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AMCP 706-112

ENGINEERING DESIGN HANDBOOK

EXPERIMENTAL STATISTICS

SECTION 3

PLANNING AND ANALYSIS OF COMPARATIVE EXPERIMENTS

HEADQUARTERS, U.S. ARMY MATERIEL COMMAND

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ENGINEERING DESIGN HANDBOOK

EXPERIMENTAL STATISTICS (SEC 3)

CHAPTER 11

GENERAL CONSIDERATIONS IN PLANNING EXPERIMENTS

CHAPTER 12

FACTORIAL EXPERIMENTS

*This pamphlet supersedes AMCP 706-112, 30 April 1965.

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RANDOMIZED BLOCKS, LATIN SQUARES, AND OTHER SPECIAL-PURPOSE DESIGNS

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FOREWORD

INTRODUCTION

This is one of a group of handbooks covering the engineering information and quantitative data needed in the design, development, construction, and test of military equipment which (as a group) constitute the Army Materiel Command Engineering Design Handbook.

PURPOSE OF HANDBOOK

The Handbook on Experimental Statistics has been prepared as an aid to scientists and engineers engaged in Army research and development programs, and especially as a guide and ready reference for military and civilian personnel who have responsibility for the planning and interpretation of experiments and tests relating to the performance of Army equipment in the design and developmental stages of production.

SCOPE AND USE OF HANDBOOK

This Handbook is a collection of statistical procedures and tables. It is presented in five sections, viz:

AMCP 706-110, Section 1, Basic Concepts and Analysis of Measurement Data (Chapters 1-6)

AMCP 706-111, Section 2, Analysis of Enumerative and Classificatory Data (Chapters 7-10)

AMCP 706-112, Section 3, Planning and Analysis of Comparative Experiments (Chapters 11-14)

AMCP 706-113, Section 4, Special Topics (Chapters 15-23)

AMCP 706-114, Section 5, Tables

Section 1 provides an elementary introduction to basic statistical concepts and furnishes full details on standard statistical techniques for the analysis and interpretation of measurement data. Section 2 provides detailed procedures for the analysis and interpretation of enumerative and classificatory data. Section 3 has to do with the planning and analysis of comparative experiments. Section 4 is devoted to consideration and exemplification of a number of important but as yet non-standard statistical techniques, and to discussion of various other special topics. An index for the material in all four sections is placed at the end of Section 4. Section 5 contains all the mathematical tables needed for application of the procedures given in Sections 1 through 4.

An understanding of a few basic statistical concepts, as given in Chapter 1, is necesssary; otherwise each of the first four sections is largely independent of the others. Each procedure, test, and technique described is illustrated by means of a worked example. A list of authoritative references is included, where appropriate, at the end of each chapter. Step-by-step instructions are given for attaining a stated goal, and the conditions under which a particular procedure is strictly valid are stated explicitly. An attempt is made to indicate the extent to which results obtained by a given procedure are valid to a good approximation when these conditions are not fully met. Alternative procedures are given for handling cases where the more standard procedures cannot be trusted to yield reliable results.

The Handbook is intended for the user with an engineering background who, although he has an occasional need for statistical techniques, does not have the time or inclination to become an expert on statistical theory and methodology.

The Handbook has been written with three types of users in mind. The first is the person who has had a course or two in statistics, and who may even have had some practical experience in applying statistical methods in the past, but who does not have statistical ideas and techniques at his fingertips. For him, the Handbook will provide a ready reference source of once familiar ideas and techniques. The second is the person who feels, or has been advised, that some particular problem can be solved by means of fairly simple statistical techniques, and is in need of a book that will enable him to obtain the solution to his problem with a minimum of outside assistance. The Handbook should enable such a person to become familiar with the statistical ideas, and reasonably adept at the techniques, that are most fruitful in his particular line of research and development work. Finally, there is the individual who, as the head of, or as a member of a service group, has responsibility for analyzing and interpreting experimental and test data brought in by scientists and engineers engaged in Army research and development work. This individual needs a ready source of model work sheets and worked examples corresponding to the more common applications of statistics, to free him from the need of translating textbook discussions into step-by-step procedures that can be followed by individuals having little or no previous experience with statistical methods.

It is with this last need in mind that some of the procedures included in the Handbook have been explained and illustrated in detail twice: once for the case where the important question is whether the performance of a new material, product, or process exceeds an established standard; and again for the case where the important question is whether its performance is not up to the specified standards. Small but serious errors are often made in changing "greater than" procedures into "less than" procedures.

AUTHORSHIP AND ACKNOWLEDGMENTS

The Handbook on Experimental Statistics was prepared in the Statistical Engineering Laboratory, National Bureau of Standards, under a contract with the Department of Army. The project was under the general guidance of Churchill Eisenhart, Chief, Statistical Engineering Laboratory.

Most of the present text is by Mary G. Natrella, who had overall responsibility for the completion of the final version of the Handbook. The original plans for coverage, a first draft of the text, and some original tables were prepared by Paul N. Somerville. Chapter 6 is by Joseph M. Cameron; most of Chapter 1 and all of Chapters 20 and 23 are by Churchill Eisenhart; and Chapter 10 is based on a nearly-final draft by Mary L. Epling.

Other members of the staff of the Statistical Engineering Laboratory have aided in various ways through the years, and the assistance of all who helped is gratefully acknowledged. Particular mention should be made of Norman C. Severo, for assistance with Section 2, and of Shirley Young Lehman for help in the collection and computation of examples.

Editorial assistance and art preparation were provided by John I. Thompson & Company, Washington, D. C. Final preparation and arrangement for publication of the Handbook were performed by the Engineering Handbook Office, Duke University.

Appreciation is expressed for the generous cooperation of publishers and authors in granting permission for the use of their source material. References for tables and other material, taken wholly or in part, from published works, are given on the respective first pages.

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Comments and suggestions on this handbook are welcome and should be addressed to Army Research Office-Durham, Box CM, Duke Station, Durham, North Carolina 27706.

PREFACE

This listing is a guide to the Section and Chapter subject coverage in all Sections of the Handbook on Experimental Statistics.

DISCUSSION OF TECHNIQUES IN CHAPTERS 11 THROUGH 14

In this Section, we attempt to give only the following coverage:

(1) some broad consideration to the planning of experiments, in Chapter 11;

(2) some examples of the more widely used experimental designs, with appropriate methods of analysis, in Chapters 12 and 13;

(3) a brief description of new techniques that are useful when the purpose of experimentation is that of seeking maximum or optimum levels of the experimental factors, in Chapter 14.

