. The resulting F values will be too high; the amount of overestimation in F depends upon the differences between treatment means

TABLE XVI-2. Covariance analysis for subgroups of treatments in a completely randomized design

Source of variation	Sum of products				Adjusted ss	
	df	y^2	xy	x^2	df	ss
Total	$r_{.}-1$	T_{yy}	T_{xy}	T_{xx}	-	-
Group	v-1	-	-	-	-	-
A	a-1	A_{yy}	A_{xy}	A_{xx}	-	-
B	b-1	B_{yy}	B_{xy}	B_{xx}	-	-
Residual (Interaction)	v-a-b+1	R_{yy}	R_{xy}	R_{xx}	-	-
Error	$r_{.}-v=f_e$	E_{yy}	E_{xy}	E_{xx}	f_e-1	$E_{yy}-E_{xy}^2/E_{xx}$
Error + A	$r_{.}-v+a-1$	$A_{yy}+E_{yy}$	$A_{xy}+E_{xy}$	$A_{xx}+E_{xx}$	f_e+a-2	$(A_{yy}+E_{yy})'$
A adjusted	-	-	-	-	a-1	by subtraction
Error + B	$r_{.}-v+b-1$	$B_{yy}+E_{yy}$	$B_{xy}+E_{xy}$	$B_{xx}+E_{xx}$	f_e+b-2	$(B_{yy}+E_{yy})'$
B adjusted	-	-	-	-	b-1	by subtraction
Error + residual	$r_{.}-a-b+1$	$R_{yy}+E_{yy}$	$R_{xy}+E_{xy}$	$R_{xx}+E_{xx}$	$r_{.}-a-b$	$(R_{yy}+E_{yy})'$
Residual adj.	-	-	-	-	v-a-b+1	by subtraction

for the covariate. Since this component of error is relatively small in a number of cases, the above method of obtaining the adjusted sums of squares is useful.

XVI-3 Randomized Complete Block Design

The linear model for a covariance analysis for a randomized complete block design is

$$Y_{ij} = \mu + \tau_i + \rho_j + \beta(X_{ij} - \bar{x}) + \epsilon_{ij}, \qquad \text{(XVI-13)}$$

where μ, τ_i, ρ_j, β, and ϵ_{ij} are the mean effect, the effect of the ith treatment, the effect of the jth replicate, the average regression coefficient from the error line, and a random error component, respectively. The quantity $(X_{ij} - \bar{x})$ represents the deviation of the observed X_{ij} from the mean of the experiment for the covariate.

In the above linear model the assumption is made that all treatments are subject to the same error variation after correction for regression. Also, it is assumed that the error regressions for the different treatments are all estimates of the same parameter β. Any effect of the treatments on the mean of the covariate must be considered when interpreting the results from a covariance analysis.

The analysis of covariance for a randomized complete block design is

presented in table XVI-3. The average within treatment and replicate regression, β, is estimated by the quantity,

$$b_{yx} = E_{xy}/E_{xx}. \tag{XVI-14}$$

The reduction in the error sum of squares due to fitting a linear regression of the covariate X on the variate Y is obtained from equation (XVI-7), and the adjusted treatment means are obtained from formula (XVI-9), using the value of b obtained in equation (XVI-14). Tests of significance are made in the manner described in the preceding section.

TABLE XVI-3. Covariance analysis for a randomized complete block design

Source of variation	Sum of products				Adjusted ss	
	df	y^2	xy	x^2	df	ss
Replicate	$r-1$	R_{yy}	R_{xy}	R_{xx}	-	-
Treatment	$v-1$	T_{yy}	T_{xy}	T_{xx}	-	-
Error	$(r-1)(v-1)$	E_{yy}	E_{xy}	E_{xx}	$(r-1)(v-1)-1$	$E_{yy}' = E_{yy} - E_{xy}^2/E_{xx}$
Treatment + error	$r(v-1)$	$T_{yy}+E_{yy}$	$T_{xy}+E_{xy}$	$T_{xx}+E_{xx}$	$r(v-1)-1$	$(T_{yy}+E_{yy})' = T_{yy}+E_{yy} -(T_{xy}+E_{xy})^2/(T_{xx}+E_{xx})$
Treatment adjusted for average error regression					$v-1$	$(T_{yy}+E_{yy})'-E_{yy}'=T_{yy}'$

The treatment sum of squares may be partitioned into its component parts, and each part may be adjusted for the average error regression. The procedure follows that outlined in the preceding section.

Snedecor [273, sec. 12.7] discusses an unusual example of a covariance analysis. The yields, Y_{ij}, of sugar beets for the various fertilizer treatments were significantly different, but the adjustment of yields for number, X_{ij}, of sugar beet plants per acre erased the differences between treatments. The treatments affected the number of sugar beet plants per unit area but did not affect the weight per individual sugar beet. The covariance analysis pointed up the characteristic affected by the treatments and the reason for the increased tonnage per acre.

If several samples or individuals are obtained from each experimental unit, the problem of which regression to use arises. To illustrate, suppose that s random samples are obtained from each experimental unit and that the linear model is of the form,

$$Y_{ijh} = \mu + \tau_i + \rho_j + \beta(X\,\text{dev}) + \epsilon_{ij} + \delta_{ijh}, \tag{XVI-15}$$

where μ, τ_i, and ρ_j are defined in the same manner as in equation (XVI-13), ϵ_{ij} represents a random component associated with the ijth experimental unit,

δ_{ijh} represents a random component associated with the hth sample in the ijth experimental unit, β is the regression coefficient, and the (X dev) depends upon the regression used. The (X dev) in ordinary field experiments is equal to[1]

$$(X \text{ dev}) = \left\{\frac{X_{ij.}}{s} - \frac{X_{...}}{svr}\right\} = \left\{\frac{X_{ij.}}{s} - \bar{x}\right\}, \qquad \text{(XVI-16)}$$

where $X_{ij.}$ = experimental unit total, $X_{...}$ = experiment total, r = number of replicates, v = number of treatments, and s = number of samples per experimental unit. The computation of the regression coefficient from the experimental error in the analysis of variance applies to most experimental situations and is similar to the preceding example. However, there are situations in which the regression is computed as the average regression within experimental units. In this case the (X dev) is equal to

$$(X \text{ dev}) = X_{ijh} - \bar{x}. \qquad \text{(XVI-17)}$$

The (X dev) in formula (XVI-17) corresponds to the (X dev) given for the completely randomized design with the treatments in a factorial arrangement (table XVI-2).

XVI-4 Latin Square Design

The covariance analysis for the latin square design is a straightforward extension of the results given in the previous sections [127, sec. 49; 273, Ch. 12]. The linear model for the yield of the ijhth observation from a latin square design with a covariate is

$$Y_{ijh} = \mu + \gamma_i + \rho_j + \tau_h + \beta(X_{ijh} - \bar{x}) + \epsilon_{ijh}, \qquad \text{(XVI-18)}$$

where μ = mean effect, γ_i = effect of ith column, ρ_j = effect of jth row, τ_h = effect of hth treatment, β = regression coefficient of residual Y variations on the residual X variations, and ϵ_{ijh} = random error component. The key-out for the degrees of freedom and for the sums of squares in the covariance analysis of an experiment arranged in a $k \times k$ latin square design is given in table XVI-4. The average error regression, β, is estimated by the quantity,

$$b = E_{xy}/E_{xx}; \qquad \text{(XVI-19)}$$

the average correlation of the error deviations is estimated by

$$r = E_{xy}/\sqrt{E_{xx}E_{yy}}. \qquad \text{(XVI-20)}$$

The adjusted treatment means are obtained from formula (XVI-9), using the value of b obtained in formula (XVI-19). The variance of a difference

[1]In some experiments the value of the covariate is available on the experimental unit but not on each individual item within the unit (e.g., stand per plot), and, therefore, the (X dev) will necessarily be that given by formula (XVI-16).

between two adjusted treatment means is obtained from formula (XVI-10) with appropriate substitutions.

The treatment sum of squares in the latin square design may be partitioned into its component parts and treated in the manner described in section XVI-2. If a group of individuals or samples constitutes the experimental unit, the problem of which regression to use is treated in the same

TABLE XVI-4. Analysis of covariance for a k × k latin square design

Source of variation	Sum of products				Adjusted ss	
	df	y^2	xy	x^2	df	ss
Row	k-1	R_{yy}	R_{xy}	R_{xx}	-	-
Column	k-1	C_{yy}	C_{xy}	C_{xx}	-	-
Treatment	k-1	T_{yy}	T_{xy}	T_{xx}	-	-
Error	(k-1)(k-2)	E_{yy}	E_{xy}	E_{xx}	k^2-3k+1	$E_{yy}' = E_{yy} - E_{xy}^2/E_{xx}$
Treatment + error	$(k-1)^2$	V_{yy}	V_{xy}	V_{xx}	k^2-2k	$V_{yy}' = V_{yy} - V_{xy}^2/V_{xx}$
Treatment adjusted for average error regression					k-1	$V_{yy}' - E_{yy}' = T_{yy}'$

manner as described in the previous section. Also, heterogeneity of regression coefficients or of residual variances and treatment differences in the covariate cause the same problems in the latin square design as in other designs.

Example XVI-1. In the production of double cross hybrid corn an advantageous procedure is to use a single cross which produces no viable pollen (male sterile single cross). The detasseling of the female line is eliminated. Before a male sterile single cross can be substituted for its nonsterile counterpart, it is necessary to ascertain whether or not yields from the resulting double cross hybrids differ. H. L. Everett[1] conducted a number of these yield tests comparing the standard single cross with its male sterile counterpart. Theoretically, the sterile and nonsterile single crosses should differ only in the characteristic of sterility.

The yield and stand data presented in table XVI-5 are for the six varieties (double crosses) related in the following manner:

Other single cross in the double cross combination	Sterile single cross = I	Nonsterile single cross = II
1	A = 1 × I	F = 1 × II
2	B = 2 × I	C = 2 × II
3	E = 3 × I	D = 3 × II

[1]Cornell Univ., unpublished results.

A 6 × 6 latin square design was used. The plot size was twenty-four plants. Data were recorded on various other characteristics in addition to yield and stand. Since the stand varied from plot to plot and since stand was not considered to be a varietal characteristic, the yields of the six varieties corrected for stand differences were compared. The adjustment for stand differences is to be made regardless of whether or not there is a significant reduction due to fitting a linear regression of yield on stand. If the reduction due to fitting a regression is relatively small, then little is to be gained from the additional work, since the unadjusted and adjusted means are approximately equal. The procedure followed in covariance analysis should be to decide upon the covariate prior to studying the data. If the data are studied to determine which covariate to use, the resulting tests of significance and the use of tabulated probabilities are invalidated.

TABLE XVI-5. Yields (lbs./plot) of ear corn (Y) and number of plants (X) of 6 corn varieties (letters) in a 6 × 6 latin square design (systematic order of design given below)

Cross	A	B	C	D	E	F	Totals
X	18	16	18	14	15	17	98
Y	8.6	7.5	6.7	6.5	8.2	4.7	42.2
Cross	B	C	D	E	F	A	
X	16	15	16	19	21	14	101
Y	7.6	8.2	8.3	4.6	5.2	6.5	40.4
Cross	C	E	A	F	B	D	
X	15	16	16	19	15	17	98
Y	5.7	9.6	7.1	4.8	7.5	7.5	42.2
Cross	D	F	B	A	C	E	
X	18	18	20	18	22	17	113
Y	8.3	5.3	10.0	6.8	7.8	6.6	44.8
Cross	E	D	F	B	A	C	
X	15	18	19	16	16	15	99
Y	8.3	7.9	5.4	9.3	4.8	8.0	43.7
Cross	F	A	E	C	D	B	
X	18	20	19	17	17	15	106
Y	5.7	8.7	8.1	6.1	7.2	7.4	43.2
ΣX	100	103	108	105	106	95	615
ΣY	44.2	47.2	45.6	38.1	40.7	40.7	256.5

The sums of squares for both the variate Y and the covariate X are computed in the usual manner for a latin square design. The various sums of products of the variate and covariate are obtained as follows:

Total sum of xy *products:*

$$18(8.6) + 16(7.5) + \cdots + 15(7.4) - \frac{615(256.5)}{36} = -10.175.$$

Row sum of xy *products:*

$$\frac{98(42.2) + \cdots + 106(43.2)}{6} - \frac{615(256.5)}{36} = 4.708.$$

Column sum of xy *products:*

$$\frac{100(44.2) + \cdots + 95(40.7)}{6} - \frac{615(256.5)}{36} = 3.358.$$

Variety sum of xy *products:*

$$\frac{102(42.5) + \cdots + 112(31.1)}{6} - \frac{615(256.5)}{36} = -25.208.$$

Error sum of xy *products:*

$$-10.175 - (4.708 + 3.358 - 25.208) = 6.967.$$

It is instructive to compute the error sum of xy products directly from the deviations $(X_{ijh} - \bar{x}_{i..} - \bar{x}_{.j.} - \bar{x}_{..h} + 2\bar{x})$ and $(Y_{ijh} - \bar{y}_{i..} - \bar{y}_{.j.} - \bar{y}_{..h} + 2\bar{y})$ [273, ex. 12.13].

Since the treatment design was selected for specific contrasts, the treatment sum of squares was partitioned into three parts: S = steriles vs nonsteriles, P = comparison among three pairs of crosses, and $S \times P$ = interaction of pair with type of single cross. The sums of the xy products for these comparisons are obtained as follows from the totals in table XVI-6.

TABLE XVI-6. Unadjusted and adjusted means for 6 corn varieties

Variety	Lbs. of corn (Y)		Stand (X)		Adjustment for mean $[-b(\bar{x}_h - \bar{x})]$	Adjusted mean
	Total	Mean	Total	Mean		
A	42.5	7.08	102	17.00	0.008	7.09
B	49.3	8.22	98	16.33	0.077	8.30
C	42.5	7.08	102	17.00	0.008	7.09
D	45.7	7.62	100	16.67	0.042	7.66
E	45.4	7.57	101	16.83	0.026	7.60
F	31.1	5.18	112	18.67	-0.164	5.02
A + F	73.6	6.13	214	17.83	-0.077	6.05
B + C	91.8	7.65	200	16.67	0.042	7.69
E + D	91.1	7.59	201	16.75	0.034	7.62
A + B + E	137.2	7.62	301	16.72	0.037	7.66
C + D + F	119.3	6.63	314	17.44	-0.037	6.59
Total	256.5	7.125	615	17.08	-	-

Sum of xy *products for* S:

$$\frac{137.2(301) + 119.3(314)}{18} - \frac{615(256.5)}{36} = \frac{(137.2 - 119.3)(301 - 314)}{36} = -6.464.$$

Sum of xy *products for* P:

$$\frac{214(73.6) + 200(91.8) + 201(91.1)}{12} - \frac{615(256.5)}{36} = -13.417.$$

Sum of xy *products for* S $\times$ P:

$$-25.208 - (-6.464 - 13.417) = -5.327.$$

The adjusted sums of squares for error and for error + S are

$$28.939 - (6.967)^2/67.667 = 28.222$$

and
$$(8.900 + 28.939) - \frac{(-6.464 + 6.967)^2}{4.694 + 67.667} = 37.839 - .003 = 37.836.$$

The sum of squares for S adjusted for the average error regression is equal to 37.836 − 28.222 = 9.614. The other adjusted sums of squares are similarly obtained.

An alternative partitioning of the treatment sum of squares is set forth in the bottom part of table XVI-7. These comparisons among the treatments are the ones of importance if the interest lies in the difference between members of a pair. The sums of products are obtained in the manner described above.

The F tests for the comparison of the mean squares for S, P, and $S \times P$ with the error mean square are

$$F = \frac{9.614}{1.485} = 6.47 > F_{025}(1, 19df) = 5.92,$$

$$F = \frac{8.952}{1.485} = 6.03 > F_{01}\,(2, 19df) = 5.93,$$

and

$$F = \frac{3.236}{1.485} = 2.18 < F_{10}\,(2, 19df) = 2.61.$$

Thus, if the 5 per cent level of significance (per line in the analysis) were being used, the hypothesis of no difference among the pairs of crosses and between the double cross containing sterile and nonsterile single crosses would be rejected. The evidence of interaction is slight. From the alternative covariance analysis, it is noted that most of the difference in the S comparison is derived from the single contrast A vs F.

The adjusted treatment means are obtained from formula (XVI-9) and are presented in table XVI-6. The standard error of a difference between two adjusted treatment means (e.g., A vs F) is computed from formula (XVI-10) and is equal to

$$\sqrt{1.485\left\{\frac{1}{6} + \frac{1}{6} + \frac{(17.00 - 18.67)^2}{67.667}\right\}} = 0.746.$$

The coefficient of variation is equal to

$$\sqrt{1.485}/7.125 = 17 \text{ per cent.}$$

It was assumed that stand was not a characteristic of the double crosses used. This assumption does not appear to be violated when the variety mean square for stand is compared with its error; thus, $F = 4(19.917)/67.667 = 1.2$. However, when the treatment regression, $-25.208/19.917 = -1.266$, is compared with the error regression, $6.967/67.667 = 0.103$, it is found that these two regressions differ. The sum of squares for this contrast is equal to $\frac{(-25.208)^2}{19.917} + \frac{(6.967)^2}{67.667} - \frac{(-25.208 + 6.967)^2}{19.917 + 67.667}$ $= 31.905 + 0.717 - 3.799 = 28.823$, with one degree of freedom, and $F = 28.823/1.485$ $= 19.4 > F_{01}(1, 19df)$.[1] For these crosses, poorer stand and higher yield characteristics appear to be associated together (see table XVI-6), but for a given double cross the relationship of yield with stand is positive, though not significantly so. Also, the negative linear relationship between yield and stand for variety means accounts for most of the variety sum of squares; thus, the residual sum of squares is $32.413 - (-25.208)^2/19.917 = 0.508$, with 4 degrees of freedom. The difference in

[1]This assumes that treatment and error regressions have the same variance.

TABLE XVI-7. Analysis of covariance

Source of variation	df	Sum of products x^2	xy	y^2
Total	35	134.750	-10.175	73.268
Row	5	29.583	4.708	1.906
Column	5	17.583	3.358	10.010
Variety	5	19.917	-25.208	32.413
S	1	4.694	- 6.464	8.900
P	2	10.167	-13.417	17.722
S x P	2	5.056	- 5.327	5.791
Error	20	67.667	6.967	28.939
Error + S	21	72.361	0.503	37.839
Error + P	22	77.834	- 6.450	46.661
Error + (S x P)	22	72.723	1.640	34.730

Source of variation	Regression df	Regression ss	Adjusted results df	Adjusted results ss	Adjusted results ms
Error	1	0.717	19	28.222	1.485
Error + S	1	0.003	20	37.836	-
Error + P	1	0.535	21	46.126	-
Error + (S x P)	1	0.037	21	34.693	-
S adjusted for error regression			1	9.614	9.614
P " " " "			2	17.904	8.952
S x P " " " "			2	6.471	3.236

Alternative analysis of covariance

Source of variation	df	Sum of products x^2	xy	y^2
Among progenies (P)	2	10.167	-13.417	17.722
Within progenies	3	9.750	-11.791	14.691
A vs F	1	8.333	-9.500	10.830
B vs C	1	1.333	-2.267	3.853
E vs D	1	0.083	-0.025	0.008
Error	20	67.667	6.967	28.939
Error + P	22	77.834	- 6.450	46.661
Error + (A vs F)	21	76.000	- 2.533	39.769
Error + (B vs C)	21	69.000	4.700	32.792
Error + (E vs D)	21	67.750	6.942	28.947

Source of variation	Regression df	Regression ss	Adjusted results df	Adjusted results ss	Adjusted results ms
Error	1	0.717	19	28.222	1.485
Error + P	1	0.535	21	46.126	-
Error + (A vs F)	1	0.084	20	39.685	-
Error + (B vs C)	1	0.320	20	32.472	-
Error + (E vs D)	1	0.711	20	28.236	-
P adjusted for average regression			2	17.904	8.952
A vs F adjusted for average regression			1	11.463	11.463
B vs C " " " "			1	4.250	4.250
E vs D " " " "			1	0.014	0.014

yield between the two groups involving sterile and nonsterile single crosses may be due to a change in the stand characteristic. Evidently, stand is a varietal characteristic, and any use that is made of the results should be made with this in mind [273, sec. 12.5].

XVI-5 Split Plot Design

The linear model for a split plot design with a covariate may be one of the following two forms, depending upon the assumptions made by the experimenter:

$$Y_{ijk} = \mu + \rho_j + \tau_i + \delta_{ij} + \alpha_k + (\alpha\tau)_{ik} + \beta_1(\bar{x}_{ij.} - \bar{x}) + \beta_2(X_{ijk} - \bar{x}_{ij.}) + \epsilon_{ijk} \quad \text{(XVI-21)}$$

or

$$Y_{ijk} = \mu + \rho_j + \tau_i + \delta_{ij} + \alpha_k + (\alpha\tau)_{ik} + \beta(X_{ijk} - \bar{x}) + \epsilon_{ijk}. \quad \text{(XVI-22)}$$

In the first equation, it is assumed that the regression, β_1, for whole plot deviations is different from the regression, $\beta_2 = \beta$, for split plot deviations. In equation (XVI-22), it is assumed that there is only one regression, β. The key-out for the degrees of freedom and for the sums of products in the analysis of covariance, under the assumption that equation (XVI-21) is the appropriate linear model, is given in table XVI-8. If equation (XVI-22) is deemed to be the appropriate linear model for the covariance analysis, the key-out for the degrees of freedom and for the sums of products is as presented in table XVI-9.

TABLE XVI-8. Covariance analysis for a split plot design[a]

Source of variation	Sum of products				Adjusted ss	
	df	y^2	xy	x^2	df	ss
Replicate	r-1	R_{yy}	R_{xy}	R_{xx}	-	-
Variety	v-1	V_{yy}	V_{xy}	V_{xx}	-	-
Error (a)	f_a	A_{yy}	A_{xy}	A_{xx}	f_a-1	$A_{yy}' = A_{yy} - A_{xy}^2/A_{xx}$
Variety + error	r(v-1)	W_{yy}	W_{xy}	W_{xx}	r(v-1)-1	$W_{yy}' = W_{yy} - W_{xy}^2/W_{xx}$
Variety adjusted for whole plot regression					v-1	$V_{yy}' = W_{yy}' - A_{yy}'$
Treatment	t-1	T_{yy}	T_{xy}	T_{xx}	-	-
Treatment x variety	f_{tv}	TV_{yy}	TV_{xy}	TV_{xx}	-	-
Error (b)	f_b	B_{yy}	B_{xy}	B_{xx}	f_b-1	$B_{yy}' = B_{yy} - B_{xy}^2/B_{xx}$
Treatment + error	f_b+t-1	U_{yy}	U_{xy}	U_{xx}	f_b+t-2	$U_{yy}' = U_{yy} - U_{xy}^2/U_{xx}$
Trt. x var. + error	$f_{tv}+f_b$	Z_{yy}	Z_{xy}	Z_{xx}	$f_{tv}+f_b-1$	$Z_{yy}' = Z_{yy} - Z_{xy}^2/Z_{xx}$
Treatment adjusted for split plot regression					t-1	$T_{yy}' = U_{yy}' - B_{yy}'$
Treatment x variety adj. for split plot regression					f_{tv}	$TV_{yy}' = Z_{yy}' - B_{yy}'$

[a]In the above, the varieties represent the whole plot treatments and the treatments represent the split plot treatments.