Excellent books are available to give more extensive catalogs of experimental designs and more details regarding precautions in applying and analyzing these designs. A list of recommended books is given at the end of Chapter 11. When actually faced with the problem of planning an experiment, however, books will not be sufficient. The planning of experiments cannot be done in an ivory tower; and does not consist merely of writing down a few key words or parameters, looking them up in an index, and then selecting a specific plan. The proper experimental plan depends on: the purpose of the experiment; physical restrictions on the process of taking measurements; and other restrictions imposed by limitations of time, money, and the availability of material and personnel, etc. The novice experimenter is advised to consult a competent statistician and give him all the information available — not only what is thought to be important, but also what may be thought to be unimportant. In the words of Cochran and Cox*:

> Participation in the initial stages of experiments in different areas of research leads to a strong conviction that too little time and effort is put into the planning of experiments. The statistician who expects that his contribution to the planning will involve some technical matter in statistical theory finds repeatedly that he makes a much more valuable contribution simply by getting the investigator to explain clearly why he is doing the experiment, to justify the experimental treatments whose effects he proposes to compare, and to defend his claim that the completed experiment will enable its objectives to be realized. . . .
>
> It is good practice to make a written draft of the proposals for any experiment. This draft will in general have three parts: (i) a statement of the objectives; (ii) a description of the experiment, covering such matters as the experimental treatments, the size of the experiment, and the experimental material; and (iii) an outline of the method of analysis of the results.

In outlining the methods of conducting and analyzing an experiment, Anderson and Bancroft† give the following advice:

> (i) *The experimenter should clearly set forth his objectives before proceeding with the experiment.* Is this a preliminary experiment to determine the future course of experimentation, or is it intended to furnish answers to immediate questions? Are the results to be carried into practical use at once, or are they to be used to explain aspects of theory not adequately understood before? Are you mainly interested in estimates or in tests of significance? Over what range of experimental conditions do you wish to extend your results?
>
> (ii) *The experiment should be described in detail.* The treatments should be clearly defined. Is it necessary to use a control treatment in order to make comparisons with past results? The size of the experiment should be determined. If insufficient funds are available to conduct an experiment from which useful results can be obtained, the experiment should not be started. And above all, the necessary material to conduct the experiment should be available.
>
> (iii) *An outline of the analysis should be drawn up before the experiment is started.*

All A-Tables referenced in these Chapters are contained in AMCP 706-114, Section 5.

* W. G. Cochran and G. M. Cox, *Experimental Designs*, (2d edition), p. 10, John Wiley and Sons, Inc., New York, N.Y., 1957.

† R. L. Anderson and T. A. Bancroft, *Statistical Theory in Research*, p. 223, McGraw-Hill Book Co., Inc., New York, N.Y., 1952.

CHAPTER 11

GENERAL CONSIDERATIONS IN PLANNING EXPERIMENTS

11-1 THE NATURE OF EXPERIMENTATION

An experiment has been defined, in the most general sense, as "a considered course of action aimed at answering one or more carefully framed questions." Observational programs in the natural sciences and sample surveys in the social sciences are clearly included in this general definition. In ordnance engineering, however, we are concerned with a more restricted kind of experiment in which the experimenter *does something* to at least some of the things under study and then *observes the effect of his action.*

The things under study which are being deliberately varied in a controlled fashion may be called the *factors*. These factors may be quantitative factors such as temperature which can be varied along a continuous scale (at least for practical purposes the scale may be called continuous) or they may be qualitative factors (such as different machines, different operators, different composition of charge, etc.). The use of the proper *experimental pattern* aids in the evaluation of the factors. See Paragraph 11-2.

In addition to the factors, which are varied in a controlled fashion, the experimenter may be aware of certain background variables which might affect the outcome of the experiment. For one reason or another, these background variables will not be or cannot be included as factors in the experiment, but it is often possible to plan the experiment so that:

(1) possible effects due to background variables do not affect information obtained about the factors of primary interest; and,

(2) some information about the effects of the background variables can be obtained. See Paragraph 11-3.

In addition, there may be variables of which the experimenter is unaware which have an effect on the outcome of the experiment. The effects of these variables may be given an opportunity to "balance out" by the introduction of *randomization* into the experimental pattern. See Paragraph 11-4.

Many books have been written on the general principles of experimentation, and the book by Wilson[1] is especially recommended. There are certain characteristics an experiment obviously must have in order to accomplish anything at all. We might call these *requisites of a good experiment,* and we give as a partial listing of requisites:

(1) There must be a clearly defined objective.

(2) As far as possible, the effects of the factors should not be obscured by other variables.

(3) As far as possible, the results should not be influenced by conscious or unconscious bias in the experiment or on the part of the experimenter.

(4) The experiment should provide some measure of precision.*

(5) The experiment must have sufficient precision to accomplish its purpose.

* This requisite can be relaxed in some situations, i.e., when there is a well-known history of the measurement process, and consequently good *a priori* estimates of precision.

To aid in achieving these requisites, statistical design of experiments can provide some *tools for sound experimentation*, which are listed in Table 11-1.

The tools given include: *experimental pattern, planned grouping, randomization,* and *replication.* Their functions in experimentation are shown in Table 11-1, and are amplified in Paragraphs 11-2 through 11-5.

TABLE 11-1. SOME REQUISITES AND TOOLS FOR SOUND EXPERIMENTATION

Requisites	Tools
1. The experiment should have carefully defined objectives.	1. The definition of objectives requires all of the specialized subject-matter knowledge of the experimenter, and results in such things as: (a) Choice of factors, including their range; (b) Choice of experimental materials, procedure, and equipment; (c) Knowledge of what the results are applicable to.
2. As far as possible, effects of factors should not be obscured by other variables.	2. The use of an appropriate EXPERIMENTAL PATTERN** (see Par. 11-2) helps to free the comparisons of interest from the effects of uncontrolled variables, and simplifies the analysis of the results.
3. As far as possible, the experiment should be free from bias (conscious or unconscious).	3. Some variables may be taken into account by PLANNED GROUPING (see Par. 11-3). For variables not so taken care of, use RANDOMIZATION (Par. 11-4). The use of REPLICATION aids RANDOMIZATION to do a better job.
4. Experiment should provide a measure of precision (experimental error).*	4. REPLICATION (Par. 11-5) provides the measure of precision; RANDOMIZATION assures validity of the measure of precision.
5. Precision of experiment should be sufficient to meet objectives set forth in requisite 1.	5. Greater precision may be achieved by: Refinements of technique EXPERIMENTAL PATTERN (including PLANNED GROUPING) REPLICATION.

* Except where there is a well-known history of the measurement process.