TABLE XVI-9. Alternative covariance analysis for the whole plots in a split plot design[a]

Source of variation	Sum of products				Adjusted ss	
	df	y^2	xy	x^2	df	ss
Replicate	$r-1$	R_{yy}	R_{xy}	R_{xx}	-	-
Variety	$v-1$	V_{yy}	V_{xy}	V_{xx}	-	-
Error (a)	f_a	A_{yy}	A_{xy}	A_{xx}	-	-
Error (b)	f_b	B_{yy}	B_{xy}	B_{xx}	f_b-1	$B_{yy}' = B_{yy} - B_{xy}^2/B_{xx}$
Variety + error (b)	f_b+v-1	P_{yy}	P_{xy}	P_{xx}	f_b+v-2	$P_{yy}' = P_{yy} - P_{xy}^2/P_{xx}$
Error (a) + error (b)	$f_a + f_b$	Q_{yy}	Q_{xy}	Q_{xx}	f_a+f_b-1	$Q_{yy}' = Q_{yy} - Q_{xy}^2/Q_{xx}$
Variety adjusted for split plot regression					$v-1$	$V_{yy}' = P_{yy}' - B_{yy}'$
Error (a) adjusted for split plot regression					f_a	$A_{yy}' = Q_{yy}' - B_{yy}'$

[a]The remainder of the covariance analysis is that given in the bottom part of table XVI-8.

If no knowledge is available as to which equation ((XVI-21) or (XVI-22)) is more appropriate, the results from several similar experiments may be summarized, and the regression coefficients may be tested for homogeneity [273, Ch. 12]. Then, on the basis of these results the experimenter may be able to decide which model is more suitable for his purposes. However, for most field experiments it is suggested that equation (XVI-22) be used [260]. One further consideration in favor of using the error (*b*) regression to adjust sums of squares and means is the small number of degrees of freedom usually associated with the error (*a*) sums of products. However, this procedure encounters difficulties in making tests of significance. H. F. Smith (written correspondence) has shown that $V_{yy}'f_a/A_{yy}'(v-1)$ in table XVI-9 is not distributed as Snedecor's F and has suggested that the ratio of the mean squares adjusted by formula (XVI-12), say $V_af_a/A_a(v-1)$, be used [also, see 14, p. 145]. Further work is required in order to clarify tests of significance when the analysis is of the form given in table XVI-9.

The adjusted means are obtained from formula (XVI-9), using the appropriate regression coefficient. The variance of an adjusted mean is given by equation (XVI-10). The comments relative to homogeneity of regression coefficients and of residual variances apply here as well as in preceding sections.

In a split split plot design, there is the possibility of one, two, or three regression coefficients, depending upon the assumptions made by the experimenter. Likewise, in designs with additional splitting of the plots, there is the possibility of one, two, · · ·, or more regression coefficients. The analytical procedures for these designs are determined along the lines discussed for the split plot design. The error (*b*) and error (*c*) sums of products usually will be associated with sufficient degrees of freedom to permit relatively accurate

determinations of the regression coefficients, insofar as the accuracy of these regressions depends upon the number of degrees of freedom.

XVI-6 Split Block Design

The problem of determining the appropriate regression model arises in the split block design in the same manner as in the split plot design. If the whole plot treatments are subject to different regressions, the linear model for a split block design with a covariate is

$$Y_{ijh} = \mu + \rho_j + \tau_i + \delta_{ij} + \alpha_h + \gamma_{jh} + (\alpha\tau)_{ih} + \epsilon_{ijh} + \beta_1(\bar{x}_{ij.} - \bar{x}) + \beta_2(\bar{x}_{j.h} - \bar{x}) + \beta_3(X_{ijh} - \bar{x}_{ij.} - \bar{x}_{j.h} + \bar{x}). \quad \text{(XVI-23)}$$

If there is only one regression, the linear model is

$$Y_{ijh} = \mu + \rho_j + \tau_i + \delta_{ij} + \alpha_h + \gamma_{jh} + (\alpha\tau)_{ih} + \epsilon_{ijh} + \beta(X_{ijh} - \bar{x}). \quad \text{(XVI-24)}$$

In the above equations, μ = mean effect, ρ_j = effect of jth replicate, τ_i = effect of whole plot treatments in one direction, α_h = effect of whole plot treatments in the other direction, $(\alpha\tau)_{ih}$ = effect of ith treatment in one set on hth treatment in the second set, δ_{ij} = random component of error variation associated with ijth plot, γ_{jh} = random component of error variation associated with jhth plot, ϵ_{ijh} = random component of error variation associated with ijhth plot, β_1 = regression coefficient associated with residual variations of Y on X for the first set of whole plot treatments, β_2 = regression coefficient associated with the second set of whole plot treatments, and $\beta_3 = \beta$ = regression coefficient associated with residual variations of Y on X within whole plots.

If equation (XVI-23) is the appropriate linear model, the key-out for the degrees of freedom and for the sums of products in a covariance analysis of a split block design (let treatment = whole plot treatments in one direction and let variety = whole plot treatments in the other direction) is given in table XVI-10. If equation (XVI-24) is the appropriate linear model, then the covariance analysis is similar in form to the second key-out for the split plot design (table XVI-9). The sums of products C_{yy}, C_{xy}, and C_{xx} are added to the comparable sums of products in the other lines in the analysis of covariance; the treatment means are adjusted by formula (XVI-9), using the regression coefficient $b = C_{xy}/C_{xx}$.

The adjustment of means and the computation of standard errors under the two linear models proceed in the same manner as discussed in the previous section. More complex designs of the split block nature give rise to more regression coefficients and to a linear model of the form given by equation (XVI-23). If a single regression coefficient suffices, as in equation (XVI-24), the procedure follows that outlined above.

The number of degrees of freedom associated with the $T \times$ replicate and the $V \times$ replicate mean squares is usually relatively small. An equation

TABLE XVI-10. Covariance analysis for a split block design

Source of variation	Sum of products				Adjusted ss	
	df	y^2	xy	x^2	df	ss
Replicate	r-1	R_{yy}	R_{xy}	R_{xx}	-	-
Treatment	t-1	T_{yy}	T_{xy}	T_{xx}	-	-
Trt. x replicate	f_t	A_{yy}	A_{xy}	A_{xx}	f_t-1	$A_{yy}' = A_{yy} - A_{xy}^2/A_{xx}$
Trt. + (trt. x rep.)	r(t-1)	W_{yy}	W_{xy}	W_{xx}	r(t-1)-1	$W_{yy}' = W_{yy} - W_{xy}^2/W_{xx}$
Treatment adjusted for whole plot regression					t-1	$T_{yy}' = W_{yy}' - A_{yy}'$
Variety	v-1	V_{yy}	V_{xy}	V_{xx}	-	-
Variety x replicate	f_v	B_{yy}	B_{xy}	B_{xx}	f_v-1	$B_{yy}' = B_{yy} - B_{xy}^2/B_{xx}$
Var. + (var. x rep.)	r(v-1)	U_{yy}	U_{xy}	U_{xx}	r(v-1)-1	$U_{yy}' = U_{yy} - U_{xy}^2/U_{xx}$
Variety adjusted for whole plot regression					v-1	$V_{yy}' = U_{yy}' - B_{yy}'$
Treatment x var.	f_{tv}	TV_{yy}	TV_{xy}	TV_{xx}	-	-
Rep. x trt. x var.	f_c	C_{yy}	C_{xy}	C_{xx}	f_c-1	$C_{yy}' = C_{yy} - C_{xy}^2/C_{xx}$
Trt. x var. + (rep. x trt. x var.)	rf_{tv}	Z_{yy}	Z_{xy}	Z_{xx}	$rf_{tv}-1$	$Z_{yy}' = Z_{yy} - Z_{xy}^2/Z_{xx}$
Trt. x var. adjusted for within whole plot regression					f_{tv}	$TV_{yy}' = Z_{yy}' - C_{yy}'$

similar to (XVI-24) will be appropriate for many field experiments. Therefore, it is recommended that equation (XVI-24), or a similar one, be used as the linear model for a split block design unless it is known that equation (XVI-23) is the appropriate linear model (see preceding section regarding tests of significance).

XVI-7 One-Restrictional Lattice Designs

Covariance analyses in lattice designs are complicated by several factors. Some of these complications are encountered in the designs discussed thus far, while others represent new problems. The first question that arises in the covariance analysis of a lattice design is whether or not interblock information is to be recovered. Cornish [66] and others [183, 252] have discussed the analysis of covariance without recovery of interblock information. With recovery of interblock information the next question that arises is the one pertaining to the appropriate regression for adjusting the treatment means. Cochran [48] states that the following three complications arise: (i) the expectation of the block (eliminating treatment) mean square adjusted for the intrablock error regression is not equal to $\sigma_e^2 + (r-1)k\sigma_\beta^2/r$, since the block

degrees of freedom are not orthogonal with the degrees of freedom for regression (the correct coefficient is slightly smaller than $(r-1)k/r$ because of the non-orthogonality; see section XVI-12.3), (ii) the variance of a difference between two adjusted treatment means is not of the form given by equation (XVI-10) but involves the block and intrablock mean squares for both the variate and the covariate, and (iii) the use of the intrablock error regression coefficient introduces a correlation between the pseudo-effect estimated from the replicates in which the effect is unconfounded with incomplete block differences (intrablock estimate) and the same pseudo-effect estimated from the replicates in which the effect is confounded with incomplete block differences (interblock estimate). The use of separate regression coefficients for interblock estimates and for intrablock estimates eliminates the difficulties mentioned in (i) and (iii) above. However, the calculation of the adjusted means becomes more difficult, and the difficulty of obtaining the correct standard error of a difference between two adjusted means still remains [48].

XVI-7.1 COVARIANCE ANALYSIS WITHOUT RECOVERY OF INTERBLOCK INFORMATION

Cornish [66] has discussed covariance analyses for the balanced incomplete block design (section XIII-2.1), the double lattice design (section XI-3.1), the triple lattice design (section XI-3.2), and the cubic lattice design (section XI-4.1). The form of the analysis of covariance without recovery of interblock information for the balanced incomplete block design with the treatments not in compact replicates is given in the top part of table XVI-11. The form for covariance analysis for the other lattice designs is that given in the top part of table XVI-12. Covariance analyses for other one-restrictional lattice designs may be obtained by a simple extension of the results given by Cornish [66] and others [183, 252].

The sums of squares for treatments (eliminating block) for both the variate Y and the covariate X are obtained in the manner described in section XIII-2. The sum of the xy products for the treatment (eliminating block) effects in a balanced lattice design is equal to

$$\sum_j Q_j(y)Q_j(x)/k^2r(\text{Eff}), \qquad \text{(XVI-25)}$$

where $Q_j(y) = Q_j$ value for the variate Y, $Q_j(x) = Q_j$ value for the covariate X, and the other symbols are defined in formula (XIII-8). The sum of products for partially balanced lattice designs is of a slightly more complex form. The sum of the xy products for treatment (eliminating block) effects in a double lattice design is

$$\sum_{i,j} X_{\cdot ij}\bar{y}_{ij}' - \sum_i X_{1i\cdot}\sum_j \bar{y}_{ij}'/k - \sum_j X_{2\cdot j}\sum_i \bar{y}_{ij}'/k, \qquad \text{(XVI-26)}$$

where

$$\bar{y}_{ij}' = \bar{y}_{ij} + \frac{1}{rk}(Y_{1\cdot j} - Y_{2\cdot j} + Y_{2i\cdot} - Y_{1i\cdot}).$$

The value of $\bar{y}_{ij}'$ (formula (XI-120)) is the adjusted treatment mean ignoring interblock information. The corresponding sums of the xy products for treatments (eliminating block effects) in a triple lattice design and in a cubic lattice design are, respectively,

$$\sum_{i,j} X_{\cdot ij}\bar{y}_{ij}' - \sum_i X_{1i\cdot} \sum_j \bar{y}_{ij}'/k - \sum_j X_{2\cdot j} \sum_i \bar{y}_{ij}'/k - \sum_u X_{3\cdot u} \sum_v \bar{y}_{vu}'/k \tag{XVI-27}$$

($u = i + nj$ and v = a corresponding function of i and j, say $v = i + (n - 1)j$ for $n = 2, 3, \cdots$, or $k - 1$) and

$$\sum_{i,j,k} X_{\cdot ijk}\bar{y}_{ijk}' - \sum_{j,k} X_{\cdot\cdot jk} \sum_i \bar{y}_{ijk}'/k - \sum_{i,k} X_{\cdot i\cdot k} \sum_j \bar{y}_{ijk}'/k - \sum_{i,j} X_{\cdot ij\cdot} \sum_k \bar{y}_{ijk}'/k. \tag{XVI-28}$$

The quantities $\bar{y}_{ij}'$ and $\bar{y}_{ijk}'$ are the intrablock estimates of the treatment means. The sum of the xy products for treatments (eliminating block) in other partially balanced lattice designs may be obtained from the formulae given by Rao [252, sec. 7].

The procedure for obtaining the treatment (eliminating block) sum of squares adjusted for the intrablock regression is given in the top part of tables XVI-11 and XVI-12. Tests of significance, adjustment of treatment means for intrablock regression, and computation of variances of treatment differences follow in much the same manner as for previous designs (formulae (XVI-8), (XVI-9), and (XVI-10)).

TABLE XVI-11. Covariance analysis for one-restrictional lattices with the treatments not arranged in complete replicates

Source of variation	Sum of products				Adjusted ss	
	df	y^2	xy	x^2	df	ss
Block (ign. treatment)	b-1	-	-	-	-	-
Treatment (elim. block)	v-1	V_{yy}	V_{xy}	V_{xx}	-	-
Intrablock error	f_e	E_{yy}	E_{xy}	E_{xx}	f_e-1	$E_{yy}' = E_{yy} - E_{xy}^2/E_{xx}$
Treatment + error	f_e+v-1	W_{yy}	W_{xy}	W_{xx}	f_e+v-2	$W_{yy}' = W_{yy} - W_{xy}^2/W_{xx}$
Trt. (elim. block) adjusted for intrablock regression					v-1	$V_{yy}' = W_{yy}' - E_{yy}'$
Treatment (ign. block)	v-1	-	-	-	-	-
Block (elim. treatment)	b-1	B_{yy}	B_{xy}	B_{xx}	b-2	$B_{yy}^* = B_{yy} - B_{xy}^2/B_{xx}$
Intrablock error	f_e	E_{yy}	E_{xy}	E_{xx}	f_e-1	$E_{yy}' = E_{yy} - E_{xy}^2/E_{xx}$
Block + error	f_e+b-1	U_{yy}	U_{xy}	U_{xx}	f_e+b-2	$U_{yy}' = U_{yy} - U_{xy}^2/U_{xx}$
Block (elim. trt.) adjusted for intrablock regression					b-1	$B_{yy}' = U_{yy}' - E_{yy}'$

TABLE XVI-12. Covariance analysis for one-restrictional lattices with the treatments arranged in complete replicates

Source of variation	Sum of products				Adjusted ss	
	df	y^2	xy	x^2	df	ss
Replicate	r-1	-	-	-	-	-
Block (ign. treatment)	b-r	-	-	-	-	-
Treatment (elim. block)	v-1	V_{yy}	V_{xy}	V_{xx}	-	-
Intrablock error	f_e	E_{yy}	E_{xy}	E_{xx}	f_e-1	$E_{yy}'=E_{yy}-E_{xy}^2/E_{xx}$
Treatment + error	f_e+v-1	W_{yy}	W_{xy}	W_{xx}	f_e+v-2	$W_{yy}'=W_{yy}-W_{xy}^2/W_{xx}$
Treatment (elim. block) adj. for intrablock regression					v-1	$V_{yy}' = W_{yy}' - E_{yy}'$
Treatment (ign. block)	v-1	-	-	-	-	-
Block (elim. treatment)	b-r	B_{yy}	B_{xy}	B_{xx}	b-r-1	$B_{yy}^*=B_{yy}-B_{xy}^2/B_{xx}$
Block + error	f_e+b-r	U_{yy}	U_{xy}	U_{xx}	$f_e+b-r-1$	$U_{yy}'=U_{yy}-U_{xy}^2/U_{xx}$
Block (elim. treatment) adj. for intrablock regression					b-r	$B_{yy}' = U_{yy}' - E_{yy}'$

XVI-7.2 COVARIANCE ANALYSIS WITH RECOVERY OF INTERBLOCK INFORMATION

Covariance analyses in lattice designs with recovery of interblock information pose several problems. These are mentioned in the introduction to this section [48]. The first problem is to determine the appropriate regression coefficient to use in adjusting the treatment means for covariation in X. The two regression coefficients obtained from an analysis of an experiment designed as a one-restrictional lattice are

$$b^* = B_{xy}/B_{xx} \tag{XVI-29}$$

and

$$b = E_{xy}/E_{xx}, \tag{XVI-30}$$

where the sums of products are defined in tables XVI-11 to XVI-12. If b^* is approximately equal to b, Cochran [48] and Robinson and Watson [259] suggest that b be used in formula (XVI-9) to adjust the treatment means. If b^* and b are not approximately of the same magnitude, they suggest the use of a weighted regression coefficient but do not state the form that the weighting should take. Presumably it should be of the following form for the ijth treatment in a double lattice design with two replicates (see formula (XI-9)):

$$\bar{y}_{ij}(\text{adj}) = \bar{y}_{ij} - \frac{b}{2}\left(X_{1ij} - \frac{X_{1i\cdot}}{k} + X_{2ij} - \frac{X_{2\cdot j}}{k}\right) - \frac{b^*}{2}\left(\frac{X_{1i\cdot}}{k} - \bar{x} + \frac{X_{2\cdot j}}{k} - \bar{x}\right), \tag{XVI-31}$$

where $\bar{y}_{ij}$ = unadjusted mean of the ijth treatment, X_{gij} = yield of covariate for the ijth treatment in the gth replicate, the i subscript refers to the levels of the pseudo-factor a confounded in arrangement 1, the j subscript refers to the levels of the pseudo-factor b confounded in arrangement 2, and the regression coefficients are defined in equations (XVI-29) and (XVI-30). Formula (XVI-31) reduces to formula (XVI-9) when $b = b^*$ and when the block means are equal to $\bar{x}$. Also, formula (XVI-31) may be extended for more replicates of a double lattice design and for other one-restrictional lattice designs. Thus, for n groups (arrangements) of a one-restrictional lattice design with q repetitions of the groups the formula for adjusting the treatment means for variation in the covariate using a weighted regression is

$$\bar{y}_{ij}(\text{adj}) = \bar{y}_{ij} - \frac{b}{nq}\left(X_{1ij.} - \frac{X_{1i..}}{k} + \cdots + X_{nij.} - \frac{X_{n\cdot u\cdot}}{k}\right) - \frac{b^*}{nq}\left(\frac{X_{1i..}}{k} - q\bar{x} + \cdots + \frac{X_{n\cdot u\cdot}}{k} - q\bar{x}\right), \qquad \text{(XVI-32)}$$

where treatment ij falls in the uth block of the nth group, the last dot refers to the summation over the q repetitions of a group, and the other symbols are defined in equation (XVI-31).

After the treatment means, $\bar{y}_{ij}$, are adjusted for the covariate, the next step is to obtain the weights for the treatment means, adjusted to a common mean of the covariate, $\bar{y}_{ij}(\text{adj})$, for incomplete block differences. The weights w and w' are obtained from the covariance analysis instead of from the variance analysis. In order to denote this, let u be the estimate of intrablock information in place of w, and let u' be the estimate of interblock information in place of w'. The estimate of interblock information from a one-restrictional lattice design with $r = n$ arrangements is

$$u = (f_e - 1)/E_{yy}'; \qquad \text{(XVI-33)}$$

the estimate of interblock information is either

$$u' = \frac{r - 1}{rB_{yy}^*/(f_b - 1) - E_{yy}'/(f_e - 1)} \qquad \text{(XVI-34)}$$

or

$$u' = \frac{r - 1}{rB_{yy}'/f_b - E_{yy}'/(f_e - 1)}, \qquad \text{(XVI-35)}$$

depending upon whether a weighted regression is used to adjust the treatment means for the covariate (formula (XVI-32)) or whether b alone is used to adjust the treatment means (formula (XVI-9)). Formula (XVI-35) is an approximation, since the coefficient $(r - 1)/r$ should be decreased slightly to account for the non-orthogonality introduced by using a common regression (section XVI-12.3).

The weights or adjustments of incomplete block effects for the treatment

totals from a double lattice design adjusted for the covariate by formula (XVI-9) are

$$c_x' = \mu\{[(A)._i - 2(X)_i]_y - b[(A)._i - 2(X)_i]_x\} \qquad \text{(XVI-36)}$$

and

$$c_y' = \mu\{[(B)._j - 2(Y)_j]_y - b[(B)._j - 2(Y)_j]_x\}, \qquad \text{(XVI-37)}$$

where μ is defined by equation (XI-34) using the weights obtained from formulae (XVI-33) and (XVI-35), the quantities within the square brackets are defined in section XI-3.1, example XI-2, the y subscript in the right side of the equations refers to the variate Y, the x subscript in the right side of the equations refers to the covariate X, and b is defined by equation (XVI-30). The subscripts on the left-hand side of equations (XVI-36) and (XVI-37) refer to the two groupings in the double lattice design.

If the two regressions b and b^* were used, the last part of formulae (XVI-36) and (XVI-37) would have to be extended to a form similar to that obtained in formula (XVI-31).

The extension of formulae (XVI-36) and (XVI-37) to the triple lattice and to the rectangular lattice is straightforward and will not be given here, since they appear in the literature [48, 259]. The weights for the treatment totals adjusted for the covariate in the cubic lattice design are given below.

$$c_x' = \frac{\mu_y}{k}(j_y - bj_x) + \frac{\lambda_y - \mu_y}{k^2}[J_y + M_y' - b(J_x + M_x')], \qquad \text{(XVI-38)}$$

$$c_y' = \frac{\mu_y}{k}(m_y - bm_x) + \frac{\lambda_y - \mu_y}{k^2}[M_y + L_y' - b(M_x + L_x')], \qquad \text{(XVI-39)}$$

and

$$c_z' = \frac{\mu_y}{k}(l_y - bl_x) + \frac{\lambda_y - \mu_y}{k^2}[L_y + J_y' - b(L_x + J_x')], \qquad \text{(XVI-40)}$$

where the symbols on the right hand side of the equations are defined in section XI-4.1, example XI-6, method of analysis (i), the weights μ and λ are computed from the adjusted sums of squares using formulae (XI-63) and (XI-64), and the subscripts on the right-hand sides of the equations refer to the covariate and variate, respectively. The subscripts on the left-hand sides of the equations refer to the X, Y, and Z groupings in the cubic lattice design. The value of b is equal to E_{xy}/E_{xx}, where the latter values are defined in table XVI-12.

The analysis of the one-restrictional lattice design now proceeds in the manner described in Chapters XI and XIII using the above weights u and u' in place of w and w', respectively. The standard errors obtained from the formulae given in Chapters XI and XIII are only approximate, since they do not contain the sampling variance for the estimated regression coefficients. This additional component of variance usually will be small in the larger lattice designs [48].

XVI-8 Two- and Higher-Restrictional Lattice Designs

XVI-8.1 TWO-RESTRICTIONAL LATTICE DESIGNS

Covariance analyses for the two-restrictional lattice designs, except for the lattice square, have not been described in the literature. Cornish [66] and Bliss and Dearborn [25] present the covariance analysis, illustrated with an example, for a semi-balanced lattice square design without recovery of interblock information.

With the recovery of interblock information, complications of the nature discussed in the previous section arise in the covariance analysis for an experiment designed as a lattice square. If a common regression is used, the intrablock error regression, the same problems arise as for the one-restrictional lattice designs. If a weighted regression is used to adjust the treatment means, three regression coefficients are necessary; thus:

$$b = E_{xy}/E_{xx}, \tag{XVI-41}$$

$$b_c = C_{xy}/C_{xx}, \tag{XVI-42}$$

and

$$b_r = R_{xy}/R_{xx}, \tag{XVI-43}$$

where the sums of products are defined in tables XVI-13 and XVI-14. The adjusted treatment mean using a weighted regression similar to that given in equation (XVI-30) for the two replicates of a lattice square is

$$\begin{aligned}\bar{y}_{ij}(\text{adj}) = \bar{y}_{ij} &- \frac{b}{2}\left(X_{1ij} - \frac{(A)_{1i} + (B)_{1j}}{2k} + X_{2ij} - \frac{(AB)_{2g} + (AB^2)_{2h}}{2k}\right) \\ &- b_r\left(\frac{(A)_{1i} + (AB)_{2g}}{4k} - \frac{\bar{x}}{2}\right) - b_c\left(\frac{(B)_{ij} + (AB^2)_{2h}}{4k} - \frac{\bar{x}}{2}\right),\end{aligned} \tag{XVI-44}$$

where the b's are defined in formulae (XVI-41) to (XVI-43), the other quantities on the right-hand side of the equation except $\bar{y}_{ij}$ refer to yields of the covariate X, and the various quantities in the brackets are defined in example XII-1. The A and AB pseudo-effects are confounded with rows and the B and AB^2 pseudo-effects are confounded with columns. Formula (XVI-44) reduces to formula (XVI-9) when $b = b_r = b_c$ and when the row and column means are equal to $\bar{x}$. The extension to more replicates follows immediately from the results in the previous section.