** Capitalized words are discussed in the following paragraphs.

11-2 EXPERIMENTAL PATTERN

The term *experimental pattern* is a broad one by which we mean the planned schedule of taking the measurements. A particular pattern may or may not include the succeeding three tools (*planned grouping, randomization,* and *replication*). Each of these three tools can improve the experimental pattern in particular situations. The proper pattern for the experiment will aid in control of bias and in measurement of precision, will simplify the requisite calculations of the analysis, and will permit clear estimation of the effects of the factors.

A common experimental pattern is the so-called factorial design experiment, wherein we control several factors and investigate their effects at each of two or more levels. If two levels of each factor are involved, the experimental plan consists of taking an observation at each of the 2^n possible combinations. The factorial design, with examples, is discussed in greater detail in Chapter 12.

11-3 PLANNED GROUPING

An important class of experimental patterns is characterized by *planned grouping.* This class is often called *block designs.* The use of planned grouping (blocking) arose in comparative experiments in agricultural research, in recognition of the fact that plots that were close together in a field were usually more alike than plots that were far apart. In industrial and engineering research, the tool of planned grouping can be used to take advantage of naturally homogeneous groupings in materials, machines, time, etc., and so to take account of "background variables" which are not directly "factors" in the experiment.

Suppose we are required to compare the effect of five different treatments of a plastic material. Plastic properties vary considerably within a given sheet. To get a good comparision of the five treatment effects, we should divide the plastic sheet into more or less homogeneous areas, and subdivide each area into five parts. The five treatments could then be allocated to the five parts of a given area. Each set of five parts may be termed a block. In this case, had we had four or six treatments, we could as well have had blocks of four or six units. This is not always the case — the naturally homogeneous area (block) may not be large enough to accommodate all the treatments of interest.

If we are interested in the wearing qualities of automobile tires, the natural block is a block of four, the four wheels of an automobile. Each automobile may travel over different terrain or have different drivers. However, the four tires on any given automobile will undergo much the same conditions, particularly if they are rotated frequently.

In testing different types of plastic soles for shoes, the natural block consists of two units, the two feet of an individual.

The block may consist of observations taken at nearly the same time or place. If a machine can test four items at one time, then each run may be regarded as a block of four units, each item being a unit.

Statisticians have developed a variety of especially advantageous configurations of *block designs,* named and classified by their structure into randomized blocks, Latin squares, incomplete blocks, lattices, etc., with a number of subcategories of each. Some of these block designs are discussed in detail in Chapter 13.

11-4 RANDOMIZATION

Randomization is necessary to accomplish Requisites 3 and 4 in Table 11-1. In order to eliminate bias from the experiment (Requisite 3), experimental variables which are not specifically controlled as factors, or "blocked out" by planned grouping, should be randomized — e.g., the allocations of specimens to treatments or methods should be made by some mechanical method of randomization.

Randomization also assures valid estimates of experimental error (Requisite 4), and makes possible the application of statistical tests of significance and the construction of confidence intervals.

There are many famous examples of experiments where failure to randomize at a crucial stage led to completely misleading results. As always, however, the coin has another side; the beneficial effects of randomization are obtained in the long run, and not in a single isolated experiment. Randomization may be thought of as insurance, and, like insurance, may sometimes be too expensive. If a variable is thought unlikely to have an effect, and if it is very difficult to randomize with respect to the variable, we may choose not to randomize.

In general, we should try to think of all variables that could possibly affect the results, select as factors as many variables as can reasonably be studied, and use planned grouping where possible. Ideally, then, we randomize with respect to everything else — but it must be recognized that the ideal cannot always be realized in practice.

The word *randomization* has been used rather than *randomness* to emphasize the fact that experimental material rarely, if ever, has a random distribution in itself, that we are never really safe in assuming that it has, and that consequently randomness has to be assured by formal or mechanical randomization.

11-5 REPLICATION

In order to evaluate the effects of factors, a measure of precision (experimental error) must be available. In some kinds of experiments, notably in biological or agricultural research, this measure must be obtained from the experiment itself, since no other source would provide an appropriate measure. In some industrial and engineering experimentation, however, records may be available on a relatively stable measurement process, and this data may provide an appropriate measure. Where the measure of precision must be obtained from the experiment itself, *replication* provides the measure. In addition to providing the measure of precision, replication provides an opportunity for the effects of uncontrolled factors to balance out, and thus aids randomization as a bias-decreasing tool. (In successive replications, the randomization features must be independent.) Replication will also help to spot gross errors in the measurements.

11-6 THE LANGUAGE OF EXPERIMENTAL DESIGN

In discussing applications of statistical design of experiments in the field of physical sciences and engineering, we are extremely handicapped by the classical language of experimental design. The early developments and applications were in the field of agriculture, where the terms used in describing the designs had real physical meaning. The *experimental area* was an area — a piece of ground. A *block* was a smaller piece of ground, small enough to be fairly uniform in soil and topography, and thus was expected to give results within a block that would be more alike than those from different blocks. A *plot* was an even smaller piece of ground, the basic unit of the design. As a unit, the plot was planted, fertilized, and harvested, and it could be *split* just by drawing a line. A *treatment* was actually a treatment (e.g., an application of fertilizer) and a *treatment combination* was a combination of treatments. A *yield* was a yield, a quantity harvested and weighed or measured.

Unfortunately for our purposes, these are the terms commonly used. Since there is no particular future in inventing a new descriptive language for a single book, we must use these terms, and we must ask the engineer or scientist to stretch his imagination to make the terms fit his experimental situation.

Experimental area can be thought of as the scope of the planned experiment. For us, a *block* can be a group of results from a particular operator, or from a particular machine, or on a particular day — any planned natural grouping which should serve to make results from one block more alike than results from different blocks. For us, a *treatment* is the factor being investigated (material, environmental condition, etc.) in a single factor experiment. In factorial experiments (where several variables are being investigated at the same time) we speak of a *treatment combination* and we mean the prescribed levels of the factors to be applied to an experimental unit. For us, a *yield* is a measured result and, happily enough, in chemistry it will sometimes be a yield.

Many good books on experimental design are available. See the following list of References and Recommended Textbooks.

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1. E. B. Wilson, Jr., *An Introduction to Scientific Research,* McGraw-Hill Book Co., Inc., New York, N.Y., 1952.

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CHAPTER 12

FACTORIAL EXPERIMENTS

12-1 INTRODUCTION

12-1.1 SOME GENERAL REMARKS AND TERMINOLOGY

Factorial experiment is the name commonly applied to an experiment wherein we control several factors and investigate their effects at each of two or more levels. The experimental plan consists of taking an observation at each one of all possible combinations that can be formed for the different levels of the factors. Each such different combination is called a *treatment combination.*

Suppose that we are interested in investigating the effect of pressure and temperature on the yield of some chemical process. Pressure and temperature will be called the *factors* in the experiment. Each specific value of pressure to be included will be called a *level* of the pressure factor, and similarly each specific value of temperature to be included will be called a *level* of the temperature factor. In the past, one common experimental approach has been the so-called "one at a time" approach. This kind of experiment would study the effect of varying pressure at some constant temperature, and then study the effect of varying temperature at some constant pressure. Factors would be varied "one at a time." The results of such an experiment are fragmentary in the sense that we have learned about the effect of different pressures at one temperature only (and the effect of different temperatures at one pressure only). The reaction of the process to different pressures may depend on the temperature used; if we had chosen a different temperature, our observed relation of yield to pressure may have been quite different. In statistical language, there may be an *interaction* effect between the two factors within the range of interest, and the "one at a time" procedure does not enable us to detect it.

In a factorial experiment, the levels of each factor we wish to investigate are chosen, and a measurement is made for each possible combination of levels of the factors. Suppose that we had chosen two levels, say 7cm. and 14cm. for pressure, and two levels, say, 70°F. and 100°F. for temperature. There would be four possible combinations of pressure and temperature, and the factorial experiment would consist of four trials. In our example, the term *level* is used in connection with quantitative factors, but the same term is also used when the factors are qualitative.

In the analysis of factorial experiments, we speak of *main effects* and *interaction effects* (or simply *interactions*). Main effects of a given factor are always functions of the average response or yield at the various levels of the factor. In the case where a factor has two levels, the *main effect* is the difference between the responses at the two levels averaged over all levels of the other factors. In the case where the factor has more than two levels, there are several independent components of the main effect, the number of components being one less than the number of levels. If the difference in the response between two levels of factor A is the same regardless of the level of factor B (except for experimental error), we say that there is no interaction between A and B, or that the AB interaction is zero. Figure 12-1 shows two examples of response or yield curves; one example shows the presence of an interaction, and the other shows no interaction. If we have two

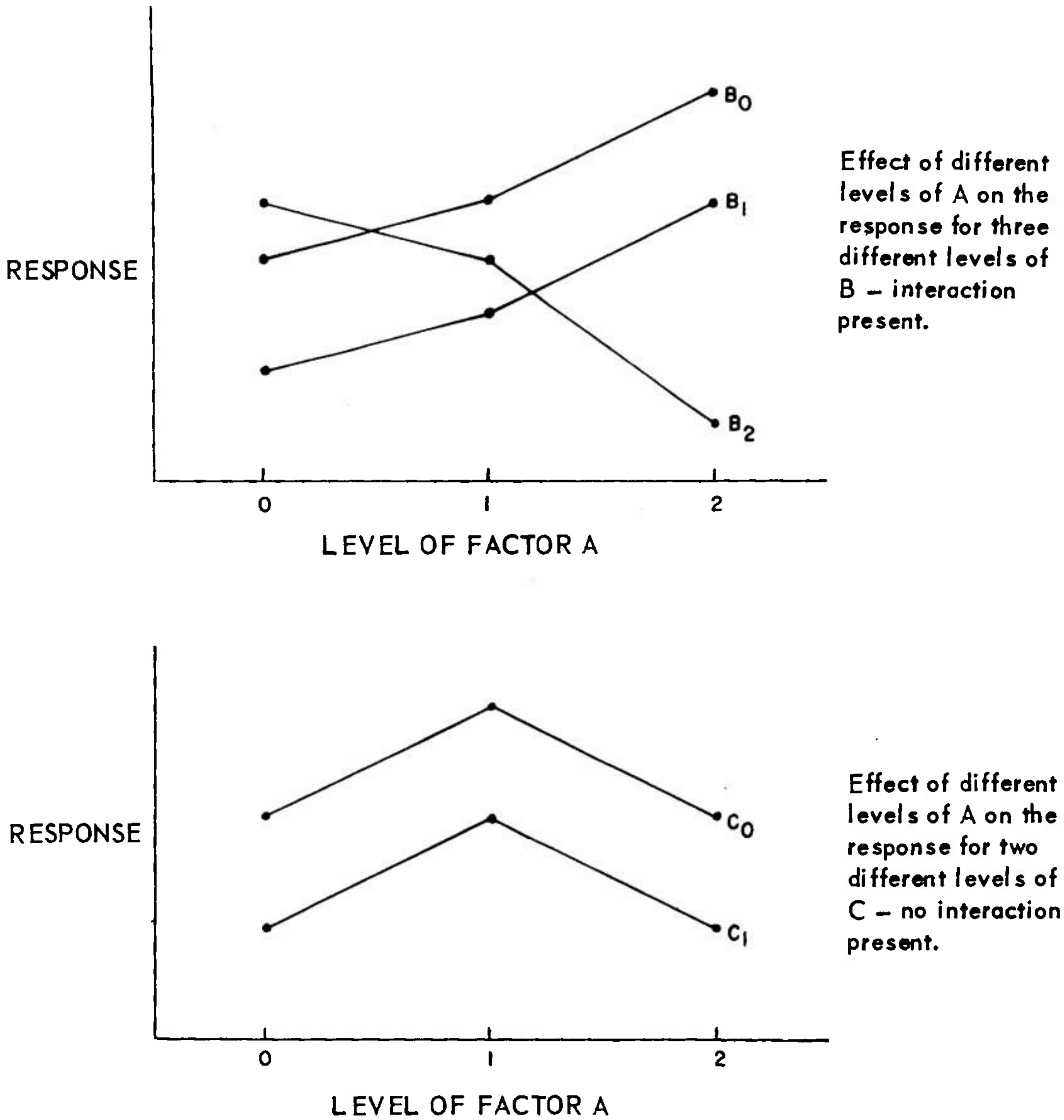

Figure 12-1. Examples of response curves showing presence or absence of interaction.

levels of each of the factors A and B, then the AB interaction (neglecting experimental error) is the difference in the yields of A at the second level of B minus the difference in the yields of A at the first level of B. If we have more than two levels of either or of both A and B, then the AB interaction is composed of more than one component. If we have a levels of the factor A and b levels of the factor B, then the AB interaction has $(a - 1)(b - 1)$ independent components.

For factorial experiments with three or more factors, interactions can be defined similarly. For instance, the ABC interaction is the interaction between the factor C and the AB interaction (or equivalently between the factor B and the AC interaction, or A and the BC interaction).