After the treatment means are adjusted to a common covariate mean, the next step is to obtain the weights for rows and columns in order to adjust the treatment means for row and column effects. The procedure for two replicates of the lattice square design follows that for the double lattice design. If a single regression coefficient is used, the same difficulties encountered in the double lattice design arise in the estimation of the row and column infor-

TABLE XVI-13. Covariance analysis for a semi-balanced lattice square design

Source of variation	Sum of products df	y^2	xy	x^2	Adjusted ss df	ss
Replicate	$r-1$	-	-	-	-	-
Treatment (ign. row and column)	$v-1$	-	-	-	-	-
Row (elim. treatment)	f_r	R_{yy}	R_{xy}	R_{xx}	f_r-1	$R_{yy}{}^*=R_{yy}-R_{xy}{}^2/R_{xx}$
Column (elim. treatment)	f_c	C_{yy}	C_{xy}	C_{xx}	f_c-1	$C_{yy}{}^*=C_{yy}-C_{xy}{}^2/C_{xx}$
Intrablock error	f_e	E_{yy}	E_{xy}	E_{xx}	f_e-1	$E_{yy}{}'=E_{yy}-E_{xy}{}^2/E_{xx}$
Row + error[a]	f_r+f_e	L_{yy}	L_{xy}	L_{xx}	f_r+f_e-1	$L_{yy}{}'=L_{yy}-L_{xy}{}^2/L_{xx}$
Column + error[a]	f_c+f_e	M_{yy}	M_{xy}	M_{xx}	f_c+f_e-1	$M_{yy}{}'=M_{yy}-M_{xy}{}^2/M_{xx}$
Row (elim. trt.) adjusted for intrablock regression					f_r	$R_{yy}{}' = L_{yy}{}' - E_{yy}{}'$
Column (elim. trt.) adjusted for intrablock regression					f_c	$C_{yy}{}' = M_{yy}{}' - E_{yy}{}'$

[a] $L_{uv} = R_{uv} + E_{uv}$; $M_{uv} = C_{uv} + E_{uv}$; $u, v = x, y$.

TABLE XVI-14. Covariance analysis for a balanced lattice square design

Source of variation	Sum of products df	y^2	xy	x^2	Adjusted ss df	ss
Replicate	$r-1$	-	-	-	-	-
Treatment (ign. row and column)	$v-1$	-	-	-	-	-
Row (elim. treatment; ign. column)	f_r	-	-	-	-	-
Column (elim. treatment and row)	f_c	C_{yy}	C_{xy}	C_{xx}	f_c-1	$C_{yy}{}^*=C_{yy}-C_{xy}{}^2/C_{xx}$
Row (elim. treatment and column)	f_r	R_{yy}	R_{xy}	R_{xx}	f_r-1	$R_{yy}{}^*=R_{yy}-R_{xy}{}^2/R_{xx}$
Column (elim. treatment; ign. row)	f_c	-	-	-	-	-
Intrablock error	f_e	E_{yy}	E_{xy}	E_{xx}	f_e-1	$E_{yy}{}'=E_{yy}-E_{xy}{}^2/E_{xx}$
Row + error[a]	f_r+f_e	L_{yy}	L_{xy}	L_{xx}	f_r+f_e-1	$L_{yy}{}'=L_{yy}-L_{xy}{}^2/L_{xx}$
Column + error[a]	f_c+f_e	M_{yy}	M_{xy}	M_{xx}	f_c+f_e-1	$M_{yy}{}'=M_{yy}-M_{xy}{}^2/M_{xx}$
Row (elim. trt. and col.) adj. for intrablock regression					f_r	$R_{yy}{}'= L_{yy}{}'- E_{yy}{}'$
Column (elim. trt. and row) adj. for intrablock regression					f_c	$C_{yy}{}'= M_{yy}{}'- E_{yy}{}'$

[a] $L_{uv} = R_{uv} + E_{uv}$; $M_{uv} = C_{uv} + E_{uv}$; $u, v = x, y$.

mation. The approximate weights u_r, u_c, and u from the covariance analysis corresponding to the estimated weights w_r, w_c, and w, from the variance analysis are

$$u_r = \frac{r-1}{rR_{yy}'/f_r - E_{yy}'/(f_e - 1)}, \tag{XVI-45}$$

$$u_c = \frac{r-1}{rC_{yy}'/f_c - E_{yy}'/(f_e - 1)}, \tag{XVI-46}$$

and

$$u = (f_e - 1)/E_{yy}', \tag{XVI-47}$$

where the sums of squares are defined in tables XVI-13 and XVI-14.

The weighting factors for rows and columns are given in formulae (XII-3) and (XII-4) using the quantities u_r, u_c, and u for the quantities w_r, w_c, and w, respectively. The following four adjustments are added to the treatment totals adjusted for covariation in X to obtain the treatment total adjusted for covariation in the X variable and for row and column effects:

$$c_x' = \lambda\{[(A)._i - 2(A)_{1i}]_y - b[(A)._i - 2(A)_{1i}]_x\}, \tag{XVI-48}$$

$$c_w' = \lambda\{[(AB)._g - 2(AB)_{2g}]_y - b[(AB)._g - 2(AB)_{2g}]_x\}, \tag{XVI-49}$$

$$c_y' = \mu\{[(B)._j - 2(B)_{1j}]_y - b[(B)._j - 2(B)_{1j}]_x\}, \tag{XVI-50}$$

and

$$c_z' = \mu\{[(AB^2)._k - 2(AB^2)_{2k}]_y - b[(AB^2)._k - 2(AB^2)_{2k}]_x\}, \tag{XVI-51}$$

where the quantities in the above equations are defined in formulae (XVI-36), (XVI-37), (XVI-41), and in example XII-1.

The remainder of the covariance analysis proceeds in the same manner as for a variance analysis. The resulting standard errors are approximate, since they do not contain a component for the sampling variance of the regression coefficient. The comments on the covariance analysis given in the previous sections hold in this section as well.

The extension of the above results to lattice square designs with more replicates and to other two-restrictional lattice designs is straightforward.

XVI-8.2 LATTICE DESIGNS WITH MORE THAN TWO RESTRICTIONS

Covariance analyses for lattice designs with more than two restrictions on the allocation of the treatments within a complete replicate are of the same nature as those described in sections XVI-7.1, XVI-7.2, and XVI-8.1. The number of regression coefficients increases, and the computations and adjustments become more complex. If possible, proper stratification of the experimental material into homogeneous subgroups of the covariate X eliminates the necessity of a covariance analysis. As the nature of the computations becomes more complex and as the assumptions relating to covariance analyses become harder to satisfy, the more necessary it becomes to control covariation of X and Y by proper stratification.

XVI-9 Change-Over Designs

Fieller [117] describes a covariance analysis for the simple change-over design (see section XIV-2.1). Covariance analyses for the other designs in Chapter XIV have not been discussed. The analysis of covariance for the simple change-over and for the double change-over designs are discussed here (tables XVI-15 and XVI-16). The analytical procedure for other designs may be obtained from the results presented below.

The linear model for the yield of the *ijh*th observation from a simple change-over design with a covariate is

$$Y_{ijh} = \mu + \rho_i + \gamma_j + \tau_h + \beta(X_{ijh} - \bar{x}) + \epsilon_{ijh}, \qquad \text{(XVI-52)}$$

where μ = mean effect, ρ_i = *i*th row effect, γ_j = *j*th column effect, τ_h = *h*th treatment effect, β = regression coefficient for Y deviations on X deviations, and ϵ_{ijh} = random component of error. The key-out for the degrees of freedom and for the sums of products is given in table XVI-15. The adjusted treatment means are computed from formula (XVI-9), and the variance of a difference between two adjusted means is obtained from formula (XVI-10).

The covariance analysis for a double change-over design is somewhat more complex. The linear model for the *ijh*th observation from a double change-over design with a covariate is (for $q = 1$)

$$Y_{ijh} = \mu + \alpha_i + \beta_j + (\alpha\beta)_{ij} + \delta_h + \rho_p + \beta(X_{ijh} - \bar{x}) + \epsilon_{ijh}, \qquad \text{(XVI-53)}$$

where the effects are defined in equation (XIV-11) and β is the regression of the Y deviations on the X deviations in the error line. The key-out for the degrees of freedom and for the sums of products in the covariance analysis of a double change-over design is presented in table XVI-16. The sums of the xy products for the treatment effects in the analysis of covariance are given below. The formulae are for three treatments and for one set, but may easily be extended for more treatments and for more sets.

TABLE XVI-15. Covariance analysis for a simple cross-over design with 2 treatments

Source of variation	Sum of products				Adjusted ss	
	df	y^2	xy	x^2	df	ss
Column	r - 1	-	-	-	-	-
Row	1	-	-	-	-	-
Treatment	1	T_{yy}	T_{xy}	T_{xx}	-	-
Error	r - 2	E_{yy}	E_{xy}	E_{xx}	r - 3	$E_{yy}' = E_{yy} - E_{xy}^2/E_{xx}$
Error + treatment[a]	r - 1	S_{yy}	S_{xy}	S_{xx}	r - 2	$S_{yy}' = S_{yy} - S_{xy}^2/S_{xx}$
Treatment adjusted for error regression					1	$T_{yy}' = S_{yy}' - E_{yy}'$

[a] $S_{uv} = T_{uv} + E_{uv}$; u, v = x, y.

TABLE XVI-16. Covariance analysis for q repetitions of a double change-over design with 3 treatments

Source of variation	Sum of products				Adjusted ss	
	df	y^2	xy	x^2	df	ss
Group	2q-1	-	-	-	-	-
Period	2	-	-	-	-	-
Period x group	2(2q-1)	-	-	-	-	-
Within group	4q	-	-	-	-	-
Direct (ign. residual)	2	-	-	-	-	-
Residual (elim. direct)	2	R_{yy}	R_{xy}	R_{xx}	-	-
Direct (elim. residual)	2	D_{yy}	D_{xy}	D_{xx}	-	-
Residual (ign. direct)	2	-	-	-	-	-
Error	8q-4	E_{yy}	E_{xy}	E_{xx}	8q-5	$E_{yy}' = E_{yy} - E_{xy}^2/E_{xx}$
Error + residual[a]	8q-2	T_{yy}	T_{xy}	T_{xx}	8q-3	$T_{yy}' = T_{yy} - T_{xy}^2/T_{xx}$
Error + direct[a]	8q-2	S_{yy}	S_{xy}	S_{xx}	8q-3	$S_{yy}' = S_{yy} - S_{xy}^2/S_{xx}$
Residual (elim. direct) adjusted for error regression					2	$R_{yy}' = T_{yy}' - E_{yy}'$
Direct (elim. residual) adjusted for error regression					2	$D_{yy}' = S_{yy}' - E_{yy}'$

[a] $T_{uv} = R_{uv} + E_{uv}$; $S_{uv} = D_{uv} + E_{uv}$; u, v = x, y.

Direct effects eliminating residual effects (see formula (XIV-4)):

$$\begin{aligned} D_{xy} = {} & [5X_{..A} + 2a - b - c + s_2 + s_6 - 2X_{...}/3][5Y_{..A} + 2a - b - c + s_2 \\ & + s_6 - 2Y_{...}/3]/120 + [5X_{..B} + 2b - a - c + s_3 + s_4 - 2X_{...}/3][5Y_{..B} \\ & + 2b - a - c + s_3 + s_4 - 2Y_{...}/3]/120 + [5X_{..C} + 2c - a - b + s_1 + s_5 \\ & - 2X_{...}/3][5Y_{..C} + 2c - a - b + s_1 + s_5 - 2Y_{...}/3]/120 \\ & - \frac{(4X_{...})(4Y_{...})}{360} \end{aligned} \qquad \text{(XVI-54)}$$

Residual effects ignoring direct effects (see formula (XIV-5)):

$$\begin{aligned} & \tfrac{1}{30}\{[3a + s_2 + s_6]_x[3a + s_2 + s_6]_y + [3b + s_3 + s_4]_x[3b + s_3 + s_4]_y \\ & + [3c + s_1 + s_5]_x[3c + s_1 + s_5]_y\} \\ & - [3(a + b + c) + X_{...}][3(a + b + c) + Y_{...}]/90. \end{aligned} \qquad \text{(XVI-55)}$$

Direct effects ignoring residual effects (see formula (XIV-3)):

$$\tfrac{1}{6}\sum^{3} X_{..k}Y_{..k} - X_{...}Y_{...}/18. \qquad \text{(XVI-56)}$$

Residual effects eliminating direct effects (see formula (XIV-6)):

$$\begin{aligned} R_{xy} = \tfrac{1}{24}\{&[3a + X_{..A} + s_2 + s_6][3a + Y_{..A} + s_2 + s_6] \\ &+ [3b + X_{..B} + s_3 + s_4][3b + Y_{..B} + s_3 + s_4] \\ &+ [3c + X_{..C} + s_1 + s_5][3c + Y_{..C} + s_1 + s_5]\} \\ &- [3(a+b+c) + 2X_{...}][3(a+b+c) + 2Y_{...}]/72. \quad \text{(XVI-57)} \end{aligned}$$

The quantities within the square brackets are computed from the data for the variate or for the covariate, depending upon whether Y or X appears in a bracket or as a subscript. The sums of products for the error line are obtained by subtraction. The errors of estimate are obtained as described in table XVI-16.

The treatment means adjusted for residual effects are obtained from an extension of formula (XVI-9). For a given treatment, say A, from a double change-over design with three treatments, the mean adjusted for residual effects (see formula (XIV-12)) and for the covariate is equal to

$$\begin{aligned} \bar{y}_A' = {}& [5Y_{..A} + 2a - b - c + s_2 + s_6 - 2Y_{...}/3]/24 \\ &- \frac{E_{xy}}{24E_{xx}}\{[5X_{..A} + 2a - b - c + s_2 + s_6 - 2X_{...}/3] - 4X_{...}/3\}, \end{aligned} \quad \text{(XVI-58)}$$

where the various quantities are defined above. The residual effect for treatment A adjusted for the direct effect of treatment A and for the covariate is obtained from the formula (see formula (XIV-19)),

$$\begin{aligned} \frac{[3a + Y_{..A} + s_2 + s_6]}{8} - \frac{E_{xy}}{8E_{xx}}\{&[3a + X_{..A} + s_2 + s_6] \\ &- [(a+b+c) + 2X_{...}/3]\}. \end{aligned} \quad \text{(XVI-59)}$$

The formulae for obtaining adjusted means for treatments B and C and for double change-over designs with more than three treatments are constructed in an analogous manner.

XVI-10 Covariance and Unequal Numbers of Observations per Treatment

XVI-10.1 COVARIANCE ON DUMMY VARIATES FOR MISSING OR MIXED-UP PLOT YIELDS

Bartlett [14] has described a method for calculating the analysis of variance for an experiment with missing data. A covariate (sometimes called a dummy covariate) consisting of zeros associated with all the yields obtained in the experiment and a one (a minus one may be more advantageous computationally) for the yield of the missing observation is used. A zero is inserted for the missing yield, and then an analysis of covariance is calculated. The error mean squares obtained in the above analysis of covariance and in

the analysis of variance after computing the yield for the missing datum are identical. The estimated yield for the missing observation is

$$y_0 - bx_0 = -b = -E_{xy}/E_{xx}, \tag{XVI-60}$$

where $y_0 = 0$, $x_0 = 1$, and the sums of products E_{xy} and E_{xx} are obtained from the error line in the analysis of covariance. The sums of squares for the covariate are simply the degrees of freedom divided by the total number of observations in the experiment.

If more than one observation is missing, a covariate is used for each missing datum, and a multiple covariance is computed [14]. Nair [216] applied Bartlett's method to the randomized complete block and to the latin square designs with several missing observations and with mixed-up observations. Smith [269] discusses the use of dummy covariates in a latin square design and presents some basic viewpoints on the use of covariance for such purposes.

The usefulness of Bartlett's covariance analysis with missing yields, the relationship of the method to the analysis of variance with estimated yields for missing data, and the use of the method in the analysis of variance with unequal numbers are illustrated in the following example.

Example XVI-2. A set of artificial data was constructed for ease of computation. The design is a randomized complete block with three treatments. The yield for treat-

TABLE XVI-17. Data for 3 replicates of a randomized complete block design with 3 treatments and one missing yield

Treatment	Replicate I		Replicate II		Replicate III		Total		Mean	
	Y	X	Y	X	Y	X	Y	X	Y	X
A	6	0	5	0	4	0	15	0	5	0
B	15	0	10	0	8	0	33	0	11	0
C	15	0	15	0	(0)	1	30	1	15	1/3
Total	36	0	30	0	12	1	78	1	-	-
Mean	12	0	10	0	6	1/3	-	-	26/3	1/9

TABLE XVI-18. Covariance analysis for data in table XVI-17

Source of variation	Sum of products: df	y^2	xy	x^2	Adjusted results: df	ss	ms
Total	8	240	-26/3	8/9	7	155.5	-
Replicate	2	104	-14/3	2/9	-	-	-
Treatment	2	62	4/3	2/9	-	-	-
Error	4	74	-16/3	4/9	3	10	10/3
Error + treatment	6	136	-12/3	6/9	5	112	-
Treatment adjusted for covariate					2	102	51
Error + replicate	6	178	-30/3	6/9	5	28	-
Replicate adjusted for covariate					2	18	9

ment C in replicate III is missing. A zero is inserted for the missing yield, and the covariate composed of zeros and a one is paired with the Y variate as shown in table XVI-17. The analysis of covariance for these data is given in table XVI-18.

The estimated value for the missing observation obtained from formula (V-5) is equal to

$$\frac{3(30) + 3(12) - 78}{(3 - 1)(3 - 1)} \quad 12.$$

This value may be obtained from formula (XVI-60); thus, $-(-16/3)/4/9 = 12$. The estimated value for the missing yield is inserted in the table of yields, and the analysis of variance is then computed (table XVI-19). The error sums of squares in table XVI-18 and XVI-19 are identical, as they should be. As stated in Chapter V the sums of squares for treatments and replicates are overestimated. The correct replicate and treatment sums of squares are given in table XVI-18, and these are somewhat smaller than the comparable ones given in table XVI-19.

TABLE XVI-19. Analysis of variance for the variate Y after estimating the yield for the missing item

Source of variation	df	ss
Total	7	160
Replicate	2	24
Treatment	2	126
Error	3	10

If the procedure for obtaining the correction for disproportion as described by Snedecor [273, Ch. 11] is followed, the correct sums of squares for treatments and for replicates are obtained. First compute the sum of squares for the effects weighted by the number of observations (table XVI-20). For example, the sum of squares for treatments is equal to

$$\frac{15^2}{3} + \frac{33^2}{3} + \frac{30^2}{2} - \frac{78^2}{8} \quad 127.5.$$

The residual sum of squares is equal to

$$155.5 - 43.5 - 127.5 = -15.5.$$

The correction for disproportion is equal to

error sum of squares − residual sum of squares $\quad 10 - (-15.5) = 25.5.$

The correction is added to the residual sum of squares and subtracted from the replicate and treatment sums of squares in table XVI-20 to obtain the correct sums of squares (tables XVI-18 and XVI-21). The total sum of squares in tables XVI-20 and

XVI-21 is equal to the replicate sum of squares plus the within-replicate (treatment (eliminating replicate effect) and error sums of squares) sum of squares.

TABLE XVI-20. Analysis of variance on yields of the variate weighted by number of observations

Source of variation	df	ss
Total	7	155.5
Replicate	2	43.5
Treatment	2	127.5
Residual	3	-15.5 (by subtraction)

TABLE XVI-21. Sums of squares in table XVI-20 corrected for disproportion (correction for disproportion = 25.5)

Source of variation	df	ss
Total	7	155.5 = 43.5 + 102 + 10
Replicate	2	18 = 43.5 - 25.5
Treatment	2	102 = 127.5 - 25.5
Error	3	10 = -15.5 + 25.5

XVI-10.2 COVARIANCE ON DUMMY VARIATES FOR NON-ORTHOGONAL TWO-WAY CLASSIFICATIONS

Quenouille [251] has extended Bartlett's [14] results to the analysis of a $2 \times s$ table with unequal numbers in the $2 \times s$ subclasses. He states that the method may be substituted for the ordinary method of analysis [273, Ch. 11] when the deviations from orthogonality are small. The short-cut method described by Snedecor [273, Ch. 11] is as simple as Quenouille's method, but the method presented by the latter author [251] is described here for completeness.

Quenouille's [251] method of analysis for a non-orthogonal $2 \times s$ classification involves the use of a covariate composed of zeros and ones. In the classification composed of two categories a one is assigned to every individual in one category and a zero is assigned to every individual in the second category. The resulting subclass totals and number of individuals per subclass are

A category	B category										Total		
	b_1			b_2				b_s					
	No.	Y	X	No.	Y	X	$\cdots$	No.	Y	X	No.	Y	X
a_1	n_{11}	$Y_{11.}$	n_{11}	n_{12}	$Y_{12.}$	n_{12}		n_{1s}	$Y_{1s.}$	n_{1s}	$n_{1.}$	$Y_{1..}$	$n_{1.}$
a_2	n_{21}	$Y_{21.}$	0	n_{22}	$Y_{22.}$	0		n_{2s}	$Y_{2s.}$	0	$n_{2.}$	$Y_{2..}$	0
Total	$n_{.1}$	$Y_{.1.}$	n_{11}	$n_{.2}$	$Y_{.2.}$	n_{12}	$\cdots$	$n_{.s}$	$Y_{.s.}$	n_{1s}	n	$Y_{...}$	$n_{1.}$

The analysis of variance for these data is presented in table XVI-22. In the analysis of variance for disproportionate numbers in a $2 \times s$ table Quenouille's method results in the same values (within rounding errors) obtained by the method described by Snedecor [273, Ch. 11].

TABLE XVI-22. Analysis for a $2 \times s$ table with unequal numbers in the subclasses

Covariance analysis

Source of variation	Sum of products				Adjusted ss	
	df	y^2	xy	x^2	df	ss
Total	n-1	T_{yy}	T_{xy}	T_{xx}	n-2	$T_{yy}' = T_{yy} - T_{xy}^2/T_{xx}$
B	s-1	B_{yy}	B_{xy}	B_{xx}	-	-
Residual	n-s	R_{yy}	R_{xy}	R_{xx}	n-s-1	$R_{yy}' = R_{yy} - R_{xy}^2/R_{xx}$
B (eliminating A effects)					s-1	$B_{yy}' = T_{yy}' - R_{yy}'$

Completed analysis

Source of variation	df	ss
B (eliminating A)	s-1	B_{yy}'
A (eliminating B)	1	$R_{xy}^2/R_{xx} = A_{yy}'$
Within subclasses	$\Sigma n_{ij} - 2s$	$\sum_{ij}\sum\left[\sum_h Y_{ijh}^2 - Y_{ij.}^2/n_{ij}\right] = W_{yy}$
Among subclasses	2s-1	$\Sigma Y_{ij.}^2/n_{ij} - Y_{...}^2/n = T_{yy}$
A x B (eliminating A and B)	s-1	$T_{yy} - B_{yy} - A_{yy}' = R_{yy}' - W_{yy}$

XVI-10.3 COVARIANCE ANALYSIS OF *n*-WAY CLASSIFICATIONS WITH UNEQUAL NUMBERS IN THE SUBCLASSES

XVI-10.3.1 The randomized complete block design with one pair of values for each experimental unit.