12-1.2 ESTIMATES OF EXPERIMENTAL ERROR FOR FACTORIAL-TYPE DESIGNS

12-1.2.1 Internal Estimates of Error. As in any experiment, we must have a measure of experimental error to use in judging the significance of the observed differences in treatments. In the larger factorial designs, estimates of higher-order interactions will be available. The usual assumption is that high-order interactions are physically impossible, and that the estimates so labelled are actually estimates of experimental error. As a working rule we often use third- and higher-order interactions for error. This does not imply that third-order interactions are always nonexistent. The judgment of the experimenter will determine which interactions may reasonably be assumed to be meaningful, and which may be assumed to be nothing more than error. These latter interactions may be combined to provide an internal estimate of error for a factorial experiment of reasonable size. For very small factorials, e.g., 2^3 or smaller, there are no estimates of high-order interactions, and the experiment must be replicated (repeated) in order to obtain an estimate of error from the experiment itself.

In the blocked factorial designs (Paragraph 12-3 and Table 12-3), some of the higher-order interactions will be *confounded* with blocks, and will not be available as estimates of error (see Paragraph 12-3.1). For example, note the plan in Table 12-3 for a 2^3 factorial arranged in two blocks of four observations. The single third-order interaction provides the blocking, i.e., the means of subdividing the experiment into homogeneous groups, and therefore will estimate block effects, not error. Here again it may be necessary to replicate the experiment in order to have an estimate of experimental error.

In the case of fractional factorials, there is obviously no point in replication of the experiment; further experimentation would probably be aimed at completing the full factorial or a larger fraction of the full factorial. The smaller fractional factorial designs (Paragraph 12-4 and Table 12-4) do not contain high-order interactions that can suitably be assumed to be error. In fact, none of the particular plans given in Table 12-4 provides a suitable internal estimate of error. Accordingly then, an independent estimate of error will be required when using a small fractional factorial. Occasionally and cautiously we might use second-order interaction effects to test main effects, if the purpose of the experiment were to look for very large main effects (much larger than second-order effects). In using interactions as estimates of error, however, we must decide before conducting the experiment (or at least before having a knowledge of the responses or yields) which of the effects may be assumed to be zero, so that they may be used in the estimate of the variation due to experimental error.

12-1.2.2 Estimates of Error From Past Experience. In the cases discussed in Paragraph 12-1.2.1 that do not provide adequate estimates of error from the experiment itself, we must depend on an estimate based upon past experience with the measurement process. In laboratory and industrial situations, this information is often at hand or can be found by simple analysis of previously recorded data.

12-2 FACTORIAL EXPERIMENTS (EACH FACTOR AT TWO LEVELS)

12-2.1 SYMBOLS

A factorial experiment in which we have n factors, each at two levels, is known as a 2^n factorial experiment. The experiment consists of 2^n trials, one at each combination of levels of the factors. To identify each of the trials, we adopt a conventional notation. A factor is identified by a capital letter, and the two levels of a factor by the subscripts *zero* and *one*. If we have three factors A, B, and C, then the corresponding levels of the factors are A_0, A_1; B_0, B_1; and C_0, C_1; respectively. By convention, the zero subscript refers to the lower level, to the normal condition, or to the absence of a condition, as appropriate. A trial is represented by a combination of small letters denoting the levels of the factors in the trial. The presence of a small letter means that the factor is at the level denoted by the subscript 1 (the higher level for quantitative factors); the absence of a letter means that the factor is at the level denoted by the subscript zero (the lower level for quantitative factors). Thus, the symbol a represents the treatment combination where A is at the level A_1, B is at B_0, and C is at C_0. The symbol bc represents the treatment combination where A is at the level A_0, B is at B_1, and C is at C_1. Conventionally, the symbol (1) represents the treatment combination with each factor at its zero level. In an experiment with three factors, each at two levels, the $2^3 = 8$ combinations, and thus the eight trials, are represented by (1), a, b, ab, c, ac, bc, abc.

Data Sample 12-2 — Flame Tests of Fire-Retardant Treatments

The data are taken from a larger experiment designed to evaluate the effect of laundering on certain fire-retardant treatments for fabrics. The experiment has four factors, each at two levels, i.e., is a 2^4 factorial. Note that all factors are qualitative in this experiment. The experimental factors and levels are:

FACTORS	LEVELS
A — Fabric	A_0 — Sateen A_1 — Monks cloth
B — Treatment	B_0 — Treatment x B_1 — Treatment y
C — Laundering condition	C_0 — Before laundering C_1 — After one laundering
D — Direction of test	D_0 — Warp D_1 — Fill

The observations reported in Table 12-1 are *inches burned,* measured on a standard size sample after a flame test. For reference, the conventional symbol representing the treatment combination appears beside the resulting observation.

TABLE 12-1. RESULTS OF FLAME TESTS OF FIRE-RETARDANT TREATMENTS (FACTORIAL EXPERIMENT OF DATA SAMPLE 12-2)

		A_0				A_1			
		B_0		B_1		B_0		B_1	
C_0	D_0	4.2	(1)	4.5	b	3.1	a	2.9	ab
	D_1	4.0	d	5.0	bd	3.0	ad	2.5	abd
C_1	D_0	3.9	c	4.6	bc	2.8	ac	3.2	abc
	D_1	4.0	cd	5.0	bcd	2.5	acd	2.3	$abcd$

12-2.2 ANALYSIS

12-2.2.1 Estimation of Main Effects and Interactions. Yates' method is a systematic method for obtaining estimates of main effects and interactions *for two-level factorials.* The method was originally described by Yates[1], and may be found in various textbooks (Cochran and Cox[2] and Davies[3]). The method as given here applies to factorials, blocked factorials (Paragraph 12-3), and fractional factorials (Paragraph 12-4), for which we have 2^n observations.* The first step in the Yates' procedure is to make a table with $n + 2$ columns, where n is the number of factors in the factorial experiment. For example, see Table 12-2, where $n + 2 = 6$. In Table 12-2, the treatment combinations are listed in a standardized order in the first column, and after following the prescribed procedure, estimated main effects and interactions result in the last column (column $n + 2$). The order in which the treatment combinations are listed in column 1 determines the order of estimated effects in column $n + 2$.

For factorials or blocked factorials, the treatment combinations should be listed in "standard order" in the first column, i.e.,

For two factors: (1), *a*, *b*, *ab*
For three factors: (1), *a*, *b*, *ab*, *c*, *ac*, *bc*, *abc*
For four factors: (1), *a*, *b*, *ab*, *c*, *ac*, *bc*, *abc*, *d*, *ad*, *bd*, *abd*, *cd*, *acd*, *bcd*, *abcd*
.
.
.
etc.