XVI-10.3.1 The randomized complete block design with one pair of values for each experimental unit. The simplest form of a two-way classification with disproportionate numbers in the subclasses is represented by a randomized complete block design with one missing observation. The missing observation may be for the variate Y or for the covariate X[1], or the given pair of values may be missing.

If a value for the variate, say Y_{ij}, is missing in a randomized complete block design, Bartlett [13] states that the corresponding value of the covariate, say X_{ij}, should be discarded. The estimated values for Y_{ij} and for X_{ij}, say $\hat{Y}_{ij}$ and $\hat{X}_{ij}$, are computed using the ordinary formula (formula (V-5)) for estimating the value for a missing observation in a randomized complete block design. The number of degrees of freedom associated with E_{xx}, E_{xy}, and E_{yy} (table XVI-3) is equal to $(r - 1)(v - 1) - 1$. The analysis of covariance now proceeds in the usual manner except that E_{yy}' is associated

[1]The covariate here is not a dummy variate as in sections XVI-10.1 and XVI-10.2.

with $(r-1)(v-1)-2$ instead of $(r-1)(v-1)-1$ degrees of freedom. The adjusted mean is computed from formula (XVI-9), using only the actual values obtained for the Y variate and only the values of the X covariate which are associated with the Y values. The same procedure is followed if both members of a pair are missing. The reason for estimating both members of the pair of values for which the Y value is missing and for which the value of the covariate X is not missing is that there should be no contribution to the regression from the missing plot when the final yields, Y, are adjusted for covariation in X. The fact that the value of the covariate is available is of no use in obtaining an adjusted mean. Only those values of X which are associated with Y values are utilized in obtaining the adjusted treatment mean.

If, however, the X and Y values are considered as coming from a bivariate distribution, the interest lies in the means, $\bar{y}$ and $\bar{x}$, the variances, and the covariances for the two variates. In this case, there is a loss in information if one of the values is discarded simply because the other member of the pair was inadvertently lost. If the value of Y is available but the associated X value is missing, it appears that the above procedure [13] is not satisfactory, since it does not utilize all the data. This is especially true for the smaller experiments. A suggested procedure in this case is to estimate the missing value for the covariate by formula (V-5). This value is inserted in the table of yields and the analysis of covariance is computed in accordance with the procedure outlined in table XVI-3. The adjusted mean for the ith treatment is obtained using all r values of Y in computing $\bar{y}_{i.}$ and the $r-1$ X values associated with the Y values in computing $\bar{x}_{i.}$. The quantity E_{yy}' is associated with $(r-1)(v-1)-1$ degrees of freedom; i.e., no reduction is made for estimating the value of X. However, this should be accounted for in computing the variance of the adjusted treatment mean.

An alternative procedure would be to estimate the missing value for the covariate subject to the restriction that E_{yy}' should be a minimum. This would necessitate the subtraction of one degree of freedom from E_{yy}'. The value for the missing covariate, say $\hat{X}_{11}$, is obtained from the formula,

$$\frac{\dfrac{(r-1)(v-1)}{rv}\hat{X}_{11}-\bar{x}_{1.}-\bar{x}_{.1}+\bar{x}}{(Y_{11}-\bar{y}_{1.}-\bar{y}_{.1}+\bar{y})}=\frac{E_{xx}}{E_{xy}}=\frac{1}{b}. \qquad \text{(XVI-61)}$$

Since the quantity $\hat{X}_{11}$ appears in E_{xx} and E_{xy}, an iterative solution is suggested, since it was not possible to solve for $\hat{X}_{11}$ explicitly. A first guess for $\hat{X}_{11}$ would be obtained from equation (V-5).

If one Y value, say Y_{11}, is missing and if it is considered that the associated X value, say X_{11}, should be retained for some special reason, the estimated missing value is obtained from the formula,

$$\hat{Y}_{11}=\frac{vY_{1.}+rY_{.1}-Y_{..}}{(r-1)(v-1)}+\frac{rv(X_{11}-\bar{x}_{1.}-\bar{x}_{.1}+\bar{x})E_{xy}}{2(r-1)(v-1)E_{xx}}, \qquad \text{(XVI-62)}$$

where

$$E_{xy} = \hat{Y}_{11}X_{11} + \sum_{ij \neq 11} X_{ij}Y_{ij} - \frac{(\hat{Y}_{11} + Y_{1.})X_{1.} + \sum_{2}^{v} X_{i.}Y_{i.}}{r}$$

$$- \frac{(\hat{Y}_{11} + Y_{.1})X_{.1} + \sum_{2}^{r} X_{.j}Y_{.j}}{v} + \frac{(\hat{Y}_{11} + Y_{..})X_{..}}{rv} \qquad \text{(XVI-63)}$$

The above formula was obtained by differentiating E_{yy}' with respect to $\hat{Y}_{11}$, setting the result equal to zero, and solving for $\hat{Y}_{11}$. In this case, E_{yy}' is associated with $(r - 1)(v - 1) - 2$ degrees of freedom, since one value was estimated.

The covariance analysis for more than one missing value or pair of values follows that described for estimating values for more than one missing plot. A similar method could be developed for the covariance analysis for data in which one or more of the yields are mixed up for either the variate or the covariate, or both.

XVI-10.3.2. The randomized complete block design with n_{ij} pairs of observations per experimental unit.[1] Covariance analyses for two-way classifications (such as the randomized complete block design, or a factorial arrangement of two factors), with unequal numbers of observations per experimental unit or per subclass may be classified as follows:

Case I—Interaction absent.

Case II—Interaction present, and the interaction effects assumed to be fixed effects.

Case III—Interaction present, and the interaction effects assumed to be random variates.

The linear model for Case I is

$$Y_{ijh} = \mu + \tau_i + \rho_j + \beta(X_{ijh} - \bar{x}) + \epsilon_{ijh}, \qquad \text{(XVI-64)}$$

where μ, τ_i, ρ_j, β, and ϵ_{ijh} represent the mean effect, the first factor (treatment) effect, the second factor (replicate) effect, the average regression of Y on X in the error line, and the random error component, respectively. The sum of squares to be minimized is

$$\sum_{i=1}^{v}\sum_{j=1}^{r_i}\sum_{h=1}^{n_{ij}} [Y_{ijh} - \mu - \tau_i - \rho_j - \beta(X_{ijh} - \bar{x})]^2. \qquad \text{(XVI-65)}$$

Differentiation of (XVI-65) with respect to μ, to τ_i, to ρ_j, and to β yields a set of $r + v + 2$ equations. These equations plus the following equations:

$$\sum_{i=1}^{v} \hat{\tau}_i = 0 = \sum_{j=1}^{r} \hat{\rho}_j, \qquad \text{(XVI-66)}$$

[1]The resulting equations and estimates for effects have been obtained for all three cases, and applications of the results have been made (Unpublished results).

yield the least squares estimates, $\hat{\mu}$, $\hat{\tau}_i$, $\hat{\rho}_j$, and $\hat{\beta}$, of the effects μ, τ_i, ρ_j, and β, respectively. Solution of the equations is possible for $n_{ij} = 0, 1, 2, \cdots$. The procedure used here follows that for the variance analysis by the "method of fitting constants" as described by Yates [318a], Snedecor [273, sec. 11.12], Hazel [152], and Henderson [156].

The linear model for Case II is

$$Y_{ijk} = \mu + \tau_i + \rho_j + \rho\tau_{ij} + \beta(X_{ijk} - \bar{x}) + \epsilon_{ijk}, \qquad \text{(XVI-67)}$$

where $\rho\tau_{ij}$ represents the interaction effect of the two factors and is assumed to be a fixed effect and the other effects are defined in equation (XVI-64). The sum of squares to be minimized is

$$\sum_{i=1}^{v} \sum_{j=1}^{r_i} \sum_{k=1}^{n_{ij}} [Y_{ijk} - \mu - \tau_i - \rho_j - \rho\tau_{ij} - \beta(X_{ijk} - \bar{x})]^2. \quad \text{(XVI-68)}$$

The resulting $rv + r + v + 2$ equations and the following equations yield the least squares estimates of the effects:

$$\sum_{i=1}^{v} \hat{\tau}_i = \sum_{j=1}^{r} \hat{\rho}_j = \sum_{i=1}^{v} \widehat{\rho\tau}_{ij} = \sum_{j=1}^{r} \widehat{\rho\tau}_{ij} = 0. \qquad \text{(XVI-69)}$$

No solution is possible if one or more of the $n_{ij} = 0$. An approximate solution may be obtained by assuming that the interaction effect is zero in any subclass for which $n_{ij} = 0$; this results in the following assumptions [155]:

$$\sum_{i=1}^{v_j} \widehat{\rho\tau}_{ij} = \sum_{j=1}^{r_i} \widehat{\rho\tau}_{ij} = 0. \qquad \text{(XVI-70)}$$

The closeness of the above approximation depends upon the number of $n_{ij} = 0$ and upon the size of the interaction effects. For Case II the variance analysis is known as the "weighted squares of means" analysis [273, sec. 11.12; 318a].

The linear model for Case III is

$$Y_{ijk} = \mu + \tau_i + \rho_j + \delta_{ij} + \beta_1(\bar{x}_{ij.} - \bar{x}) + \beta_2(X_{ijk} - \bar{x}_{ij.}) + \epsilon_{ijk}, \quad \text{(XVI-71)}$$

where δ_{ij} represents a random error component associated with the ijth subclass or experimental unit, β_1 represents the regression of Y on X in the error or interaction line, β_2 represents the regression of Y on X within subclasses, and the other effects are defined in formula (XVI-64). The least squares estimate of β_2 is

$$\hat{\beta}_2 = \sum_{i=1}^{v} \sum_{j=1}^{r_i} \sum_{k=1}^{n_{ij}} Y_{ijk}(X_{ijk} - \bar{x}_{ij.}) / \sum\sum\sum(X_{ijk} - \bar{x}_{ij.})^2. \quad \text{(XVI-72)}$$

The least squares estimates of μ, τ_i, ρ_j, and β_1 may be obtained by differentiating the following sum of squares, by equating the results to zero, and by imposing the restrictions that $\sum \hat{\tau}_i = 0 = \sum \hat{\rho}_j$:

$$\sum\sum w_{ij}[\bar{y}_{ij.} - \mu - \tau_i - \rho_j - \beta_1(\bar{x}_{ij.} - \bar{x})]^2. \qquad \text{(XVI-73)}$$

The value of the weights w_{ij} will depend upon the assumptions made. The true variance of a subclass mean $\bar{y}_{ij.}$ is $\sigma_\delta^2 + \sigma_\epsilon^2/n_{ij}$. If the subclass means are weighted inversely to the variance with which they are estimated, then $w_{ij} = 1/(\sigma_\delta^2 + \sigma_\epsilon^2/n_{ij}) = n_{ij}/(n_{ij}\sigma_\delta^2 + \sigma_\epsilon^2)$. Now, if σ_δ^2 is small relative to σ_ϵ^2/n_{ij}, then w_{ij} is essentially n_{ij}. If, on the other hand, σ_δ^2 is large compared to σ_ϵ^2/n_{ij}, then w_{ij} is essentially equal to one. However, the true situation is usually somewhere between these two limiting situations, and no solution is possible unless σ_ϵ^2 and σ_δ^2 (or $\sigma_\epsilon^2/\sigma_\delta^2$) are known. An approximate solution is possible if the estimates $\hat{\sigma}_\delta^2$ and $\hat{\sigma}_\epsilon^2$ from the experiment are used for the population variances of σ_δ^2 and σ_ϵ^2. This approximation is not too bad if sufficient degrees of freedom (say greater than 20 to 30) are available for the within-subclasses and for the error sums of squares. Also, if any $n_{ij} = 0$, the corresponding $w_{ij} = n_{ij}/(n_{ij}\sigma_\delta^2 + \sigma_\epsilon^2) = 0$.

XVI-10.3.3 n-Way classifications and multiple covariance. Hazel [152] illustrates the method for a multiple covariance analysis in a two-way classification with unequal numbers in the subclasses. His results, plus those in the preceding section, may be utilized in setting up the analytical procedure for more complex situations.

XVI-11 Covariance Versus Stratification

The discussion in the preceding sections pertains to covariance analyses for the various designs. Stratification of the experimental material was used to control the variability among the various complete blocks, incomplete blocks, rows, and columns. For certain types of experimental material, it is possible to obtain a measure of the differences, other than treatment differences, among the experimental units. These measurements may then be used as the values for the covariate, and stratification may be dispensed with. For example, it may be possible to obtain initial weights of animals going into a feeding experiment. With these initial weights the animals may be grouped into complete blocks or into incomplete blocks in such a way that the weights of the animals within the blocks are nearly equal. On the other hand, the animals could be randomly assigned to the various treatments, and a covariance analysis of final weights on initial weights could be computed. A third possibility would be to stratify the animals into relatively homogeneous groups and to obtain initial weights for a covariance analysis in order to control any residual variation caused by differences in initial weights. The joint use of a covariance analysis and stratification may be indicated for certain types of experiments [60, sec. 3.8; 127, sec. 49.1; 247; 273, Ch. 12]. Peterson *et al.* [247; see example VI-1] give an example illustrating the joint use of covariance and stratification to control the same variable, size of leaf. The use of covariance on leaf size alone would have been more efficient in this experiment than stratification either alone or with covariance.

Fisher [127, sec. 48], Quenouille [251], and Federer and Schlottfeldt [115] discuss the use of covariance to control variability in experiments where a gradient exists. A linear or curvilinear gradient may exist in the following types of experiments [115]:

(i) greenhouse experiments where the source of heat is located in the center or on the sides of the house,
(ii) field experiments located in areas containing drainage tiles or ditches,
(iii) field experiments containing a depression in the center of the replicates,
(iv) orchard or vineyard experiments on undulating topography,
(v) animal experiments with animals located at varying distances from the source of heat, food, water, etc.,
(vi) experiments in which the yields are affected by slowly migrating insects entering the area from one side,
(vii) and other experiments.

If the direction and shape of the gradient are known, the covariate may be assigned to the various experimental units and a covariance analysis computed. For example, if the gradient is linear, the sequence of numbers 1, 2, 3, $\cdots$, v may be assigned to the various units. These numbers represent the values of the covariate. If the gradient is quadratic, one of the covariates consists of the above sequence of numbers and the second covariate consists of the squares of the numbers. For more complex gradients, other functions of the numbers are necessary.

XVI-12 Expectation of Mean Squares

There are even more unsolved problems associated with variance component analyses involving a covariate than in the ordinary variance component analyses. Much theoretical work needs to be done before all problems are solved. However, enough theory is available for estimating variance components from the various adjusted mean squares.

XVI-12.1 COMPLETELY RANDOMIZED DESIGN

Variance components in a covariance analysis[1] have been discussed by Cochran [53] and Crump [80] for the completely randomized design. The estimate for σ_ϵ^2 is simply the adjusted mean square for error, $E_{yy}'/(r_{..} - v - 1)$. Two estimates of the treatment variance component are available from an analysis such as that given in table XVI-1. One estimate is obtained from the following combination of mean squares:

$$\frac{1}{k}\left\{\frac{G_{yy} - G_{xy}^2/G_{xx}}{v - 2} - \frac{E_{yy}'}{r_{..} - v - 1}\right\}. \tag{XVI-74}$$

[1]This is not to be confused with covariance components [156] which refer to the covariance of effects; e.g., $E[\tau_i \rho_j] = \sigma_{\tau\rho}$.

When $r_i = r$ for all i, $k = r$, and the expectation of equation (XVI-74) is

$$\frac{1}{r}\{\sigma_\epsilon^2 + r\sigma_\tau^2 - \sigma_\epsilon^2\} = \sigma_\tau^2. \qquad \text{(XVI-75)}$$

The second estimate of σ_τ^2 may be obtained from a combination of the mean squares obtained by dividing G_{yy}' and E_{yy}' by the appropriate degrees of freedom. The expectation of G_{yy}' is equal to

$$(v-1)\sigma_\epsilon^2 + r\sigma_\tau^2\left\{v - 1 - \frac{G_{xx}}{T_{xx}}\right\} = (v-1)\left\{\sigma_\epsilon^2 + \frac{r\sigma_\tau^2}{v-1}\left(v - 2 + \frac{E_{xx}}{T_{xx}}\right)\right\}. \qquad \text{(XVI-76)}$$

The estimate of σ_τ^2 is obtained as

$$\hat{\sigma}_\tau^2 = \frac{v-1}{r(v - 2 + E_{xx}/T_{xx})}\left\{G_{yy}'/(v-1) - \frac{E_{yy}'}{rv - v - 1}\right\}. \qquad \text{(XVI-77)}$$

In some instances one estimate has a smaller variance than the second estimate while in other cases the reverse is true [53, 80].

XVI-12.2 RANDOMIZED COMPLETE BLOCK DESIGN

Suppose that the linear model given by equation (XVI-13) is the appropriate one for the randomized complete block design. Furthermore, in order to simplify the problem, assume that the τ_i, ρ_j, and ϵ_{ij} are random independent variables. Then, the expectation of E_{yy}' is equal to (table XVI-3)

$$\begin{aligned}
E[E_{yy}'] &= E\left\{\sum_i\sum_j Y_{ij}^2 - \frac{\sum Y_{i.}^2}{r} - \frac{\sum Y_{.j}^2}{v} + \frac{Y_{..}^2}{rv}\right. \\
&\quad \left. - \frac{\left\{\sum_i\sum_j Y_{ij}(X_{ij} - \bar{x}_{i.} - \bar{x}_{.j} + \bar{x})\right\}^2}{\sum_i\sum_j (X_{ij} - \bar{x}_{i.} - \bar{x}_{.j} + \bar{x})^2 = E_{xx}}\right\} \\
&= rv(\mu^2 + \sigma_\tau^2 + \sigma_\rho^2 + \sigma_\epsilon^2) + \beta^2 S_{xx} \\
&\quad - (rv\mu^2 + rv\sigma_\tau^2 + v\sigma_\rho^2 + v\sigma_\epsilon^2 + \beta^2 T_{xx}) \\
&\quad - (rv\mu^2 + rv\sigma_\rho^2 + r\sigma_\tau^2 + r\sigma_\epsilon^2 + \beta^2 R_{xx}) + (rv\mu^2 + v\sigma_\rho^2 + r\sigma_\tau^2 + \sigma_\epsilon^2) \\
&\quad - \frac{1}{E_{xx}} E\left\{\sum\sum[\mu + \tau_i + \rho_j + \beta(X_{ij} - \bar{x}) + \epsilon_{ij}][X_{ij} - \bar{x}_{i.} - \bar{x}_{.j} + \bar{x}]\right\}^2 \\
&= (r-1)(v-1)\sigma_\epsilon^2 + E_{xx}\beta^2 - \{0 + 0 + 0 + \beta^2 E_{xx} + \sigma_\epsilon^2\} \\
&= [(r-1)(v-1) - 1]\sigma_\epsilon^2, \qquad \text{(XVI-78)}
\end{aligned}$$

where $S_{xx} = R_{xx} + T_{xx} + E_{xx}$.

The expectation of the treatment (adjusted for error regression) mean square is

$$E[T_{yy}'] = E\left\{\sum_i\sum_j\left(Y_{ij}^2 - \frac{Y_{.j}^2}{v}\right) - \frac{[\sum\sum Y_{ij}(X_{ij} - \bar{x}_{.j})]^2}{T_{xx} + E_{xx}}\right\} - E[E_{yy}']$$

$$= E\left\{T_{yy} - \frac{(T_{xy} + E_{xy})^2}{T_{xx} + E_{xx}} + \frac{E_{xy}^2}{E_{xx}}\right.$$

$$= (v-1)(\sigma_\epsilon^2 + r\sigma_\tau^2) + \beta^2 T_{xx} - \left(\frac{T_{xx} r\sigma_\tau^2}{T_{xx} + E_{xx}} + \sigma_\epsilon^2 + \beta^2(T_{xx} + E_{xx})\right)$$
$$+ (\sigma_\epsilon^2 + \beta^2 E_{xx})$$

$$= (v-1)\left\{\sigma_\epsilon^2 + \frac{r\sigma_\tau^2}{v-1}\left(v - 1 - \frac{T_{xx}}{T_{xx} + E_{xx}}\right)\right\}, \quad \text{(XVI-79)}$$

with $v - 1$ degrees of freedom.

The estimate of the treatment variance component from the mean squares T_{yy}' and E_{yy}' is obtained as follows:

$$\hat{\sigma}_\tau^2 = \frac{v-1}{r[v - 1 - T_{xx}/(T_{xx} + E_{xx})]}\left\{\frac{T_{yy}'}{v-1} - \frac{E_{yy}'}{rv - r - v}\right\}. \quad \text{(XVI-80)}$$

Likewise, an estimate of σ_τ^2 may be obtained from the mean square for treatments adjusted for the treatment regression; thus:

$$\hat{\sigma}_\tau^2 = \frac{1}{r}\left\{\frac{T_{yy} - T_{xy}^2/T_{xx}}{v-2} - \frac{E_{yy}'}{rv - r - v}\right\}. \quad \text{(XVI-81)}$$

The expectation of the other sums of squares in the analysis of covariance and of sums of squares with more than one sample or individual per experimental unit may be obtained in a similar manner [152, 156].

XVI-12.3 DOUBLE LATTICE DESIGN

The linear model for one set of the double lattice design with a covariate is

$$\left.\begin{aligned} Y_{1ij} &= \mu + \rho_1 + \beta_{1i} + \tau_{ij} + \epsilon_{1ij} + \beta_1(\bar{x}_{1i.} - \bar{x}) + \beta_2(X_{1ij} - \bar{x}_{1i.}) \\ Y_{2ij} &= \mu + \rho_2 + \beta_{2j} + \tau_{ij} + \epsilon_{2ij} + \beta_1(\bar{x}_{2.j} - \bar{x}) + \beta_2(X_{2ij} - \bar{x}_{2.j}) \end{aligned}\right\}, \quad \text{(XVI-82)}$$

where the effects are defined in sections XI-8 and XVI-7, β_1 is the block (eliminating treatment) regression coefficient, and β_2 is the intrablock error regression coefficient. In the above set of equations, it should be noted that the β_{1i} and β_{2j} are not regression coefficients in the same sense that β_1 and β_2 are. The expected value of $\beta_1^2 = \beta_1^2$, and of $\beta_2^2 = \beta_2^2$, but the expected value of β_{1i}^2 and of $\beta_{2j}^2 = \sigma_\beta^2$.

If $\beta_1 = \beta_2 = \beta$, the above equations reduce to

$$\left.\begin{aligned} Y_{1ij} &= \mu + \rho_1 + \beta_{1i} + \tau_{ij} + \epsilon_{1ij} + \beta(X_{1ij} - \bar{x}) \\ Y_{2ij} &= \mu + \rho_2 + \beta_{2j} + \tau_{ij} + \epsilon_{2ij} + \beta(X_{2ij} - \bar{x}) \end{aligned}\right\}. \tag{XVI-83}$$

In obtaining the expectations for the various sums of products given in table XVI-12, it is assumed that formula (XVI-83) is the appropriate linear model for a double lattice design with a covariate. The expectation of B_{yy} is

$$\begin{aligned} E[B_{yy}] &= \frac{k}{2}E\left\{\sum_i(\bar{y}_{2i\cdot} - \bar{y}_{1i\cdot} - \bar{y}_{2\cdot\cdot} + \bar{y}_{1\cdot\cdot})^2 \right. \\ &\quad \left. + \sum_j(\bar{y}_{1\cdot j} - \bar{y}_{2\cdot j} - \bar{y}_{1\cdot\cdot} + \bar{y}_{2\cdot\cdot})^2\right\} \\ &= 2(k-1)\sigma_\epsilon^2 + k(k-1)\sigma_\beta^2 + \beta^2 B_{xx}. \end{aligned} \tag{XVI-84}$$

The expectation of B_{yy}^* is equal to

$$\begin{aligned} E\left\{B_{yy} - \frac{B_{xy}^2}{B_{xx}}\right\} &= E[B_{yy}] - \frac{1}{4B_{xx}}E\left\{\sum_i(Y_{2i\cdot} - Y_{1i\cdot})(\bar{x}_{2i\cdot} - \bar{x}_{1i\cdot} - \bar{x}_{2\cdot\cdot} + \bar{x}_{1\cdot\cdot})\right. \\ &\quad \left. + \sum_j(Y_{1\cdot j} - Y_{2\cdot j})(\bar{x}_{1\cdot j} - \bar{x}_{2\cdot j} - \bar{x}_{1\cdot\cdot} + \bar{x}_{2\cdot\cdot})\right\}^2 \\ &= 2(k-1)\sigma_\epsilon^2 + k(k-1)\sigma_\beta^2 + \beta^2 B_{xx} - \frac{k}{2}\sigma_\beta^2 - \sigma_\epsilon^2 - \beta^2 B_{xx} \\ &= (2k-3)(\sigma_\epsilon^2 + k\sigma_\beta^2/2), \end{aligned} \tag{XVI-85}$$

with $2k - 3$ degrees of freedom.

The expected value of the block (eliminating treatment) sum of squares adjusted for intrablock regression, B_{yy}', is equal to

$$\begin{aligned} &E\left\{B_{yy} - \frac{(B_{xy} + E_{xy})^2}{B_{xx} + E_{xx}} + \frac{E_{xy}^2}{E_{xx}}\right\} = E[B_{yy}] \\ &- \frac{1}{B_{xx} + E_{xx}}E\left\{\sum_i\sum_j[Y_{1ij}(X_{1ij} - \bar{x}_{1\cdot\cdot} - \bar{x}_{\cdot ij} + \bar{x}) \right. \\ &\left. + Y_{2ij}(X_{2ij} - \bar{x}_{2\cdot\cdot} - \bar{x}_{\cdot ij} + \bar{x})]\right\}^2 + \frac{1}{E_{xx}}E\left\{\sum_i\sum_j Y_{1ij}[X_{1ij} - \bar{x}_{1\cdot\cdot} - \bar{x}_{\cdot ij} + \bar{x} \right. \\ &- \tfrac{1}{2}(\bar{x}_{2i\cdot} - \bar{x}_{1i\cdot} - \bar{x}_{2\cdot\cdot} + \bar{x}_{1\cdot\cdot})] + \sum_i\sum_j Y_{2ij}[X_{2ij} - \bar{x}_{2\cdot\cdot} - \bar{x}_{\cdot ij} + \bar{x} \\ &\left. - \tfrac{1}{2}(\bar{x}_{1\cdot j} - \bar{x}_{2\cdot j} - \bar{x}_{1\cdot\cdot} + \bar{x}_{2\cdot\cdot})]\right\}^2 \\ &= 2(k-1)\sigma_\epsilon^2 + k(k-1)\sigma_\beta^2 + \beta^2 B_{xx} \\ &- \left(\sigma_\epsilon^2 + \frac{kB_{xx}}{2(B_{xx} + E_{xx})}\sigma_\beta^2 + \beta^2(B_{xx} + E_{xx})\right) + (\sigma_\epsilon^2 + \beta^2 E_{xx}) \\ &= 2(k-1)\left\{\sigma_\epsilon^2 + \frac{k\sigma_\beta^2}{2(k-1)}\left(k - 1 - \frac{B_{xx}}{2(B_{xx} + E_{xx})}\right)\right\}, \end{aligned} \tag{XVI-86}$$

with $2(k-1)$ degrees of freedom.

The expectation of the intrablock error sum of squares adjusted for covariation in X is

$$E[E_{yy}'] = E\{E_{yy} - E_{xy}^2/E_{xx}\}$$

$$= E\Big\{\sum_i\sum_j[Y_{1ij} - \bar{y}_{1\cdot\cdot} - \bar{y}_{\cdot ij} + \bar{y} - \tfrac{1}{2}(\bar{y}_{2i\cdot} - \bar{y}_{1i\cdot} - \bar{y}_{2\cdot\cdot} + \bar{y}_{1\cdot\cdot})]^2$$

$$+ \sum_i\sum_j[Y_{2ij} - \bar{y}_{2\cdot\cdot} - \bar{y}_{\cdot ij} + \bar{y} - \tfrac{1}{2}(\bar{y}_{1\cdot j} - \bar{y}_{2\cdot j} - \bar{y}_{1\cdot\cdot} + \bar{y}_{2\cdot\cdot})]^2\Big\}$$

$$- \frac{E}{E_{xx}}\{E_{xy}^2\} = (k-1)^2\sigma_\epsilon^2 + \beta^2E_{xx} - (\sigma_\epsilon^2 + \beta^2E_{xx}) = k(k-2)\sigma_\epsilon^2, \quad \text{(XVI-87)}$$

with $k(k-2)$ degrees of freedom.

From equations (XVI-86) and (XVI-87) the estimated amount of interblock information is equal to

$$w' = \frac{1 - B_{xx}/2(k-1)(B_{xx} + E_{xx})}{[B_{yy}'/(k-1)] - [E_{yy}'/k(k-1)][1 + B_{xx}/2(k-1)(B_{xx} + E_{xx})]}, \quad \text{(XVI-88)}$$

and the estimate of interblock information is

$$w = \frac{k(k-2)}{E_{yy}'}. \quad \text{(XVI-89)}$$

Formula (XVI-88) results in a different value for w' than that obtained from formula (XVI-35). However, the difference will not be large if k is greater than 4 or 5 and if $B_{xx}/2(k-1)$ is not radically different from $E_{xx}/(k-1)^2$. As an approximation, it would appear that formula (XVI-35) is suitable for estimating w' using B_{yy}' and E_{yy}'. If B_{yy}^* and E_{yy}' are used to obtain the estimate w', then formula (XVI-34) gives the correct value.

If the two regression coefficients, β_1 and β_2, in equation (XVI-82) are not equal, then the expectations of the mean squares may be obtained in a manner similar to that described above, and the weights may be estimated accordingly.

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PROBLEMS

CHAPTER IV - THE COMPLETELY RANDOMIZED DESIGN

PROBLEMS

1. Fifty-four plants are selected at random from the area planted to variety 109. These are the same plants on which dry weight of shrub was obtained in table IV-2. The character rubber percentage was obtained on the individual plants. The data are:

Plant No.	Normals	Offtypes	Aberrants
1	6.97	6.21	4.28
2	7.11	5.70	7.71
3	7.26	6.04	6.48
4	6.80	4.47	7.71
5	7.01	5.22	7.37
6	7.00	5.55	7.20
7	6.35	4.45	7.06
8	6.37	4.84	6.40
9	7.29	5.88	8.93
10	7.31	5.82	5.91
11	6.86	6.09	5.51
12	6.81	5.59	6.36
13	6.43	6.06	
14	7.43	5.59	
15	6.68	6.74	
16	7.29		
17	7.12		
18	6.68		
19	7.34		
20	5.15		
21	6.41		
22	6.45		
23	6.32		
24	6.82		
25	6.86		
26	6.48		
27	7.28		
ΣY_i	183.88	84.25	80.92
ΣY_i^2	1258.36	478.98	561.64
$\Sigma X_i Y_i$	21046.54	8267.75	2099.80

(i) Test the mean differences of normals and offtypes and of offtypes and aberrants by t-test.

(ii) Are the variances homogeneous?

(iii) Run a covariance analysis of rubber percentage (Y) on dry weight (X) of shrub.

(iv) Does the regression of the means differ from the average within-type of plant regression?

(v) Do the within-type regressions differ from the average within regression?

(vi) Is the relative variation among dry weights of shrubs significantly greater than that among rubber percentages for aberrants?

2. The 54 plants of variety 109 were analyzed for resin percentages. The data follow:

Plant No.	Normals	Offtypes	Aberrants
1	5.71	6.17	3.97
2	6.15	6.04	6.65
3	6.05	5.89	5.44
4	5.64	5.91	7.20
5	3.85	5.22	6.52
6	5.62	5.75	6.51
7	5.60	5.38	5.92
8	5.00	5.99	6.81
9	6.06	5.44	7.34
10	6.05	5.88	5.55
11	5.24	6.13	5.22
12	5.66	5.83	5.95
13	5.53	5.88	
14	6.25	6.34	
15	6.06	5.83	
16	6.10		
17	6.07		
18	7.13		
19	6.53		
20	5.83		
21	5.85		
22	5.67		
23	6.01		
24	5.64		
25	5.88		
26	5.63		
27	6.35		
ΣX_i	157.16	87.68	73.08
ΣX_i^2	923.21	513.80	455.06

(i) Do the types differ with regard to resin percentages at the end of one year's growth?

(ii) Compute a least or minimum significant difference. Has it any meaning for these data?

(iii) Compute the coefficient of variation. Do you believe that the variation among plants was so great as to obscure differences among the types for resin percentages?

3. For the sample $X_1 = 4$, $X_2 = 2$, and $X_3 = 3$, graph the sum of squares for various values of $\hat{\mu}$. Find the least squares estimate of μ and its variance.

4. Given the following breakdown of the total degrees of freedom:

Source of variation	df
Among regions	2
Among types within regions	5
Among individuals within types	23

Find the expectation of mean squares, estimate the variance components, and obtain an analysis of covariance for the data presented by Day and Fisher, Ann. Eugenics 7:333, 1937.

5. By the method of section IV-2.5, obtain the least squares estimate of μ and β in the model $Y_i = \mu + \beta(X_i - \bar{x}) + \epsilon_i$. Obtain the variances and the covariance of $\bar{y}$ and b.

6. H. O. Dunn (Cornell Univ., unpublished data) obtained the body weights of calves at 8 weeks of age. There were 3 levels of feeding given to a random sample of five calves each. The completely randomized design was used. Obtain the analysis of variance, compute F, and obtain the standard error of a feeding treatment mean for the data on body weights:

	Level of feeding		
	Subnormal	Normal	Supernormal
	118	142	162
	122	129	173
	121	134	168
	126	132	183
	109	135	172
Total	596	672	858

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(24 soils and fertilizer treatments with 4 pots each)

Norton, C. L., et al., J. Dairy Sci. 29:231, 1946.
(Examples of 2, 2, and 3 feeding treatments with 8, 12, and 5 calves each, respectively)

Salisbury, G. W., J. Dairy Sci. 27:551, 1944.
(2 treatments on several characteristics of bull semen with 10 bulls on each treatment)

Wilcoxon, F., Biometrics 1:80, 1945.
(2 insecticides on houseflies with 8 trials each)

Wishart, J., and Sanders, H. G., Emp. Cotton Growing Corp., 1936.
(5 pseudo-treatments on uniformity data in 10 plots each)

Woodman, H. E., et al., J. Agr. Sci. 26:546, 1936.
(3 group feeding treatments with 10 pigs in each)

Other examples may be found in various textbooks, e.g., Snedecor, Statistical Methods (ch. 10; pp. 318, 341); Goulden, Methods of Statistical Analysis (ex. 29); Leonard and Clark, Field Plot Technique (ch. 12, table 1); Cochran and Cox, Experimental Designs (pp. 86-93); Fisher, Statistical Methods for Research Workers (ex. 41, 44); Paterson, Statistical Techniques in Agricultural Research (p. 48); Snedecor, Calculation and Interpretation of Analysis of Variance and Covariance (ex. 1, 11); and Kendall, The Advanced Theory of Statistics, vol. II (pp. 175-181).

CHAPTER V - RANDOMIZED COMPLETE BLOCK DESIGN

PROBLEMS

1. Bartlett (J. Roy. Stat. Soc., Suppl. 3:185, 1936) presents the following data (counts) obtained from an experiment on control of leatherjackets by certain toxic emulsions:

	Treatments					
Replicates	1(control)	2(control)	3	4	5	6
1	92	66	19	29	16	25
2	60	46	35	10	11	5
3	46	81	17	22	16	9
4	120	59	43	13	10	2
5	49	64	25	24	8	7
6	134	60	52	20	28	11

The design was a randomized complete block. Complete the analysis of variance. Are the variances homogeneous and is a transformation necessary? Using the method of section II-1.1.4 set 95 percent confidence intervals on the differences.

2. Partition the treatment sum of squares in example 1 into the four orthogonal comparisons each with one degree of freedom. Two of the components are linear and quadratic. What are the other two? The linear component sum of squares is 3492.29, and the additional sum of squares due to quadratic regression is 137.05. Verify the latter figure and obtain the sum of squares for the other two contrasts each with one degree of freedom.

3. Delete the yields 9.9 and 36.6 in replicate VI of table V-1. Compute the values for the missing data, and complete the analysis of variance with the adjusted sum of squares for treatments. Compute the standard error for a treatment mean.

4. Delete the following values in table V-3: 4.06 and 3.75 in plot 7, 2.06 in plot 1, 5.12 in plot 14, and 5.00 in plot 29. Complete the analysis of variance table and compute the standard errors for the various means.

5. The following number of celery plants for various treatments were available for an experiment:

Sowing dates	Cold treated	Untreated or controls
I	50	10
II	60	24
III	96	48
IV	100	95

It is desired to obtain information on all 8 treatments and to use a randomized block design. The plants are to be planted in rows 3 feet apart with 7 inches between plants within the row. The data to be taken are number of days from planting to flowering. Since celery is a biennial plant it is suspected that the controls (untreated plants) will not flower. How would you design this experiment and why? What would be your second choice of a design?

6. An experimenter had the following amount of material available:

3 clones	40 plants each	17 different items
3 S_1 lines	80 " "	
3 F_2's	80 " "	
7 F_1's	40 " "	
1 F_1	24 " "	

It was desired to make use of all the material if possible and to use 8 plant plots. Also, the experimenter wished to compare these items for yield and other characteristics under field conditions. What design would you use? How many replicates of each entry would be available? What is your second choice of a design? Why? Give the breakdown of the degrees of freedom for each design.

7. For an experiment with one-year-old cherry trees, 8 storage treatments are to be compared. Eighty trees are available for each treatment. The 640 trees are to be planted one foot apart in a 20' x 32' area. Measurements and observations to be made are number surviving, the length of shoot growth the first year, and the diameter increase at the end of the first and second years. The trees will probably be removed after the second year. What experimental design would you use? Give the complete experimental plan including the randomization and plot numbering; give the reasons for your selection of the design and the plot size.

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Autrey, K. M., et al., J. Dairy Sci. 30:775, 1947.
(21 silage mixtures in miniature silos, 10 replicates)

Bartlett, M. S., J. Roy. Stat. Soc., Suppl. 3:68, 1936.
(6 weed control treatments on oats, 4 replicates; 7 treatments on number of wheat seeds germinating, 4 replicates)

——, J. Roy. Stat. Soc., Suppl. 4:137, 1937.
(2 experiments on dairy cows - 4 treatments in 4 replicates, missing data; 7 treatments, germination data on peas, 6 replicates; 7 treatments, insecticides on blowflies, 5 replicates)

——, and Kendall, D. G., J. Roy. Stat. Soc., Suppl. 8:128, 1946.
(3 groups and 15 days)

Beall, G., Biometrika 32:243, 1942.
(7 entomological experiments with 3-7 treatments and 6-10 replicates)

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(2 treatments on pairs of animals)

Bigot, A., Landbouwk. Tijdschr. Wageningen 46:640, 1934.

Bliss, C. I., J. Am. Stat. Assoc. 35:498, 1940.
(6 treatments on the 6 rats of a litter with 6 litters)

Bose, R. D., Agr. and Livestock in India 2:603, 1932.
(4 barley varieties in 4 replicates)

Bose, S. S., and Mahalanobis, P. C., Sankhyā 4:103, 1938.
(5 treatments in 4 replicates)

Brandt, A. E., J. Am. Stat. Assoc. 36:283, 1941.
(2 corn varieties in 5 years)

Braude, R., and Foote, A. S., J. Agr. Sci. 32:324, 1942.
(3 feeding treatments on 8 three-pig litters)

Carruth, L. A., J. Econ. Ent. 29:205, 1936.
(11 chemical and mechanical treatments for controlling corn ear worm larvae in 6 replicates)

Chowdhury, S., Indian J. Agr. Sci. 14:391, 1944.
(Effect of 5 manurial treatments on root rot of pan in 6 replicates)

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(9 varieties of cotton each planted twice in a replicate; 5 replicates)

Cochran, W. G., Emp. J. Exp. Agr. 6:157, 1938.
(Plant numbers of mangolds from 8 treatments in 4 replicates; 4 groups of words (treatments), reaction time from 6 sittings (blocks))

——, Biometrics 3:22, 1947.
(4 times of applying nitrogen to wheat in 4 replicates)

Crampton, E. W., Am. Soc. Animal Prod., Proc. 23:56, 1930.
(2 feeding treatments on 10 pairs of animals)

——, Sci. Agr. 11:281, 347; 12:174, 1931.
(2 feeding treatments on 5 pairs of pigs; 2 feeding treatments on 9 pairs of cows; 2 feeding treatments on 10 pairs of pigs)

Dabral, B. M., and Chiney, S. S., Indian J. Agr. Sci. 8:629, 1938.
(6 fertilizer treatments on cotton in 6 replicates)

Das, S., Indian J. Agr. Sci. 15:47, 1945.
(5 depths of placing superphosphate on mustard and on ragi in 4 replicates)

Dudley, F. J., and Read, D. R., Harper Adams Utility Poultry J. 34:65, 1949.
(Several examples, blocks = pens and feeding rations)

Ensign, R. D., et al., Mimeo. report, Colo. A and M, Fort Collins, 1947.
(Several examples on pinto bean yields)

Fisher, R. A., and Wishart, J., Imp. Bur. Soil Sci., Tech. Comm. 10, 1930.
(5 fertilizer treatments on barley in 6 replicates)

Goulden, C. H., Sci. Agr. 11:681, 1931.

(9 fertilizers on potatoes in 4 replicates)

Heath, O. V. S., Emp. J. Exp. Agr. 2:205, 1934.
(3 treatments of soil compaction in 4 replicates)

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(8 black currant varieties in 6 replicates)

Kalamkar, R. J., and Dhannalal, L. A., Sankhyā 4:567, 1940.
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(6 varieties of cotton in 10 replicates)

Krishna Iyer, P. V., Indian J. Agr. Sci. 10:686, 1940.
(9 varieties in 6 replicates with 17 missing values)

Ladell, W. R. S., and Cochran, W. G., Ann. Appl. Biol. 25:341, 1938.
(3 treatments on wireworm in 10 replicates; 6 treatments on wireworm in 8 replicates)

Loosli, J. K., et al., Cornell Univ. Agr. Exp. Sta. Mem. 265, 1944.
(2 feeding treatments on 9 pairs, 10 pairs, and on 6 pairs of animals)

Lynes, F. F., J. Am. Soc. Agron. 27:980, 1935.
(15 treatments on bindweed eradication in 5 replicates)

Ma, P. C., J. Agr. Assoc. China 158:13, 1937.

Mahalanobis, P. C., and Bose, S. S., Indian J. Agr. Sci. 3:339, 544, 1933.
(5 cotton varieties in 5 replicates; 5 wheat varieties in 8 replicates)

——, and Ray, S., Sankhyā 2:397, 1936.
(5 classes of students and 5 schools on school marks and I.Q.'s)

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(Several examples, some are hypothetical)

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(Numerical examples of interchanged and substituted plots)

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(5 cotton varieties in 10 replicates)

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(Several characters on 5 sugar cane varieties in 8 replicates)

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(5 fertilizers on rice in 6 replicates)

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(4 preservative treatments on alfalfa silage in 5 periods)

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(Several examples)

Telegdy Kovats, L. v., Mezögazdás. Kutatas. 10:1, 1937.

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(Several examples on yields of pinto bean varieties; 5 fertilizer treatments on pinto beans in 5 replicates)

Thornton, H. G., J. Agr. Sci. 19:373, 1929.

(7 seed inoculation treatments on runner beans in 6 replicates)

Titus, H. W., and Harshaw, H. M., Poultry Sci. 14:3, 1935.
(4 diets on Rhode Island cockerels in 15 replicates)

Turner, P. E., Trop. Agr. 9:120, 206, 1932.
(Tillage and manurial treatments on sugar cane)

Vaidyanathan, M., Indian J. Agr. Sci. 4:327, 1934.
(4 treatments on tea in 6 replicates)

Watson, S. J., and Ferguson, W. S., J. Agr. Sci. 26:337, 1936.
(4 feeding treatments on 5 sets of 4 cows each; also a missing value)

Wilcoxon, F., Am. Cyanamid Co., Stamford, Conn., 1946.
(5 seed treatments in 6 replicates)

Wishart, J., Biometrika 30:16, 1938.
(3 protein diets on pigs in 5 replicates)

——, Imp. Bur. Plant Br. and Genetics, 1940.
(5 barley varieties in 4 replicates)

Wood, R. C., Trop. Agr. 9:290, 1932.
(8 varieties of sugar cane in 4 replicates)

——, Emp. J. Exp. Agr. 4:365, 1936.
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(Examples of feeding treatments on pigs)

Woodman, R. M., and Johnson, D. A., J. Agr. Sci. 36:87, 1946.
(4 soils in 9 replicates)

Yates, F., Emp. J. Exp. Agr. 1:129, 1933.
(Examples with several missing plots)

In addition to the above examples others may be found in various textbooks; e.g., Baten, Mathematical Statistics (p. 253); Cochran and Cox, Experimental Designs (pp. 40, 96); Fisher, Design of Experiments (pp. 66, 141); Goulden, Methods of Statistical Analysis (ch. XI); Hayes and Immer, Methods of Plant Breeding (pp. 309, 326); Immer, Applied Statistics (pp. 3, 11, 16, 33, 62); Johnson, Statistical Methods of Research (p. 289); Kendall, Advanced Theory of Statistics, vol. II (pp. 184, 230); Leonard and Clark, Field Plot Technique (pp. 172, 179, 184, 185); Love, Experimental Methods in Agricultural Research (pp. 21, 37, 59, 69, 75, 115); Mather, Statistical Analysis in Biology (pp. 56, 130); Paterson, Statistical Techniques in Agricultural Research (pp. 53, 59, 163, 190, 206, 216, 221, 226); Snedecor, Calculation and Interpretation of Analysis of Variance and Covariance (ex. 4, 6); Snedecor, Statistical Methods (pp. 254, 265, 270, 280).

CHAPTER VI - THE LATIN SQUARE DESIGN

PROBLEMS

1. Compute the standard error of a treatment total and the corresponding lsd and hsd for the data of table VI-1.

2. Find the efficiency of the latin square design in table 11.11 of Snedecor's book relative to a completely randomized design and to the two randomized complete block designs obtained when the columns are used as replicates and when the rows are used as replicates. Discuss briefly the effect of reducing the block size from 25 plots to 5 and the 2-way elimination of variation. Obtain the efficiencies adjusted for the difference in degrees of freedom in the two error variances.

3. In example VI-2 an experiment on the marketing of McIntosh apples was discussed. The entire experiment consisted of four 4x4 latin squares. The data from two of the latin squares are given in table VI-2. The data from the other two latin squares are given below:

	Second part - Week 7					Second part - Week 8				
	Store number					Store number				
	1	2	3	4	Total	1	2	3	4	Total
Friday A.M.	B 8	A28	D58	C34	128	D44	C68	B40	A34	186
Friday P.M.	C52	D112	B68	A60	292	B16	A48	C100	D94	258
Saturday A.M.	D56	C52	A40	B42	190	C20	D40	A28	B32	120
Saturday P.M.	A28	B20	C60	D52	160	A45	B44	D64	C25	178
Total	144	212	226	188	770	125	200	232	185	742

Compute the analysis of variance on each of the above latin squares. Combine the data from the four latin squares into a single analysis of variance. Test all meaningful hypotheses and state your conclusions. Compute the standard error of a treatment mean from the combined analysis. Also, compute the coefficient of variation and the hsd.

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(5x5 l.s.)

Beckett, W. H., Trop. Agr. 9:7, 1932.
(4 treatments in a 4x4 l.s.)