"Standard order" for five factors is obtained by listing all the treatment combinations given for four factors, followed by *e*, *ae*, *be*, *abe*, . . . , *abcde* (i.e., the new element multiplied by all previous treatment combinations). Standard order for a higher number of factors is obtained in similar fashion, beginning with the series for the next smaller number of factors, and continuing by multiplying that series by the new element introduced.

The estimated main effects and interactions also appear in a standard order:

For two factors: T, A, B, AB
For three factors: T, A, B, AB, C, AC, BC, ABC
.
.
.
etc.

where T corresponds to the overall average effect, A to the main effect of factor A, AB to the interaction of factors A and B, etc.

For fractional factorials, the treatment combinations in column 1 should be listed in the order given in the plans of Table 12-4. The order of the estimated effects is also given in Table 12-4. For fractional factorial plans other than those given in Table 12-4, see Davies[3] for the necessary ordering for the Yates method of analysis.

* In a $\frac{1}{2^b}$ fraction of a 2^n factorial, there are $2n'$ observations, where $n' = n - b$ (See Par. 12-4).

The systematic procedure for Yates' method is as follows:

Procedure	**Example**
(1) Make a table with $n + 2$ columns. In the first column, list the treatment combinations in standard order.	(1) Use Data Sample 12-2, the results of which are summarized in Table 12-1. This is a 2^4 factorial ($n = 4$). Therefore, our Table will have six columns, as shown in Table 12-2.
(2) In column 2, enter the observed yield or response corresponding to each treatment combination listed in column 1.	(2) See Table 12-2.
(3) In the top half of column 3, enter, in order, the sums of consecutive pairs of entries in column 2. In the bottom half of the column enter, in order, the differences between the same consecutive pairs of entries, i.e., second entry minus first entry, fourth entry minus third entry, etc.	(3) See Table 12-2. For example: $4.2 + 3.1 = 7.3$ $4.5 + 2.9 = 7.4$ $3.9 + 2.8 = 6.7$ etc., and, $3.1 - 4.2 = -1.1$ $2.9 - 4.5 = -1.6$ $2.8 - 3.9 = -1.1$ etc.
(4) Obtain columns 4, 5, ..., $n + 2$, in the same manner as column 3, i.e., by obtaining in each case the sums and differences of the pairs in the preceding column in the manner described in step 3.	(4) See Table 12-2.
(5) The entries in the last column (column $n + 2$) are called g_T, g_A, g_B, g_{AB}, etc., corresponding to the ordered effects T, A, B, AB, etc. Estimates of main effects and interactions are obtained by dividing the appropriate g by 2^{n-1}. g_T divided by 2^{n-1} is the overall mean.	(5) In Table 12-2, $g_A = -12.9;$ the estimated main effect of $A = -12.9/8$ $= -1.6.$ $g_{AD} = -2.5;$ the estimated effect of AD interaction $= -2.5/8$ $= -0.3,$ etc.
Note: The remaining Steps of this procedure are checks on the computation.	*Note:* The following Steps are checks on the computations in Table 12-2.

Procedure	Example
(6) The sum of all the 2^n individual responses (column 2) should equal the total given in the first entry of the last column (column $n + 2$).	(6) The sum of column 2 should equal g_T, $57.5 = 57.5$
(7) The sum of the squares of the individual responses (column 2) should equal the sum of the squares of the entries in the last column (column $n + 2$) divided by 2^n.	(7) The sum of squares of entries in column 2 should equal the sum of squares of the entries in the last column, divided by 2^4 ($= 16$), $219.15 = 3506.40 \div 16 = 219.15$
(8) For any main effect, the entry in the last column (column $n + 2$) equals the sum of the responses in which that factor is at its higher level minus the sum of the responses in which that factor is at its lower level.	(8) see below

(8)

$$\begin{aligned} g_A &= (a + ab + ac + abc + ad + abd \\ &\quad + acd + abcd) \\ &\quad - ((1) + b + c + bc + d + bd \\ &\quad + cd + bcd) \\ &= (22.3) - (35.2) \\ &= -12.9 \end{aligned}$$

$$\begin{aligned} g_B &= (b + ab + bc + abc + bd + abd \\ &\quad + bcd + abcd) \\ &\quad - ((1) + a + c + ac + d + ad \\ &\quad + cd + acd) \\ &= (30.0) - (27.5) \\ &= 2.5 \end{aligned}$$

$$\begin{aligned} g_C &= (c + ac + bc + abc + cd + acd \\ &\quad + bcd + abcd) \\ &\quad - ((1) + a + b + ab + d + ad \\ &\quad + bd + abd) \\ &= (28.3) - (29.2) \\ &= -0.9 \end{aligned}$$

$$\begin{aligned} g_D &= (d + ad + bd + abd + cd + acd \\ &\quad + bcd + abcd) \\ &\quad - ((1) + a + b + ab + c + ac \\ &\quad + bc + abc) \\ &= (28.3) - (29.2) \\ &= -0.9 \end{aligned}$$

TABLE 12-2. YATES' METHOD OF ANALYSIS USING DATA SAMPLE 12-2

1 Treatment Combination	2 Response (Yield)	3	4	5	6 g
(1)	4.2	7.3	14.7	29.2	$57.5 = g_T$
a	3.1	7.4	14.5	28.3	$-12.9 = g_A$, an estimate of $8A$
b	4.5	6.7	14.5	−5.2	$2.5 = g_B$ " $8B$
ab	2.9	7.8	13.8	−7.7	$-3.5 = g_{AB}$ " $8AB$
c	3.9	7.0	−2.7	1.2	$-0.9 = g_C$ " $8C$
ac	2.8	7.5	−2.5	1.3	$-0.5 = g_{AC}$ " $8AC$
bc	4.6	6.5	−3.5	−0.8	$1.3 = g_{BC}$ " $8BC$
abc	3.2	7.3	−4.2	−2.7	$0.5 = g_{ABC}$ " $8ABC$
d	4.0	−1.1	0.1	−0.2	$-0.9 = g_D$ " $8D$
ad	3.0	−1.6	1.1	−0.7	$-2.5 = g_{AD}$ " $8AD$
bd	5.0	−1.1	0.5	0.2	$0.1 = g_{BD}$ " $8BD$
abd	2.5	−1.4	0.8	−0.7	$-1.9 = g_{ABD}$ " $8ABD$
cd	4.0	−1.0	−0.5	1.0	$-0.5 = g_{CD}$ " $8CD$
acd	2.5	−2.5	−0.3	0.3	$-0.9 = g_{ACD}$ " $8ACD$
bcd	5.0	−1.5	−1.5	0.2	$-0.7 = g_{BCD}$ " $8BCD$
$abcd$	2.3	−2.7	−1.2	0.3	$0.1 = g_{ABCD}$ " $8ABCD$
Total	57.5				
Sum of Squares	219.15				3506.40

12-2.2.2 Testing for Significance of Main Effects and Interactions. Before using this procedure, read Paragraph 12-1.2 and perform the computation described in Paragraph 12-2.2.1.