Bliss, C. I., and Marks, H. P., Quart. J. Pharm. and Pharm. 12:82, 182, 1939.
(4 dates of insulin injection, 4 dosages, and 8 rabbits in two 4x4 l.s.; 8x8 l.s. on 4 dosages of insulin on two days)

Bose, R. D., Agr. and Livestock in India 2:603, 1932.
(4 barley varieties in a 4x4 l.s.)

Bose, S. S., and Mahalanobis, P. C., Sankhyā 4:103, 1938.
(4x4 l.s. with 2 mixed-up yields; two 4x4 l.s. with 2 mixed-up yields)

Cochran, W. G., Emp. J. Exp. Agr. 6:157, 1938.
(5x5 l.s. to estimate effect of 5 soil fumigants on number of wireworms)

——, and Watson, D. J., Emp. J. Exp. Agr. 4:69, 1936.
(6x6 l.s. used to control and measure observer's bias in estimating plant height; rows = order, columns = areas and treatments = observers)

Dominick, B. A., Jr., Unpubl. Ph. D. thesis, Cornell Univ., 1952.
(Several examples on marketing apples)

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(5 fertilizer treatments on barley in a 5x5 l.s.)

Gaines, J. C., J. Econ. Ent. 30:119, 1937.
(6 dusting treatments on cotton in two 6x6 l.s.)

Garrett, H. E., and Zubin, J., Psychol. Bul. 40:233, 1943.
(Four orders of presentation, four illumination levels, and four colors were presented to subjects; 4x4 l.s.)

Gilbert, S. M., E. Africa Agr. J. 4:131, 1938.
(4x4 l.s. on coffee trees)

Hansberry, T. R., and Richardson, C. H., Iowa State College J. Sci. 10:27, 1935.
(4 spraying treatments on apple trees in a 4x4 l.s.)

Hansford, C. G., <u>et al</u>., Ann. Appl. Biol. 20:404, 1933.
(4 seed treatments on cotton in a 4x4 l.s.; several characters)

Harrison, C. J., <u>et al</u>., Sankhyā 2:33, 1935.
(8x8 l.s. on effect of 8 manurial treatments, 8 dates, and 8 methods of manufacturing on quality of tea)

Inman, W. R., <u>et al</u>., Sci. Agr. 20:33, 1939; 22:18, 1941.
(4 feeding rations in a 4x4 l.s. with the rows being different foxes and the columns periods)

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(5x5 l.s. with rows laid end to end on 5 soil treatments for controlling wireworms)

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(4x4 l.s. on the weaving of cotton cloth)

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(4 fertilizer treatments on potatoes in a 4x4 l.s.)

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(3x3 l.s.)

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(Numerous examples on various crops and with various treatments)

Taylor, S. A., Unpubl. Ph. D. thesis, Cornell Univ., 1949.
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——, and Potter, J. A., Trop. Agr. 9:44, 1932.
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Ward, G. M., and Smith, V. R., J. Dairy Sci. 32:17, 1949.
(5 times of milking as they affect total yield of milk in a 5x5 l.s.)

Watson, C. J., et al., Sci. Agr. 20:175, 238, 1939; 20:458, 1940; 22:561, 1942; 23:708, 1943; 29:263, 400, 1949.
(Several numerical examples of feeding treatments with ruminants; second and fifth papers have some missing plots as the animals refused to eat the hay)

Wishart, J., Imp. Bur. Plant Br. and Genetics, 1940.
(4 varieties of sugar beets in a 4x4 l.s.)

——, and Sanders, H. G., Emp. Cotton Growing Corp., London, 1936.
(5 fertilizers on wheat in a 5x5 l.s.)

Wood, R. C., Emp. J. Exp. Agr. 1:316, 1933.
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——, and Hale, R. W., J. Roy. Stat. Soc., Suppl. 6:67, 1939.
(7x7 l.s. with 2 missing treatments)

——, and Watson, D. J., Emp. J. Exp. Agr. 2:174, 1934.
(10 observers counting plants and tillers in a 10x10 l.s.)

Youden, W. J., Contr. Boyce Thompson Inst. 11:207, 1940.
(5x5 l.s. on soybeans with 5 seed treatments; one column missing)

——, and Beale, H. P., Contr. Boyce Thompson Inst. 6:437, 1934.
(5 leaf inoculation treatments of virus on tobacco in a 5x5 l.s.)

Young, D. M., and Romans, R. G., Biometrics 4:122, 1948.
(3 numerical examples of 4 insulin injections into rabbits (columns) on different dates (rows))

In addition to the above numerical examples, others may be found in various textbooks; e.g., Baten, Mathematical Statistics (pp. 259, 261); Edwards, Experimental Design in Psychological Research (pp. 307, 321, 327-332); Fisher, Statistical Methods (ex. 46, 46.1); Fisher, Design of Experiments (p. 88); Goulden, Methods of Statistical Analysis (ex. 34); Hayes and Immer, Methods of Plant Breeding (ch. XX); Leonard and Clark, Field Plot Technique (pp. 174, 184); Love, Application of Statistical Methods to Agricultural Research (p. 441); Kendall, Advanced Theory of Statistics, vol. II (p. 258); Love, Experimental Methods in Agricultural Research (pp. 31, 44, 86, 94, 112); Mather, Statistical Analysis in Biology (p. 98); Paterson, Statistical Techniques in Agricultural Research (pp.

169, 173); Snedecor, Calculation and Interpretation of the Analysis of Variance and Covariance (ex. 5, 12); Snedecor, Statistical Methods (pp. 271, 275, 424, 446, 448).

PROBLEMS

1. Six replicates of a randomized complete block design were used to compare nine treatments. Eight of the treatments recieved a lime application in addition to one of the eight combinations of three factors, n, p, and k each at two levels. Thus, the nine treatments represent a 2^3 factorial arrangement of the factors n, p, and k plus a check. Do you agree with the following breakdown of the total degrees of freedom? Why or why not?

Source of variation	df	
Replicates	5	
Treatments	8	
Ch. vs others		1
N		1
P		1
K		1
NP		1
NK		1
PK		1
NPK		1
Error	40	
Total	53	

What coefficients with what signs are required for the above comparisons? Answer by completing the following table:

Effect	ch.	l	ln	lp	lnp	lk	lnk	lpk	lnpk	Divisor
ch. vs others										
N										
P										
NP										
K										
NK										
PK										
NPK										

If E = error mean square, what is the standard error of difference for the comparison of lime versus no lime? What is the lsd for an effect total? for an effect mean?

2. The following data are of the same nature as described for the experiment

discussed in example VII-2 except that the 48 boys were asked a series of questions on stereophotographic pictures. Complete the analysis of these data as described in example VII-2. Give the combined analysis for the data on stereophotographic and on non-stereophotographic pictures.

		Right	Wrong			Right	Wrong
A = 000	1	18	0	E = 001	1	16	2
	2	16	2		2	18	0
	3	16	2		3	16	2
	4	18	0		4	18	0
	5	18	0		5	17	1
	6	17	1		6	18	0
B = 100	1	18	0	F = 101	1	17	1
	2	14	4		2	18	0
	3	14	4		3	16	2
	4	17	1		4	17	1
	5	16	2		5	16	2
	6	18	0		6	17	1
C = 010	1	20	7	G = 011	1	18	9
	2	17	10		2	25	2
	3	25	2		3	25	2
	4	25	2		4	26	1
	5	23	4		5	25	2
	6	25	2		6	19	8
D = 110	1	24	3	H = 111	1	27	0
	2	23	4		2	26	1
	3	26	1		3	23	4
	4	23	4		4	25	2
	5	22	5		5	25	2
	6	19	8		6	25	2

3. Set up a table in which all effects in a 2^3 factorial are zero except ABC. Start with the fact that all levels of effect totals are equal to 40, and then set up the general case where all levels of effects are equal to a constant, say k, and the yield of treatment 000 is x.

4. Partition the error sum of squares in table VII-1 into its three component parts. Test the homogeneity of the three variances. Discuss the results.

5. Using the method of section II-1.1.4 calculate the 95% confidence intervals for all differences among pairs of means for the data of example VII-2.

6. A 2^4 factorial experiment on fertilizers is to be set up in an established orchard. It is planned to use individual tree plots, to use 96 trees for the experiment, and to use a randomized complete block design. Discuss the flaws in such experiments, the likelihood of errors, and the selection of the experimental trees. Give the experimental plan, including the randomization procedure and the key-out for the degrees of freedom in the experiment.

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(Discussion and plan for a 2^5 factorial feeding experiment on sheep; original data not given)

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(Check plus 2^3 fertilizer treatments on coconuts in 4 replicates; 4 years' data)

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(2 levels of nitrogen x 2 levels of sulphate x 2 times of application plus 4 checks in 8 replicates)

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(3x3 factorial of potato varieties and fertilizers in 3 blocks)

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(Discusses a 2x3x2 and a 4x4 factorial, and quality x quantity interaction)

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(2x2 fertilizer factorial on tung trees in 3 blocks)

Singh, R. N., Indian J. Agr. Sci. 12:588, 1942.
(3x3 factorial of distances from leaf and pressure in spraying for woolly aphis in 4 blocks)

Wishart, J., and Hines, H. J. G., J. Ministry Agr. 36:524, 1929.
(3 nitrogen x 3 potash factorial on potatoes in 9 replicates)

Yates, F., Emp. J. Exp. Agr. 1:129, 1933.
(2^3 factorial in 10 replicates with 9 missing values)

——, Imp. Bur. Soil Sci., Tech. Comm. 35, 1937.
(2^3 fertilizer factorial on potatoes in 4 replicates of a randomized complete block design)

In addition to the above citations, numerical examples may be found in various textbooks; e.g., Cochran and Cox, Experimental Designs (ch. 5), and Snedecor, Statistical Methods (sec. 15.9).

CHAPTER VIII - OTHER FACTORIAL EXPERIMENTS

PROBLEMS

1. For the data in example VIII-1 compute the standard errors on a mean per plot basis. Convert the figures for means and standard errors to tons per acre. On the assumption that Sword Bean and Sunnhemp have something in common make the comparisons Sunnhemp vs Sword Bean and Sunnhemp + Sword Bean vs 2 Woolly Pyrol. Also, partition the interaction sum of squares into the two contrasts. Plot the results.

2. Using the model of section II-1.1.4 set 95 percent confidence intervals on the variety means and on the nitrogen means.

3. Leave out the treatments a_0b_0, a_1b_1, and a_2b_2 in the 3x3 factorial in example VII-3 and compute the analysis described in section VIII-5.3 on the remaining data.

4. Delete all treatments except a_0b_1, a_0b_2, and a_1b_2 in example VII-3 and carry out the analysis proposed in section VIII-5.4.

5. Verify that the variances and covariances for the $\hat{\alpha}_i$, $\hat{\beta}_j$, and $\widehat{\alpha\beta}_{ij}$ are as stated in section VIII-5.1.

LITERATURE CITATIONS

Autrey, K. M., et al., J. Dairy Sci. 30:775, 1947.
(3 forage crops x 7 wilting treatments in 10 replicates; silage)

Bell, G. D. H., et al., J. Agr. Sci. 32:255, 1942.
(3 potato varieties x 5 cutting treatments in 6 replicates)

Bliss, C. I., J. Am. Stat. Assoc. 35:498, 1940.
(3x2 factorial in a nutritional investigation in 6 replicates)

Brandt, A. E., J. Am. Soc. Agron. 29:658, 1937.
(3 times x 2 levels of chemical A x 2 levels of chemical B x 2 levels of chemical C in 24 flasks)

Davis, J. F., et al., J. Am. Soc. Agron. 34:521, 1942.
(4 fertilizers x 3 methods of placement in 5 replicates)

Durbin, J., and Stuart, A., J. Roy. Stat. Soc., A 114:163, 1951.
(3^3 x 2^3 factorial in 7 replicates on response errors in personal interviews)

Gobeil, R., La Forêt, Quebec 2:31, 1940.
(3x3x2 fertilizer factorial)

Griswold, R. M., and Blakeslee, L. H., Proc. Am. Soc. Animal Prod. 32:305, 1939.
(7 wrappings x 3 temperatures x 2 storages with 11 characteristics on frozen pork chops)

Harrison, C. J., and Bose, S. S., Sankhyā 6:151, 1942.
(2 prunings x 4 jats in an 8x8 l.s. on tea)

Hoblyn, T. N., Imp. Bur. Fruit Prod., Tech. Comm. 2, 1931.
(3 strawberry varieties x 4 dates of planting in 4 replicates)

Klingebiel, A. A., and Brown, P. E., J. Am. Soc. Agron. 30:1, 1938.
(2 inoculations x 4 levels of limestone in 24 pots)

Peh, S. C., J. Am. Soc. Agron. 29:167, 1937.
(3 rice varieties x 7 numbers of seeds per row in 6 replicates; 4 varieties x 7 numbers of seeds in 6 replicates; 5 numbers of plants per hill x 6 distances between hills in 10 replicates)

Snedecor, G. W., J. Am. Stat. Assoc. 31:690, 1936.
(4 cows x 2 feeds x 3 periods)

Tharp, W. H., et al., Phytopathology 31:26, 1941.
(3 nitrogen x 2 potash x 2 phosphorus x 3 cotton varieties in 4 blocks)

Wilcoxon, F., Am. Cyanamid Co., Stamford, Conn., 1949.
(2x2x3 factorial on seed treatments on corn in 3 replicates)

Wood, R. C., Emp. J. Exp. Agr. 1:316, 1933.
(Several factorials on yams)

In addition to the above citations, numerical examples may be found in various textbooks; e.g., Anderson and Bancroft, Statistical Theory in Research (ch. 20); Cochran and Cox, Experimental Designs (ch. 5); and Snedecor, Statistical Methods (ch. 11).

CHAPTER IX - CONFOUNDING IN FACTORIAL EXPERIMENTS

PROBLEMS

1. What design would be most appropriate given the following information? Nine roasts can be cut from each of 12 animals. The experimenter wishes to study the effect of freezing and storage temperatures and length of storage upon tenderness of roasts and to use the following in all combinations:

storage temperatures	-	0, 10, and 15 degrees,
freezing temperatures	-	0, -10, and -20 degrees, and
length of storage	-	30, 90, and 180 days.

Give the breakdown of the total degrees of freedom and set up the design. Give the design and analysis for six roasts from each of 12 animals with two freezing temperatures, 0 and -20.

2. The following is a field plan for a 2^3 factorial experiment. What effects are confounded in each of the replicates?

I		II		III	
ac	c	ab	c	(1)	bc
(1)	abc	a	(1)	ab	a
bc	b	abc	bc	c	b
ab	a	ac	b	abc	ac

Write out the subdivision of the total degrees of freedom in the analysis of variance, summary tables, and adjustments for treatment totals.

3. For a 2^3 factorial experiment with four replicates give the appropriate subdivision of the total degrees of freedom, the design, the summary tables, and the adjustments for the treatment means for:

(i) a randomized incomplete block design in which the AB interaction is completely confounded in all replicates;

(ii) a randomized incomplete block design in which the
ABC effect is confounded in replicate 1,
BC effect is confounded in replicate 2,
AC effect is confounded in replicate 3, and
AB effect is confounded in replicate 4.

4. Compute the sums of squares for the linear and quadratic components for the factor a in example IX-3 and partition the sums of squares for the AB, AC, and ABC effects into single degrees of freedom. Is there any change in the interpretation of the effects? Compute the standard error for each of the 11 comparisons.

5. Show algebraically that the factor 3/4 is the correct factor to use in example IX-2 for obtaining the adjusted effects for the interactions which are partially confounded with incomplete block differences.

6. Suppose that the B effect was partially confounded with incomplete block differences in 2 replicates and that the AB effect was confounded in the other 2 replicates of a 2^2 factorial in incomplete blocks of 2 experimental units. Give the algebraic formulae for the adjusted effect totals obtained from the adjusted treatment totals.

7. Given the following experimental lay-out:

Variety v_0		Variety v_1	
Seed size s_0	Seed size s_1	Seed size s_1	Seed size s_0

A 2 x 3^3 factorial is arranged at random within each of the 4 rectangles. Key out the degrees of freedom for each rectangle and for all rectangles combined. Indicate the components constituting the experimental error and the assumptions involved.

8. Discuss confounding in experimental work. Illustrate with examples.

9. Discuss the factors determining the number of replicates used in experimental work. What factors affect the precision of comparisons among treatment means in a replicated experiment?

LITERATURE CITATIONS

Eden, T., and Fisher, R. A., J. Agr. Sci. 19:201, 1929.
(3^3 factorial in incomplete blocks of 9)

Finney, D. J., Ann. Eugenics 12:291, 1945.
(1/2 replicate of a 4 x 2^4 factorial on potatoes)

——, J. Agr. Sci. 36:184, 1946.
(1/2 replicate of a 2^6 in blocks of 8)

Fisher, R. A., and Wishart, J., Imp. Bur. Soil Sci., Tech. Comm. 10, 1930.
(2x2x4 and a 2x4 in incomplete blocks of 12)

Klingebiel, A. A., and Brown, P. E., J. Am. Soc. Agron. 29:944, 1937.
(2 crops x 4 applications of limestone x 2 regions)

Ma, R. H., and Kao, L. M., Emp. J. Exp. Agr. 8:23, 1940.
(3 varieties of rice x 3 dates x 3 spacings with partial confounding of the 3 factor interaction)

Rothamsted Exp. Sta. Report, 1933.
(Several examples of confounding)

Snedecor, G. W., Biometrics 4:211, 1948.

(2^3 factorial in incomplete block of 2 treatments)

Tischer, R. G., and Kempthorne, O., Food Tech. 5:200, 1951.

(Analysis only for a 1/9 replicate of a 3^7 in incomplete blocks of 27)

Turner, P. E., Trop. Agr. 12:293, 320, 1935.
(4n x 2k x 2p x 2ca in incomplete blocks of 16 in sugar cane in 4 replicates)

Vaidyanathan, M., and Subramonia Iyer, S., Indian J. Agr. Sci. 10:213, 1940.

(3^3 treatments on sugar cane in incomplete blocks of 9; 3^4 treatments on sugar cane in incomplete blocks of 9)

Wishart, J., J. Agr. Sci. 28:299, 1938.

(2^3 factorial on asparagus uniformity trial data in incomplete blocks of 4 with 3 factor interaction completely confounded)

———, Imp. Bur. Plant Br. and Genetics, 1940.

(2^3 (n, p, k) factorial on asparagus in 4 replicates; NPK completely confounded)

Yates, F., J. Roy. Stat. Soc., Suppl. 2:181, 1935.

(2^3 factorial on peas with NPK completely confounded in 3 replicates; 3^3 in blocks of 9 plots with one replicate on sugar beets)

———, Imp. Bur. Soil Sci., Tech. Comm. 35, 1937.

(3^3 in incomplete blocks of 9; 3x2x2 in incomplete blocks of 6)

In addition to the above citations, numerical examples may be found in various textbooks; e.g., Cochran and Cox, Experimental Designs (ch. 6); Fisher, The Design of Experiments (sec. 52-4); Goulden, Methods of Statistical Analysis, 2nd ed. (ch. 12); Immer, Applied Statistics (ch. 16); Kempthorne, The Design and Analysis of Experiments (sec. 13.4, 14.2, 14.7, 15.8, 16.5); and Leonard and Clark, Field Plot Technique (ch. XIX).

PROBLEMS

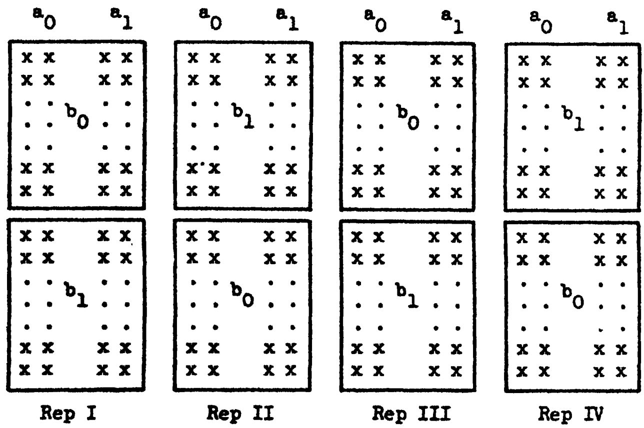
1. 1943 yield data were obtained on the same six corn hybrids as used in example X-2. The yields in pounds of ear corn from a 2 x 10 hill plot for the six hybrids from four replicates of a randomized complete block design are given below below in a systematic arrangement of replicates and varieties for Districts I and II. Compute the analysis of variance for these data. Compute the analysis of variance for years 1942 and 1943 for each district and obtain the combined analysis over both districts and years. Set 95 percent confidence intervals on the differences for pairs of means. Give an interpretation of the results.

Table for Problem 1

Systematic arrangement of replicates and varieties of corn hybrid yield data for Districts I and II for 1943.

Doublecross designation	Replicate number I	II	III	IV	Total	Doublecross totals
	District I					
1 - 1	37.5	36.6	34.9	33.2	142.2	
2 - 2	37.2	37.0	34.5	33.8	142.5	
4 - 3	28.7	32.2	31.0	28.5	120.4	
15 - 43	34.7	32.7	31.0	30.7	129.1	
8 - 38	40.3	37.0	37.0	36.6	150.9	
7 - 39	34.1	33.2	32.8	31.6	131.7	
Total	212.5	208.7	201.2	194.4	816.8	
	District II					
1 - 1	24.2	27.7	26.5	25.4	103.8	246.0
2 - 2	28.7	31.1	27.4	29.5	116.7	259.2
4 - 3	25.8	24.4	19.9	20.5	90.6	211.0
15 - 43	25.5	32.1	28.8	21.8	108.2	237.3
8 - 38	28.3	26.2	27.3	24.9	106.7	257.6
7 - 39	28.1	29.6	26.2	24.5	108.4	240.1
Total	160.6	171.1	156.1	146.6	634.4	1451.2

2. The following design of an experiment was used in an orchard:



The a_0 and a_1 treatments were systematically arranged in all four replicates. Likewise, the b_0 and b_1 treatments were arranged in the manner shown by design and not by chance. The a_0 treatment represents no dusting treatment and a_1 represents dusting treatment. The b_0 and b_1 treatments (fertilizers) were arranged in the manner shown because the experimenter wished them to be "representative". The character studied is pounds of fruit per tree. What effects are confounded and what effects are unconfounded? Is it correct to use the F test to compare the dusting mean square, the fertilizer mean square, or the interaction mean square? Why or why not? Also, it was argued that no other design could have been used since the fertilizer and dusting applications were made in a farmer's orchard and since one must use the farmer and regular farm machinery for these operations. Do you agree or disagree on the reasons for designing the experiment in this manner? Why?

3. In a grazing experiment with 24 dairy cows two replicates in a pasture were available; it was desired to study the effect of three grazing treatments on equal areas of land - continuous grazing, 12 days of no grazing and then 12 days of grazing, and two days of grazing and then two days of no grazing - with four cows per grazing treatment. In addition, two of the four cows on each grazing treatment were to be fed concentrates and the other two were not. The yield character studied is pounds of butterfat per cow per year. Set up a design for such an experiment conducted over a three year period and key out the degrees of freedom in the analysis of variance. What are the correct error mean squares for testing the variation among the various means under the hypothesis stated by you?

4. A randomized complete block design of 24 treatments in 3 replicates is

used. Since the experiment (with the same treatments) is conducted at different locations several miles apart, or in different years at the same location, but on different parts of the experimental area:

(i) is it necessary to rerandomize for each location and for each year,

(ii) is it advisable to rerandomize, and

(iii) why?

5. The following analyses of variance are from 3 locations. The treatments are the same at all 3 places:

	Location I		Location II		Location III	
	df	ss	df	ss	df	ss
Treatments	9	T_1	9	T_2	9	T_3
Replicates	3	R_1	4	R_2	5	R_3
Error	27	E_1	36	E_2	45	E_3
Total	39	G_1	49	G_2	59	G_3

What is the method of computation and breakdown of degrees of freedom for the combined analysis? How does one compute a least significant difference for place or location means; i.e., what is the n in the formula

$$\sqrt{\frac{2}{n}\text{ error mean square}} \quad ?$$

Would such a statistic have any usefulness? Explain.

6. Ten fertilizer treatments, A, B, C, D, E, F, G, H, I, and J were tested in 4 randomized complete blocks on cabbage. The following arrangement of plots is for one of the replicates:

H	G	I	J	A	D	E	B	C	F

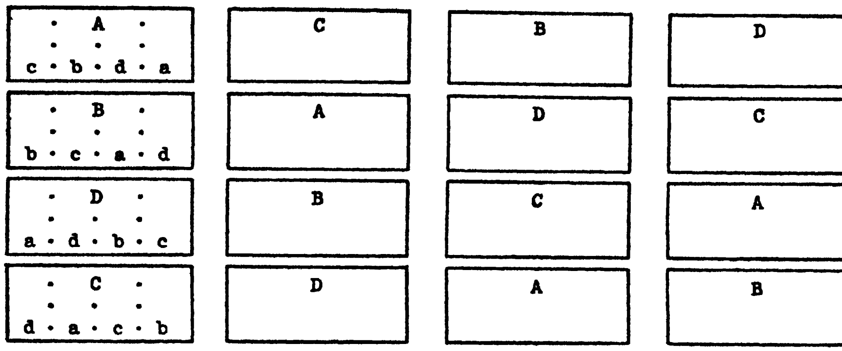
The replicate was divided into two parts across all fertilizer plots. Then two applications of DDT dusting (none and dusting) were made, at random, to the two halves of the plots; thus

No dust
- - - - - -
Dust

Is there any way to calculate the interaction of fertilizer treatments and applications of DDT dust? Key out the degrees of freedom for this design and indicate the appropriate error mean squares for making the various comparisons.

7. An experiment was conducted on 4 fertility levels, A, B, C, and D, and 4 levels of plant populations, a, b, c, and d. The crop was sweet corn. The following field arrangement of a 4x4 latin square with the split plots ordered

in each column was used:



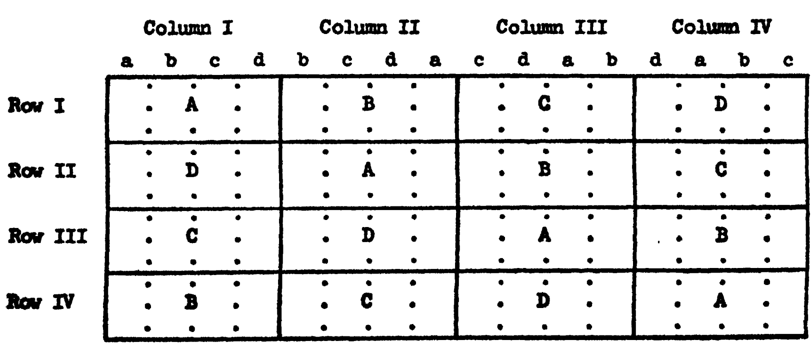
The remaining columns are likewise subdivided but with a different randomization. What is the analysis, taking into consideration the latin square arrangement of the split or sub-plots?

8. Four fertilizer treatments on peas were tested in 3 randomized complete blocks in 1946 and in 4 blocks in 1948. The following breakdown of the total degrees of freedom and sums of squares was obtained for the individual years:

	1946		1948	
Source of variation	df	ss	df	ss
Replicate	2	A	3	G
Treatment	3	B	3	H
Error = replicate x treatment	6	C	9	I
Total	11	D	15	J
Correction for mean	1	E	1	K
Total uncorrected ss.	12	F	16	L

What is the combined analysis of variance with regard to the breakdown of the degrees of freedom? How are the sums of squares for years and treatment by year interaction calculated?

9.



The field design presented above of a 4x4 latin square superimposed on a 4x4 latin square was made (the arrangement is systematic here but was random in the field). The treatments a, b, c, and d in column I are laid out in a systematic manner over the plots containing varieties A, B, C, and D in column I; i.e., treatment a continues across the 4 rows of the latin square containing the 4 varieties, A, B, C, and D. Treatments a, b, c, and d are arranged so that each one occupies each of the 4 orders within the columns. What is the breakdown of the degrees of freedom in the analysis of variance for such a design?

10. Give the diagrammatic lay-out and the key-out of the degrees of freedom for 2^3 whole plot treatments in an 8x8 latin square design with split plots of a 2^2 factorial set of treatments allotted at random within each whole plot. Give the design and key-out of the degrees of freedom taking into account the order within whole plots. Explain the randomization procedure used for both designs.

11. R. E. Blaser (Virginia Polytechnic Inst., 1950, unpublished results) conducted an experiment on 5 brome grass strains (a, b, d, e, f) both alone and with alfalfa. The 2 whole plots (with and without alfalfa) were laid out in a randomized complete block design in 4 replicates. The 5 brome grass strains represented the split plots and were randomly allotted to the plots within each whole plot. The following yields (dry weight in grams) were obtained:

Strain of brome	Rep I		Rep II		Rep III		Rep IV	
	With alfalfa	Alone	With alfalfa	Alone	With alfalfa	Alone	With alfalfa	Alone
a	730	786	1004	838	871	1033	844	867
b	601	1038	978	1111	1059	1380	1053	1229
d	840	1047	1099	1393	938	1208	1170	1433
e	844	993	990	970	965	1308	1111	1311
f	768	883	1029	1130	909	1247	1124	1289
Total	3783	4747	5100	5442	4742	6176	5302	6129
Rep totals	8530		10542		10918		11431	

Complete the analysis and write an interpretation of the results.

LITERATURE CITATIONS

Anderson, R. L., Biometrics 2:41, 1946.
(4 replicates of a r.c.b. design with 4 whole plots and 4 split plots with the procedure for estimating a missing split plot or a missing whole plot)

Arceneaux, G., and Herbert, L. P., J. Am. Soc. Agron. 35:148, 1943.
(6 varieties of sugar cane in a 6x6 l.s. for 4 years and 4 stations; they do not give original yields but do give the individual sums of squares)

Bachér, I., Beretn. Nordisk Jordbrforsker. Foren: 5th Kongr. København 4-7; Hefte:329, 1935.

(Split plot and more complicated designs)

Bartlett, M. S., J. Roy. Stat. Soc., Suppl. 3:68, 1936.
(Control of cockchafer larvae, 5 treatments in whole plots, 8 replicates of a r.c.b. design, split plots are two age groups of larvae; transformation of data)

Bescoby, H. B., J. S. E. Agr. College, Wye, Kent 30:215, 1932.
(4 kinds of nitrogen on barley are the whole plots in a 4x4 l.s.; 2 levels of phosphorus x 2 levels of potash are the split plot treatments)

Burkett, A. L., et al., J. Am. Soc. Agron. 41:255, 1949.
(2 replicates of 4 ages x 2 harvests as whole plots and 4 turning methods x 2 exposure periods as split plots; retting of hemp)

Chakravertti, S. C. et al., Indian J. Agr. Sci. 6:34, 1936.
(Split split plot design of 5 sowing dates for rice with sub-units of 3 varieties with each variety plot being subdivided into 3 spacings x 3 numbers of seedlings per hole)

de Verteuil, J., Trop. Agr. 11:315, 1934.
(Check and 2^3 fertilizer treatments on coconuts in r.c.b. designs for 3 years)

Ensign, R. D., et al., Mimeo report, Colo. A and M, Fort Collins, 1947.
(12 pinto bean varieties in 6 replicates of a r.c.b. design at two places; 6 fertilizer treatments in 4 replicates of a r.c.b. design at two places)

Finney, D. J., J. Agr. Sci. 36:56, 63, 1946.
(4x4 l.s. with 4 manurial treatments as whole plots; each whole plot was split in two and either n and k added or (1) and nk to the two plots; potatoes; 6x6 l.s. similar to the above; 12 treatments of organic manures as whole plots with each plot split in two and either n and k added or (1) and nk; carrots)

Fisher, R. A., Biometrics 5:300, 1949.
(Split split plot of 10 farms with 12 cows per farm with the 12 cows being divided into groups of 4 for 3 times of reading with 4 treatments or injections into the left and right sides of a cow's neck)

Gould, C. E., and Hampton, W. M., J. Roy. Stat. Soc., Suppl. 3:137, 1936.
(2 pots with 5 runs per pot with 5 journeys and 3 cylinders per run per pot of glass)

Gregory, F. G., et al. J. Agr. Sci. 22:617, 1932.
(Split split plot of 4 dates of sowing cotton; the first split was for 3 spacings x 3 irrigations; the second split was for manure and no manure)

Harrington, J. B., Sci. Agr. 21:589, 1941.
(2 varieties each of oats, barley, and wheat in a 6x6 l.s. with split plots of 3 spacings of rows carried on for 4 years)

Immer, F. R., J. Am. Soc. Agron. 34:844, 1942.
(2 whole plots with 4 varieties or crosses per whole plot in 6 replicates)

——, et al., J. Am. Soc. Agron. 26:403, 1934.
(3 replicates and 10 barley varieties grown for 2 years at 6 stations)

Jensen, N. F., Cornell Univ. Agr. Exp. Sta. Mem. 305, 1951.
(2 dusting treatments, 8 replicates, 7 barley varieties)

Jodon, N. E., and Beachell, H. M., J. Am. Soc. Agron. 30:212, 1938.
(16 whole plot treatments of 4 rates of seeding x 4 varieties in 4 replicates with 4 subplot treatments on spacings and rows at 2 locations)

Mitra, N., Indian J. Agr. Sci. 7:459, 1937.
(6 seed treatments per each of 6 replicates with 3 wheat varieties per seed treatment)

Ostle, B., Biometrics 5:71, 1949.
(6 bulls equals whole plots with split plots in a 3x3 l.s. composed of 3

collections per bull = rows, 3 dilution rates = columns, and 3 groups of insemination = treatments)

Paterson, D. D., J. Agr. Sci. 23:615, 1933.
(3 cutting dates on 3 treatments on tropical fodder grasses in 8 replicates)

——, Trop. Agr. 10:267, 303, 346, 1933.
(2 cotton varieties in 4 replicates for 3 years; 2 methods of cultivation on corn in 2 replicates with 3 rates of seeding as the split plot)

——, and Hanschell, D. M., Trop. Agr. 15:199, 1938.
(5 varieties of sugar cane in 5 replicates with 2 spacings as the subplots)

Peterson, M. L., J. Am. Soc. Agron. 39:412, 1947.
(2 grazing treatments in 3 replicates for 5 years)

Quinby, J. R., et al., J. Am. Soc. Agron. 29:269, 1937.
(19 cotton varieties in 4 replicates for 3 years)

Rau, A. A., Unpubl. M. S. thesis, Iowa State College, 1948.
(Perennial experiment on coffee)

Rothamsted Exp. Sta. Report, 1927-1934.
(Several examples of split plot designs and variations)

Russell, C. S., Biometrika 33:213, 1945.
(2 readings on each of several patients on measurement of puerperal uterus)

Russell, G. A., and Little, V. A., J. Am. Soc. Agron. 38:646, 1946.
(2^3 fertilizers in 4 replicates with 3 dates of harvest from plot on devil's shoestring)

Salisbury, G. W., et al., J. Animal Sci. 1:199, 1942.
(Two times on 4 bulls with 4 methods of making semen smears)

Sen, H. D., Indian J. Agr. Sci. 10:172, 1940.
(9 manures as whole plot in 6 replicates with 3 sugar cane varieties as subplots)

Singh, M., Emp. J. Exp. Agr. 18:190, 1950.
(Confounding in split plot designs;
3^3x2 and 3^4x2 on potatoes)

Snedecor, G. W., and Haber, E. S., Biometrics 2:61, 1946.
(6 replicates of 3 cutting dates on asparagus for 10 years)

Taylor, S. A., Unpubl. Ph. D. thesis, Cornell Univ., 1949.
(6 treatments of soil aeration in 8 replicates at 2 dates)

Thomas, W. D., et al., Colo. Agr. Exp. Sta. Misc. Series 435, 1949.
(4 replicates of 9 pinto bean varieties at 3 dates)

Turner, P. E., Trop. Agr. 12:320, 1935.
(8x8 l.s. on 2x4 fertilizer treatments with each row of the l.s. divided into 3 strip plot treatments)

Tysdal, H. M., J. Am. Soc. Agron. 27:384, 1935.
(3 alfalfa varieties in 3 replicates for 4 years)

Watson, S. J., and Ferguson, W. S., J. Agr. Sci. 27:67, 1937.
(2 silos with 4 loads per silo and 3 samples per load per silo)

Wenger, L. E., J. Am. Soc. Agron. 33:135, 1941.
(5 seed soaking treatments on buffalo grass with 3 dates of planting)

Wilm, H. G., Biometrics 1:16, 1945.
(5 treatments in 4 replicates for 3 years)

Wishart, J., and Clapham, A. R., J. Agr. Sci. 19:600, 1929.

(9 whole plot treatments on potatoes with 3 replicates and 2 sub-plot treatments)

, and Hines, H. J. G., J. Ministry Agr. 36:524, 1929.
(4 fertilizers on meadow hay in 4x4 l.s. for 3 years)

——, and Sanders, H. G., Emp. Cotton Growing Corp., 1936.
(6 replicates of 3 mechanical treatments with 4 manurial treatments as the subplots)

Wood, R. C., and Paterson, D. D., Trop. Agr. 9:14, 1932.
(15 varieties of sugar cane in 4 replicates for 4 years)

Yates, F., J. Roy. Stat. Soc., Suppl. 2:181, 1935.
(3 varieties as the whole plot and 4 nitrogen levels as the split plot in 6 replicates)

-, and Cochran, W. G., J. Agr. Sci. 28:556, 1938.
(Several locations and several years' summary of 5 varieties in a r.c.b. design)

Youden, W. J., and Zimmerman, P. W., Contr. Boyce Thompson Inst. 8:317, 1936.
(8 whole plots on 5 fibre pots in a 5x5 l.s. with 2^3 treatments as the split plots; tomatoes)

Numerical examples of split plot designs may also be found in various textbooks; e.g., Goulden, Methods of Statistical Analysis (p. 155); Hayes and Immer, Methods of Plant Breeding (ch. XX); Leonard and Clark, Field Plot Technique (ch. XVIII); Love, Experimental Methods in Agricultural Research (p. 94); Paterson, Statistical Technique in Agricultural Research (pp. 190, 197, 211); Snedecor, Statistical Methods (pp. 305, 310, 314, 414); and Yates, Design and Analysis of Factorial Experiments (p. 74).

CHAPTER XI - INCOMPLETE BLOCK DESIGNS - GENERAL CONSIDERATIONS AND THE ONE-RESTRICTIONAL LATTICES WITH TREATMENTS IN COMPLETE REPLICATES

PROBLEMS

1. Give the analysis for replicates II and III in table XI-14.

2. Give the analysis for replicates I, III, and IV in table XI-18.

3. Give the analysis for replicates I, II, and III in table XI-10.

4. Analyze the balanced lattice in example XI-4 as a quadruple lattice. Show algebraically that the four adjustments in a quadruple lattice are equivalent to the single adjustment in table XI-19 for $k = 3$.

5. Give Rao's method of analysis for the data in examples XI-2, XI-3, and XI-4.

6. Analyze examples XI-2, XI-3, and XI-4 by the method described in chapter IX.

7. Give the analysis for replicates II and III in table XI-21.

8. Make up a 2^3 three-dimensional lattice with 4 and with 5 replicates and with $k = 2$ from the uniformity trial data in table XI-22. Complete the analysis of both experiments.

9. The following results on yield (grams of grain) from 4 replicates of an experiment designed as a double lattice were obtained for 16 barley varieties (nos. 25 to 40) by Henderson (Virginia Polytechnic Inst., 1950, unpublished results):

Rep I					Rep II				
25	26	27	28		25	26	27	28	
2323	2470	2223	2394	9410	2579	2767	2329	1966	9641
29	30	31	32		29	30	31	32	
2959	3560	3248	2687	12454	2462	2507	2522	2421	9912
33	34	35	36		33	34	35	36	
3184	3021	2694	2791	11690	2967	2634	3178	2950	11729
37	38	39	40		37	38	39	40	
2583	2819	2452	2737	10591	2168	2709	2610	2491	9978
				44145					41260

Rep III

25	29	33	37	
1203	1559	2080	2445	7287
26	30	34	38	
1545	2884	2395	3223	10047
27	31	35	39	
1540	2310	3527	3783	11160
28	32	36	40	
1668	2324	1471	2771	8234
				36728

Rep IV

25	29	33	37	
2009	2369	2410	2474	9262
26	30	34	38	
1951	2138	2644	1694	8427
27	31	35	39	
1497	2238	1963	2138	7836
28	32	36	40	
1781	1808	2658	2109	8356
				33881

Complete the analysis of the data and write a short interpretation of the results.

10. Henderson (Virginia Polytechnic Inst., 1947, unpublished results) conducted an experiment on the yield of 12 tobacco varieties. The design was a 3x4 rectangular lattice in 6 replicates. The following yields were obtained:

Group X — Rep I

			Block totals
10	11	12	
770	560	529	1859
1	2	3	
677	576	584	1837
7	8	9	
297	546	550	1393
4	5	6	
625	674	411	1710
			6799

Group Y — Rep III

			Block totals
1	7	4	
648	352	683	1683
10	8	5	
458	631	422	1511
11	2	6	
384	581	434	1399
12	3	9	
682	614	516	1812
			6405

Group Z — Rep V

			Block totals
2	9	5	
663	675	732	2070
12	7	6	
714	240	586	1540
10	3	4	
533	820	748	2101
11	1	8	
675	727	619	2021
			7732

Rep II

			Block totals
10	11	12	
809	554	762	2125
1	2	3	
704	629	616	1949
7	8	9	
472	653	685	1810
4	5	6	
662	677	479	1818
			7702

Rep IV

			Block totals
1	7	4	
770	450	631	1851
10	8	5	
439	642	470	1551
11	2	6	
652	675	540	1867
12	3	9	
833	658	730	2221
			7490

Rep VI

			Block totals
2	9	5	
744	744	653	2141
12	7	6	
671	337	409	1417
10	3	4	
435	646	653	1734
11	1	8	
396	541	510	1447
			6739

Complete the analysis and write a short interpretation of the results.

11. Henderson (Virginia Polytechnic Inst., 1950, unpublished results) conducted a yield test of 12 Burley tobacco varieties. A near balance 3x4 rectangular lattice design with four replicates was used. The following data were obtained for the early harvesting and for the late harvesting:

Early Harvesting

Rep I

			Block totals
19	21	20	
447	497	515	1459
22	24	23	
509	549	588	1646
17	18	16	
565	482	541	1588
13	15	14	
517	642	356	1515
			6208

Rep II

			Block totals
20	13	23	
521	464	598	1583
24	17	14	
565	579	419	1563
22	16	19	
485	593	542	1620
18	21	15	
538	503	617	1658
			6424

Rep III

			Block totals
13	22	18	
556	534	500	1590
17	21	23	
569	388	482	1439
19	15	24	
428	406	530	1364
14	16	20	
428	507	448	1383
			5776

Rep IV

			Block totals
14	21	22	
498	502	539	1539
19	17	13	
441	556	528	1525
20	18	24	
564	590	507	1661
15	16	23	
488	540	515	1543
			6268

Late Harvesting

Rep I

			Block totals
5	8	7	
639	644	542	1825
6	1	11	
687	677	815	2179
4	2	3	
542	813	728	2083
12	10	9	
584	622	729	1935
			8022

Rep II

			Block totals
4	12	8	
481	566	629	1676
10	6	2	
585	670	735	1990
9	5	1	
760	635	674	2069
3	7	11	
718	613	766	2097
			7832

Rep III			Block totals		Rep IV			Block totals
4	11	9			8	11	10	
488	701	637	1826		657	688	622	1967
6	12	7			6	4	5	
635	612	495	1742		771	542	570	1883
5	10	3			7	2	9	
539	553	689	1781		559	555	605	1719
8	1	2			1	12	3	
536	603	530	1669		492	477	607	1576
			7018					7145

Complete the analysis for each harvesting. Write a short interpretation of the results.

12. Gish (Virginia Polytechnic Inst., 1949, unpublished results) conducted an experiment on 9 top dressings of fertilizer on alfalfa. The design was a 3x3 balanced lattice. The following yields of cured hay (pounds per 50 square feet) were obtained:

Reps	I		II		III		IV	
	6	6.73	3	3.61	8	8.34	1	7.81
	5	6.78	9	6.78	4	8.36	6	7.35
	4	8.21	6	6.40	3	3.48	8	8.56
	9	6.89	8	7.57	5	6.07	7	6.11
	7	6.01	2	7.75	1	7.67	3	4.82
	8	9.13	5	7.23	9	6.51	5	6.82
	1	7.39	7	6.11	2	7.57	9	6.11
	2	6.04	4	7.94	6	6.38	4	8.31
	3	3.87	1	7.66	7	5.96	2	7.03
Rep totals		61.05		61.05		60.34		62.92

Complete the analysis of variance and write a short interpretation of the results.

LITERATURE CITATIONS

Atkins, R. E., Unpubl. Ph. D. thesis, Iowa State College, 1948.
(8^3 cubic lattice in 3 replicates on oat varieties)

Chan-Choong, P. A., Agr. J. British Guiana 10:78, 1939.
(13x13 quasifactorial on a rice variety trial)

Comstock, R. E., et al., J. Animal Sci. 7:320, 1948.
(3x3 balanced lattice on swine with littermates forming an incomplete block)

Cornish, E. A., Ann. Eugenics 10:137, 1940.

(Illustrates method of estimating missing values in Dawson's cubic lattice and Yates' lattice square)

Dawson, C. D. R., Ann. Eugenics 9:157, 1939.
($v = 27$, $k = 3$, $r = 3$, $b = 27$ on corn; 4 characters)

Federer, W. T., Biometrics 5:144, 1949.
($v = 27$, $k = 3$, $r = 4$ on corn uniformity data)

——, Cornell Univ. Agr. Exp. Sta. Memoir 299, 1950.
($v = 3^4$ "varieties" on corn uniformity trial data in blocks of $k = 3$ with $r = 4$ and with $r = 6$ replicates)

——, and Robson, D. S., Cornell Univ. Agr. Exp. Sta. Memoir 309, 1952.
($v = 2^5$ "varieties" on corn uniformity trial data in blocks of $k = 4$ with $r = 6$ replicates)

Galinat, W. C., and Everett, H. L., Agron. J. 41:443, 1949.
(9 varieties of sweet corn, $k = 3$, judges compared sweet corn for 3 varieties at a time, $r = 4$)

Goulden, C. H., Sci. Agr. 25:115, 1944.
($k^2 = 25$, $k = 5$, $r = 4$, quadruple lattice; $k^2 = 16$, $k = 4$, $r = 4$, simple lattice; $k^2 = 25$, $k = 5$, $r = 6$, balanced lattice)

Harshbarger, B., Ann. Math. Stat. 15:307, 1944; 16:387, 1945.
(6x6 lattice in 4 groups, $r = 8$; corn)

——, Va. Agr. Exp. Sta. Memoir 1:1, 1947.
(6x5 simple rectangular lattice in 6 replicates)

——, Biometrics 5:1, 1949.
(4x3 triple rectangular lattice, 6 replicates; alfalfa varieties)

Homeyer, P. G., et al., Iowa Agr. Exp. Sta. Res. Bul. 347, 1947.
(Triple lattice, $k^2 = 81$, $r = 6$, corn; simple lattice, $k^2 = 64$, $r = 4$, corn; balanced lattice, $k^2 = 25$, $r = 6$, corn; cubic lattice, $k^3 = 64$, $r = 9$, cotton)

James, E., and Bancroft, T. A., Agron. J. 43:96, 1951.
(2^3 factorial in a balanced incomplete design of 7 replicates with incomplete blocks of size $k = 2$)

Nair, K. R., Biometrics 8:122, 1952.
(2 associate class p.b.i.b. design = simple lattice with $v = 25$ and $r = 2$)

——, and Mahalanobis, P. C., Indian J. Agr. Sci. 10:663, 1940.
(100 varieties of rice in simple lattice in 4 replicates but replicates were not together in one area as is the usual case)

Robinson, H. F., and Watson, G. S., N. C. Agr. Exp. Sta. Tech. Bul. 88, 1949.
(7x8 simple rectangular lattice in 6 replicates, cotton; 7x6 triple rectangular lattice in 3 replicates, corn; estimation of missing values in rectangular lattices)

Wellhausen, E. J., J. Am. Soc. Agron. 35:66, 1943.
(3x3 balanced lattice in 4 replicates on corn)

Yates, F., J. Agr. Sci. 26:424, 1936.
(7x7 simple lattice and 4^3 cubic lattice on orange tree uniformity data without recovery of inter-block information)

——, Ann. Eugenics 7:319, 1937.
(5x5 lattice square in 3 replicates on orange tree uniformity data without recovery of inter-block information)

——, Ann. Eugenics 9:136, 1939.