Procedure

(1) Choose α, the level of significance.

(2) If there is no available estimate of the variation due to experimental error,* find the sum of squares of the g's corresponding to interactions of three or more factors in Table 12-2.

(3) To obtain s^2, divide the sum of squares obtained in Step 2 by $2^n\nu$, where ν is the number of interactions included. In a 2^n factorial, the number of third and higher interactions will be $2^n - (n^2 + n + 2)/2$. If an independent estimate of the variation due to experimental error *is* available, use this s^2.

Example

(1) Let $\alpha = .05$

(2) Using Table 12-2,

$$g^2_{ABC} + g^2_{ABD} + g^2_{ACD} + g^2_{BCD} + g^2_{ABCD} = 5.17$$

(3)
$$n = 4$$
$$\nu = 5$$
$$2^n\nu = 16\,(5) = 80$$
$$s^2 = 5.17/80 = .0646$$
$$s = .254$$

* See Paragraph 12-1.2.

Procedure	Example
(4) Look up $t_{1-\alpha/2}$ for ν degrees of freedom in Table A-4. If higher order interactions are used to obtain s^2, ν is the number of interactions included. If an independent estimate of s^2 is used, ν is the degrees of freedom associated with this estimate.	(4) $t_{.975}$ for 5 d.f. = 2.571
(5) Compute $w = (2^n)^{\frac{1}{2}} t_{1-\alpha/2}\, s$	(5) $w = 4\ (2.571)\ (0.254) = 2.61$
(6) For any main effect or interaction X, if the absolute value of g_X is greater than w, conclude that X is different from zero, e.g., if $\lvert g_A \rvert > w$, conclude that the A effect is different from zero. Otherwise, there is no reason to believe that X is different from zero.	(6) See Table 12-2. $\lvert g_A \rvert = 12.9$, and $\lvert g_{AB} \rvert = 3.5$ are greater than w; therefore, the main effect of A and the interaction AB are believed to be significant.

12-3 FACTORIAL EXPERIMENTS WHEN UNIFORM CONDITIONS CANNOT BE MAINTAINED THROUGHOUT THE EXPERIMENT (EACH FACTOR AT TWO LEVELS)

12-3.1 SOME EXPERIMENTAL ARRANGEMENTS

When the number of factors to be investigated are more than just a few, it may be that the required number of trials 2^n is too large to be carried out under reasonably uniform conditions — e.g., on one batch of raw material, or on one piece of equipment. In such cases, the design can be arranged in groups or blocks so that conditions affecting each block can be made as uniform as possible. The use of planned grouping within a factorial design (i.e., a *blocked* factorial) will improve the precision of estimation of experimental error, and will enable us to estimate the main effects free of block differences; but, the structure of the designs is such that certain interaction effects will be inextricable from block effects. In most designs, however, only three-factor and higher-order interactions will be confused ("confounded") with blocks.

Some experimental arrangements of this kind are given in Table 12-3, and their analysis and interpretation are given in Paragraph 12-3.2.

Blocked factorial designs have not been very widely used in experimentation in the physical sciences, and the presumption is that they are usually not the most suitable designs for the kinds of non-homogeneity that occur in these applications. (See Chapter 13 for other designs which make use of blocking.) For this reason, no numerical example is given in this Paragraph. This Paragraph is included for completeness, and serves to link the full factorials (Paragraph 12-2) and the fractional factorials (Paragraph 12-4).

TABLE 12-3. SOME BLOCKED FACTORIAL PLANS
(FOR USE WHEN FACTORIAL EXPERIMENT MUST BE SUB-DIVIDED INTO HOMOGENEOUS GROUPS)

Plans for Three Factors: $2^3 = 8$ Observations

(i) Four observations per block (*ABC* confounded with block effects).

Block 1 (1), *ab, ac, bc*

Block 2 *a, b, c, abc*

Plans for Four Factors: $2^4 = 16$ Observations

(i) Eight observations per block (*ABCD* interaction confounded with block effects).

Block 1 (1), *ab, ac, bc, ad, bd, cd, abcd*

Block 2 *a, b, c, abc, d, abd, acd, bcd*

(ii) Four observations per block (*AD, ABC, BCD,* confounded with block effects).

Block 1 (1), *bc, abd, acd*

Block 2 *a, abc, bd, cd*

Block 3 *b, c, ad, abcd*

Block 4 *d, bcd, ab, ac*

Plans for Five Factors: $2^5 = 32$ Observations

(i) Sixteen observations per block (*ABCDE* interaction confounded with block effects).

Block 1 (1), *ab, ac, bc, ad, bd, cd, abcd, ae, be, ce, abce, de, abde, acde, bcde*

Block 2 *a, b, c, abc, d, abd, acd, bcd, e, abe, ace, bce, ade, bde, cde, abcde*

(ii) Eight observations per block (*BCE, ADE, ABCD,* confounded with block effects).

Block 1 (1), *ad, bc, abcd, abe, bde, ace, cde*

Block 2 *a, d, abc, bcd, be, abde, ce, acde*

Block 3 *b, abd, c, acd, ae, de, abce, bcde*

Block 4 *e, ade, bce, abcde, ab, bd, ac, cd*

(iii) Four observations per block (*AD, BE, ABC, BCD, CDE, ACE, ABDE,* confounded with block effects).

Block 1 (1), *bce, acd, abde*

Block 2 *a, abce, cd, bde*

Block 3 *b, ce, abcd, ade*

Block 4 *c, be, ad, abcde*

Block 5 *d, bcde, ac, abe*

Block 6 *e, bc, acde, abd*

Block 7 *ab, ace, bcd, de*

Block 8 *ae, abc, cde, bd*

TABLE 12-3. SOME BLOCKED FACTORIAL PLANS (Continued)

Plans for Six Factors: $2^6 = 64$ Observations

(i) Thirty-two observations per block (*ABCDEF* confounded with block effects).

Block 1	(1), *abcdef*, plus all treatment combinations represented by two letters (e.g., *ab*, *ac*, etc.) and by four letters (e.g., *abcd*, *bcde*, etc.)
Block 2	All treatment combinations represented by a single letter, by three letters, and by five letters.