(4^3 cubic lattice with no complete replicates on orange tree uniformity data with recovery of inter-block information)

——, J. Agr. Sci. 30:672, 1940.
(5x5 lattice square in 3 replicates on sugar beets)

Zuber, M. S., J. Am. Soc. Agron. 34:30, 1942.
(5x5 lattice square in 3 replicates on corn uniformity data)

In addition to the above references, numerical examples may be found in various textbooks, e.g., Anderson and Bancroft, Statistical Theory in Research (ch. 19); Cochran and Cox, Experimental Designs (ch. 10); Goulden, Methods of Statistical Analysis (pp. 184, 189, 195, 207); Hayes and Immer, Methods of Plant Breeding (ch. XXII); Leonard and Clark, Field Plot Technique (ch. XX); and Love, Experimental Methods in Agricultural Research (p. 128).

CHAPTER XII - LATTICE DESIGNS WITH MORE THAN ONE RESTRICTION ON THE ALLOCATION OF TREATMENTS IN THE COMPLETE BLOCK

PROBLEMS

1. An experiment was set up to test 7 varieties of corn with 7 different spacings. Given that 4 replicates are to be used give the possible experimental designs with a key-out of the degrees of freedom for each design. Discuss briefly the applicability of each experimental design listed.

2. An experimenter desires to compare 119 varieties and either 2 or 6 check varieties. From previous experience it is known that the coefficient of variation from experiments designed as randomized complete blocks is usually between 7 and 11 percent. What type of design would you recommend? Why? If the coefficient of variation were 25 percent which design or designs would probably be the most appropriate? Why?

3. Complete the analysis for the data from replicates III and IV in table XII-3.

4. Complete the analysis for replicates I, II, and III in table XII-3.

5. Complete the analysis for replicates IV, V, and VI in table XII-16 by both methods; i.e., by the method of pseudo-effects and by the method of row and column totals.

6. Using the results in chapter IX complete the analysis for replicates I, II, III, and IV in table XII-3. Show arithmetically and algebraically that the 4 row adjustments are equivalent to the single adjustment $\lambda L'$, and show that the 4 column adjustments are equivalent to $\mu M'$.

LITERATURE CITATIONS

Bliss, C. I., and Dearborn, R. B., Proc. Am. Soc. Hort. Sci. 41:324, 1942.
(5x5 lattice square in 3 replicates on corn; missing plot formula; F test of adjusted means)

Boyd, L. L., Unpubl. M. S. thesis, Iowa State College, 1948.
(7x7 lattice square, 8 replicates on 49 types of nails in blocks of wood)

Cochran, W. G., Iowa Agr. Exp. Sta. Res. Bul. 318:731, 1943.
(7x7 lattice square in 3 replicates; plan for an 8x8 lattice square in 4 replicates)

Cornish, E. A., J. Australian Inst. Agr. Sci. 7:19, 1941.
(Estimates a missing row or column or treatment in a lattice square; Yates' 5x5 example)

———, Council Sci. and Ind. Res. Bul. (Australia) No. 175, 1944.
(Recovery of inter-block information in lattice square experiments with incomplete data)

Day, B., and Austin, L., J. Agr. Res. 59:101, 1939.
(Plan for $k^3 = 729$ entries in k blocks of k of k in 3 replicates)

Federer, W. T., Unpubl. results, 1949.
(kxk lattice square with split plots of k plots in 3 and 6 replicates; k = 2 and 5 on corn uniformity data)

———, Biometrics 6:34, 1950.
(6x6 incomplete lattice square with 3 and 6 replicates on corn uniformity data)

Green, J. M., Unpubl. Ph. D. thesis, Iowa State College, 1947.
(512 varieties of corn in 8 blocks of 8 split into 8 varieties in 6 replicates)

Homeyer, P. G., et al., Iowa Agr. Exp. Sta. Res. Bul. 347, 1947.
(Lattice squares with $k^2 = 121$, $k = 11$, $r = 6$, and $k^2 = 25$, $k = 5$, $r = 6$)

Kempthorne, O., J. Agr. Sci. 37:156, 1947.
(5x5 lattice square with 3 replicates; 22 treatments plus 3 controls on potatoes; each plot split into 4 plots with either n, p, k, or npk, or (1), np, nk, pk on the 4 plots)

Weiss, M. G., and Cox, G. M., Iowa Agr. Exp. Sta. Res. Bul. 257:291, 1939.
(31 soybean varieties, $k = 6$, $r = 6$, $b = 31$; 49 soybean varieties in a lattice square of 4 replicates; neither analysis recovers inter-block information)

In addition to the above references, numerical examples may be found in various textbooks; e.g., Cochran and Cox, Experimental Designs (ch. 12); and Goulden, Methods of Statistical Analysis, 2nd ed. (p. 293).

CHAPTER XIII - OTHER INCOMPLETE BLOCK DESIGNS

PROBLEMS

1. For the 10 treatments in example XIII-1 treatments 3, 4, 5, 6, 7, 8, 9, and 10 are denoted as 000, 001, 010, 011, 100, 101, 110, and 111, respectively. Partition the adjusted treatment sum of squares into the following contrasts:

Contrast	df	Contrast	df
G (powder)	1	GP	1
M (moisture)	1	MP	1
GM	1	GMP	1
P (pack)	1	2^3 set versus checks	1
		Checks	1

Test the hypothesis of no effect for each contrast. What other set of contrasts is indicated by the selection of the particular 10 treatments (see the original paper)? Obtain the sums of squares for the various contrasts. Give your interpretation for both analyses.

2. For the data of example XIII-1 obtain the treatment means adjusted for intrablock information only. Compute the appropriate standard error of a mean and the standard error of a difference between two means adjusted without recovery of interblock information. How many degrees of freedom are associated with these standard errors?

3. Partition the sum of squares for treatments (eliminating block) in example XIII-2 into 8 individual comparisons, each with one degree of freedom. Give an interpretation of the results

4. Given that the linear model for the design in section XIII-4.1 is $X_{ijh} = \mu + \rho_j + \tau_i + \alpha_{ij} + \gamma_{ih} + \beta_{ijh}$, where μ = mean effect, ρ_j = replicate effect, τ_i = whole plot or cross effect, α_{ij} = random error deviation associated with whole plots, γ_{ih} = effect of hth line or selection from ith cross, β_{ijh} = random error deviation associated with ijhth split plot; $j = 1, 2, \ldots, r$; $i = 1, 2, \ldots, g$; and $h = 1, 2, \ldots, k$; h is dependent upon i since h = 1 in cross i has nothing in common with h = 1 in cross i' for $i' \neq i$; derive the standard errors given in formulae (XIII-79) to (XIII-81).

5. Ignore the fact that the data in table XI-18 are in replicates, and assume that the groups of treatments were randomly allotted to the 12 incomplete blocks. Complete the analysis for the data as described in example XIII-1.

6. Assume that there is no grouping of the blocks into compact replicates in table XI-14. Complete the analysis as described in example XIII-2.

LITERATURE CITATIONS

Bliss, C. I., Biometrics 3:69, 1947.
(v = 4(2x2), k = 3 times of injections per day, b = 4 dogs, r = 3 repeated 5 times; balanced incomplete block lattice; v = 4 treatments, k = 2 times of injection, b = 6 dogs, 2 groups of 6 dogs with the whole set repeated 3 times)

Capó, B. G., J. Agr. Univ. Puerto Rico 28:22, 1944.
(v = 8, k = 2, b = 27)

Cornish, E. A., Ann. Eugenics 10:112, 1940.
(v = 9, b = 18, r = 8, λ = 3, with 3 missing values)

——, J. Australian Inst. Agr. Sci. 6:31, 1940.
(Estimates missing blocks and missing treatment for balanced incomplete block designs and for Youden square; v = 9, k = 4, λ = 3)

Moore, W., and Bliss, C. I., J. Econ. Ent. 35:544, 1942.
(v = 7, r = 3, b = 7, k = 3 with split plots of 4 concentrations of chemicals)

Rao, C. R., J. Am. Stat. Assoc. 42:541, 1947.
(Partially balanced incomplete block, v = 20, b = 16, k = 5, r = 4)

Van Rest, E. D., J. Roy. Stat. Soc., Suppl. 4:184, 1937.
(No data but gives experimental plans for 4 treatments in blocks of 2 where the incomplete blocks are halves of a board)

Wishart, J., Imp. Bur. Plant Br. and Genetics, 1940.
(21 barley varieties, r = 5, b = 21, k = 5 without recovery of interblock information)

Yates, F., Ann. Eugenics 7:121, 1936.
(v = 5, k = 2, b = 30, r = 12; 6 plants with 5 leaves each and leaves divided into left and right portions; v = 7, b = 7 litters of rats, k = 4 = litter size, in 4 replicates)

——, Ann. Eugenics 10:317, 1940.
(v = 9, r = 8, k = 4, b = 18, λ = 3 on rat uniformity data; v = 21 tomato varieties, k = 5, r = 5, b = 21, λ = 1)

Youden, W. J., Contr. Boyce Thompson Inst. 9:41, 1937; 11:207, 219, 1940.
(v = 21, k = 5, b = 21 on tobacco; v = 11 talc powders on wheat, k = 6, r = 6, b = 11; v = 21, k = 5, r = 5, b = 21)

In addition to the above references, numerical examples may be found in various textbooks; e.g., Anderson and Bancroft, Statistical Theory in Research (ch. 19); Cochran and Cox, Experimental Designs (ch. 11, 13); Goulden, Methods of Statistical Analysis (pp. 196, 208); Kempthorne, The Design and Analysis of Experiments (sec. 27.5); and Love, Experimental Methods of Agricultural Research (p. 121).

CHAPTER XIV - BALANCED DESIGNS

PROBLEMS

1. Complete the computations suggested in examples 15.22 and 15.23, Snedecor, G. W., Statistical Methods, 1946, and discuss the results of the two experiments involved.

2. Complete the design for the 24 cycles suggested by Cochran, Autrey, and Cannon, J. Dairy Sci. 24:949, 1941.

3. Complete the analysis of the following data (plant height in cm. of tobacco plants for two different dosages of cathode rays on tobacco seed; averages of 20 plants; from an experiment conducted by H. H. Smith, Cornell Univ., 1950, unpublished results):

Part of replicate	Replicate number								Total
	1	2	3	4	5	6	7	8	
Better	C-50	C-66	G-40	C-59	G-41	G-52	C-48	G-47	403
Poorer	G-44	G-49	C-49	G-30	C-49	C-43	G-43	C-59	366
Total	94	115	89	89	90	95	91	106	769

Total for treatment C = 423; total for treatment G = 346.

4. Complete the analysis on the other half of the plots for the experiment described in example XIV-3. Also, run a similar analysis on the yields of wheat, peas, and mangolds. Run a combined analysis on all 4 crops. Do you agree with the changes made in the experiment?

5. Compute the standard errors for the various differences and totals in table XIV-3. Partition the treatment sum of squares into the comparisons A vs B, C vs D, and A + B vs C + D. The latter contrast compares organic with inorganic manures. Discuss the results.

LITERATURE CITATIONS TO CHANGE OVER EXPERIMENTS

Blosser, T. H., and Smith, V. R., J. Dairy Sci. 30:951, 1947.
(2 numerical examples of reversal or switch back designs, 2 treatments)

Braude, R., J. Dairy Sci. 37:45, 1947.
(3 pairs of 2 pigs in a switch-over design)

Brandt, A. E., Iowa Agr. Exp. Sta. Res. Bul. 234:60, 1938.
(3 numerical examples of switch back or reversal designs)

Brouwer, E., and Dijkstra, N. D., J. Agr. Sci. 28:695, 1938.
(2 groups of 4 cows each in a change over design)

Cannon, C. Y., et al., Iowa Agr. Exp. Sta. Res. Bul. 292:101, 1932.
(4 periods, 2 lots of cows and 2 treatments in a double reversal)

Cochran, W. G., et al., J. Dairy Sci. 24:937, 1941.
(Double change over on 18 cows for 3 periods; some missing values)

Donald, H. P., Emp. J. Exp. Agr. 7:32, 1939.
(Switch back, 2 periods, 2 treatments on pigs)

Johnson, B. C., et al., J. Animal Sci. 1:236, 1942.
(3 numerical examples of cross over designs on sheep)

Loosli, J. K., and Lucas, H. L., J. Dairy Sci. 26:291, 1943.
(Double reversal design, 4 Holstein cows and 3 periods)

——, et al., Cornell Univ. Agr. Exp. Sta. Memoir 265, 1944.
(4 periods and 12 cows in 3 4x4 l.s. in a double change over design)

Seath, D. M., J. Dairy Sci. 27:159, 1944.
(4 treatments, 3 periods, 4 groups of 5 cows in a double reversal)

Watson, S. J., and Ferguson, W. S., J. Agr. Sci. 26:189, 1936.
(Change over design, 2 treatments, 4 periods)

Youden, W. J., and Beale, H. P., Contr. Boyce Thompson Inst. 6:437, 1934.
(2 treatments, 8 plants, 6 leaf positions, 2 halves of leaf)

LITERATURE CITATIONS TO ROTATION EXPERIMENTS

Cochran, W. G., J. Roy. Stat. Soc., Suppl. 6:104, 1939.
(4 course rotation on 4 crops with each crop whole plot split into 4 split plots of fertilizers; also a 3 course and 6 course rotation example plus several plans)

Cook, R. L., et al., Soil Sci. Soc. Am., Proc. 10:213, 1945.
(Numerical examples of seven 5-year rotations in 4 replicates)

Finney, D. J., Emp. J. Exp. Agr. 8:111, 1940.
(Systematic design and frequent changes of treatments with an analysis)

Mahalanobis, P. C., Indian J. Agr. Sci. 4:361, 1934.
(Rotation experiment on cotton, groundnut, and juar; systematic scheme with an analysis; also some plans)

Menon, T. V. G., and Bose, S. S., Indian J. Agr. Sci. 7:193, 1937.
(4 course rotation applied in two years with 11 cycles)

Rothamsted Exp. Sta. Reports, 1930-1936.
(Several examples)

Russell, E. J., et al., Fifty years of field experiments at the Woburn Exp. Sta., Longmans, Green and Co., London, 1936, p. 196.
(Complete details of example XIV-3)

Smith, G. E., Mo. Agr. Exp. Sta. Bul. 458:1, 1942.
(Data for various rotations)

Wood, R. C., Trop. Agr. 15:147, 1938.
(Yields from various crops in 4 replicates for years 1930-1938)

CHAPTER XV - SOME ADDITIONAL DESIGNS

PROBLEMS

1. Sir Ronald Fisher in section 36 of his book, The Design of Experiments, gives a problem concerning the complete description of the fourth fascist and of the Welsh lawyer. Specify all traits of the 16 passengers. Is your specification unique or is there more than one way of describing the 16 passengers?

2. Construct the experimental plan for Dunlop's proposed experiment described in J. Agr. Sci. 23:580, 1933. Discuss this design for feeding trials on swine. Would you expect the factors used to interact? Why or why not?

3. In section XV-4 the difference between the first 4 weighings and the last 4 weighings is an estimate of the object a as well as of the A effect. What combination of weighings gives estimates of the weights of the remaining objects? Relate the weights of the objects to the effects in a 2^3 factorial.

4. By a procedure analogous to that described for the design for weighing 7 objects construct the design for weighing 15 very light objects. On the first weighing weigh all 15 objects, On the second weighing weigh 7 objects. Complete the design for the additional 14 weighings. There will be a total of 16 weighings. Set up the relationship between the estimated weights of the 15 objects and the effects in a 2^4 factorial experiment.

5. Using uniformity trial data construct numerical examples and give the analyses for the following: a 4x4 graeco-latin square design, a 5x5 hyper-graeco-latin square design, a 6x6 magic latin square design, and a quasi-latin square design for a 2^3 factorial arrangement in two 4x4 latin square designs (for two different schemes of confounding). Describe the randomization procedure as well as the method of analysis.

LITERATURE CITATIONS

Borden, R. J., Hawaiian Planters' Record 43:73, 1939.
(3 varieties x 3 levels of nitrogen in 4 3x3 graeco-latin squares)

Chen, K. K., et al., J. Pharm. and Exp. Ther. 74:223, 1942.
(12x12 half-plaid l.s.)

Cochran, W. G., and Cox, G. M., Experimental Designs, ch. 8, Wiley, N. Y., 1950.

(2^3 factorial in two 4x4 quasi-latin squares)

Dastur, R. H., and Singh, M., Indian J. Agr. Sci. 12:679, 1942; 13:610, 1943.
(8x8 quasi-latin square of 2^4 treatments; 2^5 in an 8x8 quasi-latin square)

Jacob, W. C., Biometrics 9:157, 1953.
(A 9x9 split-plot half-plaid l.s.)

Rothamsted Exp. Sta. Reports, 1929-1931.
(Plaid l.s.: pp. 102, 117, 1929; 142, 161, 1930; 150, 1931)

Tippett, L. H. C., Manchester Stat. Soc., 1934.
(4x4 hyper-graeco-latin square)

Wishart, J., Archiv für Pflanzenbau 5:561, 1931.
(12x12 plaid l.s., 12 seed treatments; columns contained P_0 and P_1 and rows contained two varieties)

Yates, F., J. Agr. Sci. 23:108, 1933.
(12x12 plaid l.s.; also gives an 8x8 quasi-l.s. design)

——, Imp. Bur. Soil Sci., Tech. Comm. 35, 1937 (sec. 8).
(2^3 factorial arrangement on sugar beet uniformity trial data in two 4x4 quasi-l.s. with columns interlaced)

CHAPTER XVI - COVARIANCE

PROBLEMS

1. The following artificial example of a 3x3 latin square design has been constructed for ease of computation:

Row number	Column number						Total		Mean	
	1		2		3					
	Y	X	Y	X	Y	X	Y	X	Y	X
1	B23	6	A17	5	C29	4	69	15	23	5
2	A16	15	C25	10	B16	8	57	33	19	11
3	C24	15	B18	15	A12	12	54	42	18	14
Total	63	36	60	30	57	24	180	90	—	—
Mean	21	12	20	10	19	8	20	10	—	—

Calculate the analysis of variance on the Y variate and on the X variate; calculate the analysis of covariance with Y as the dependent variate; compute tests of significance, the various standard errors, and the adjusted treatment (A, B, C) means; graph the results; compare the regression of the treatment means with the average error regression; discuss the results.

2. Delete the yield in the 3rd row and second column for the Y variate in the previous example. Compute the missing plot value as described in chapter VI. Using the method described in example XVI-2 obtain the analysis of variance of the results. Are the resulting tests of significance exact? Are the resulting tests of significance using a missing plot analysis exact? Why or why not?

3. Given the following data from example XVI-1, use the differences to compute the sum of products for steriles vs non-steriles (S) and for the interaction of progenies (P) with the S effect:

P	Steriles $(S)_1$		Non-steriles $(S)_0$		$(S)_1 - (S)_0$ = difference	
	Y	X	Y	X	Y	X
Progeny 1	(A) 42.5	102	(F) 31.1	112	11.4	- 10
2	(B) 49.3	98	(C) 42.5	102	6.8	- 4
3	(E) 45.4	101	(D) 45.7	100	- 0.3	1
Total	137.2	301	119.3	314	17.9	- 13

4. Data on resin percentage and shrub weight (grams) of plants were obtained for the varieties and replicates of example V-2. Compare the varieties for resin percentage corrected for shrub weight. Discuss the results with regard to transformation of the data, applicability of a covariance analysis, suitability of the regression used, and differences among varieties.

Plot number, variety number, resin percentage, and shrub weight of 2 randomly selected plants from each of 7 guayule varieties grown in five randomized complete blocks

[a] 7-130 5.24- 61 5.84- 50	6-406 4.85- 84 5.64- 91	5-593 5.99- 86 5.22- 83	4-109 3.97- 34 6.17-103	3-416 5.50-65 4.78-53	2-405 4.49- 88 3.36-143	1-407 5.74- 39 5.71-102
8-109 5.71- 58 6.15-109	9-593 5.15- 89 5.69- 76	10-405 5.15- 28 5.10-146	11-406 4.86- 67 5.65-110	12-416 6.00- 96 5.43-104	13-130 5.49-152 5.54-106	14-407 6.15- 88 5.76- 92
21-593 5.88- 58 5.59-101	20-407 4.88- 61 5.97- 93	19-406 5.82-125 5.20-112	18-416 4.75-125 6.15- 91	17-130 5.60- 95 5.76- 73	16-405 6.22-111 4.85- 15	15-109 6.05- 87 6.04- 84
22-130 6.13- 70 5.63-116	23-109 5.64-101 6.65- 12	24-405 7.24-115 5.95-170	25-416 6.30-101 5.75- 15	26-593 5.36- 99 5.21-105	27-407 6.20-127 6.74-194	28-406 5.17-104 6.62-117
35-593 5.80- 93 6.13- 73	34-130 5.78- 98 5.38-102	33-416 5.85- 8 5.73- 33	32-405 5.67-132 4.59- 83	31-406 5.86-144 5.75- 98	30-109 3.85-105 5.89- 88	29-407 6.18-169 5.54- 90

[a] First number = plot number; second number = varietal designation, and last two pairs of numbers equal percentage of resin and dry weight of shrub from the two plants.

5. The following data on leaf width (total in cm. for 20 plants) on tobacco plants are from the experiment described by Federer and Schlottfeldt, Biometrics 10:282, 1954. Complete the analysis of these data in the manner described by the above authors. Compute the gain in efficiency of the covariance analysis over the variance analysis using formula (I-1). Compare your result with that obtained in the original paper. Compute the standard error of a difference between the adjusted totals for A and F.

Covariate X_1	Covariate X_2	Replicate number 1	2	3	4	5	6	7	8	Total
-3	9	F 408.0	B 433.6	A 405.0	F 414.0	F 393.5	G 396.9	B 405.1	D 394.2	3250.3
-2	4	G 353.5	E 340.5	E 358.2	A 356.1	G 321.5	B 323.0	A 343.7	E 367.6	2764.1
-1	1	D 350.0	F 349.5	C 334.6	G 302.6	D 296.2	D 298.9	E 290.2	B 307.7	2529.7
0	0	C 359.8	G 372.2	G 347.1	D 343.8	C 339.2	C 330.5	F 323.7	A 325.7	2742.0
1	1	A 398.7	C 400.1	B 405.4	B 396.3	A 371.7	A 361.5	C 361.8	C 375.3	3070.8
2	4	B 386.7	A 403.4	F 380.5	E 355.4	B 360.4	E 354.2	G 344.0	F 365.8	2950.4
3	9	E 418.8	D 414.8	D 384.9	C 381.0	E 358.1	F 369.1	D 363.2	G 381.2	3071.1
0	28	2675.5	2714.1	2615.7	2549.2	2440.6	2434.1	2431.7	2517.5	20378.4

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(Covariance analyses for rectangular lattices)

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(Covariance in analysis of missing and damaged observations in a l.s.)

In addition to the above citations, examples of the analysis of covariance may be found in most texts on statistics; e.g., Fisher, Statistical Methods for Research Workers (ch. VIII); Snedecor, Statistical Methods (ch. 12, 13); Wishart, Field Trials II. The Analysis of Covariance; etc.