(ii) Sixteen observations per block (*ABCD*, *BCEF*, *ADEF*, confounded with block effects).

Block 1	(1), *bc, ad, abcd, ef, bcef, adef, abcdef, bde, cde, abe, ace, bdf, cdf, abf, acf*
Block 2	*a, abc, d, bcd, aef, abcef, def, bcdef, abde, acde, be, ce, abdf, acdf, bf, cf*
Block 3	*b, c, abd, acd, bef, cef, abdef, acdef, de, bcde, ae, abce, df, bcdf, af, abcf*
Block 4	*e, bce, ade, abcde, f, bcf, adf, abcdf, bd, cd, ab, ac, bdef, cdef, abef, acef*

(iii) Eight observations per block (*ADE*, *BCE*, *ACF*, *BDF*, *ABCD*, *ABEF*, *CDEF*, confounded with block effects).

Block 1	(1), *ace, bde, abcd, adf, cdef, abef, bcf*
Block 2	*a, ce, abde, bcd, df, acdef, bef, abcf*
Block 3	*b, abce, de, acd, abdf, bcdef, aef, cf*
Block 4	*c, ae, bcde, abd, acdf, def, abcef, bf*
Block 5	*d, acde, be, abc, af, cef, abdef, bcdf*
Block 6	*e, ac, bd, abcde, adef, cdf, abf, bcef*
Block 7	*f, acef, bdef, abcdf, ad, cde, abe, bc*
Block 8	*ab, bce, ade, cd, bdf, abcdef, ef, acf*

(iv) Four observations per block (*AD*, *BE*, *CF*, *ABC*, *BCD*, *CDE*, *DEF*, *ACE*, *AEF*, *ABF*, *BDF*, *ABDE*, *BCEF*, *ACDF*, *ABCDEF*, confounded with block effects).

Block 1	(1), *bcef, acdf, abde*
Block 2	*a, abcef, cdf, bde*
Block 3	*b, cef, abcdf, ade*
Block 4	*c, bef, adf, abcde*
Block 5	*d, bcdef, acf, abe*
Block 6	*e, bcf, acdef, abd*
Block 7	*f, bce, acd, abdef*
Block 8	*ab, acef, bcdf, de*
Block 9	*ac, abef, df, bcde*
Block 10	*ad, abcdef, cf, be*
Block 11	*ae, abcf, cdef, bd*
Block 12	*af, abce, cd, bdef*
Block 13	*bc, ef, abdf, acde*
Block 14	*bf, ce, abcd, adef*
Block 15	*abc, aef, bdf, cde*
Block 16	*abf, ace, bcd, def*

TABLE 12-3. SOME BLOCKED FACTORIAL PLANS (Continued)

Plans for Seven Factors: $2^7 = 128$ Observations

(i) Sixty-four observations per block (*ABCDEFG* confounded with block effects).

Block 1 (1), and all treatment combinations represented by two letters, four letters, or six letters (e.g., *ab*, *abcd*, etc.).

Block 2 All treatment combinations represented by a single letter, by three letters, and by five letters, plus *abcdefg*.

(ii) Thirty-two observations per block (*ABCD, ABEFG, CDEFG,* confounded with block effects).

Block 1 (1), *ab, abcd, ace, acf, acg, ade, adf, adg, bce, bcf, cdef, cdeg, cdfg, abcdef, abcdeg, abcdfg, abef, bcg, bde, bdf, bdg, abeg, abfg, cd, ef, eg, fg, acefg, adefg, bcefg, bdefg*

Block 2 *a, b, bcd, ce, cf, cg, de, df, dg, abce, abcf, acdef, acdeg, acdfg, bcdef, bcdeg, bcdfg, bef, abcg, abde, abdf, abdg, beg, bfg, acd, aef, aeg, afg, cefg, defg, abcefg, abdefg*

Block 3 *c, abc, abd, ae, af, ag, acde, acdf, acdg, be, bf, def, deg, dfg, abdef, abdeg, abdfg, abcef, bg, bcde, bcdf, bcdg, abceg, abcfg, d, cef, cfg, aefg, acdefg, befg, bcdefg, ceg*

Block 4 *e, abe, abcde, ac, acef, aceg, ad, adef, adeg, bc, bcef, cdf, cdg, cdefg, abcdf, abcdg, abcdefg, abf, bceg, bd, bdef, bdeg, abg, abefg, cde, f, g, efg, acfg, adfg, bcfg, bdfg*

(iii) Sixteen observations per block (*ABCD, BCEF, ADEF, ACFG, BDFG, ABEG, CDEG,* confounded with block effects).

Block 1 (1), *bde, adg, abeg, bcg, cdeg, abcd, ace, efg, bdfg, adef, abf, bcef, cdf, abcdefg, acfg*

Block 2 *a, abde, dg, beg, abcg, acdeg, bcd, ce, aefg, abdfg, def, bf, abcef, acdf, bcdefg, cfg*

Block 3 *b, de, abdg, aeg, cg, bcdeg, acd, abce, befg, dfg, abdef, af, cef, bcdf, acdefg, abcfg*

Block 4 *c, bcde, acdg, abceg, bg, deg, abd, ae, cefg, bcdfg, acdef, abcf, bef, df, abdefg, afg*

Block 5 *d, be, ag, abdeg, bcdg, ceg, abc, acde, defg, bfg, aef, abdf, bcdef, cf, abcefg, acdfg*

Block 6 *e, bd, adeg, abg, bceg, cdg, abcde, ac, fg, bdefg, adf, abef, bcf, cdef, abcdfg, acefg*

Block 7 *f, bdef, adfg, abefg, bcfg, cdefg, abcdf, acef, eg, bdg, ade, ab, bce, cd, abcdeg, acg*

Block 8 *g, bdeg, ad, abe, bc, cde, abcdg, aceg, ef, bdf, adefg, abfg, bcefg, cdfg, abcdef, acf*

(iv) Eight observations per block (*ACF, ADE, BCE, BDF, CDG, ABG, EFG, ABEF, CDEF, ABCD, BDEG, ACEG, ADFG, BCFG, ABCDEFG,* confounded with block effects).

Block 1 (1), *aceg, bdeg, abcd, adfg, cdef, abef, bcfg*

Block 2 *a, ceg, abdeg, bcd, dfg, acdef, bef, abcfg*

Block 3 *b, abceg, deg, acd, abdfg, bcdef, aef, cfg*

Block 4 *c, aeg, bcdeg, abd, acdfg, def, abcef, bfg*

Block 5 *d, acdeg, beg, abc, afg, cef, abdef, bcdfg*

Block 6 *e, acg, bdg, abcde, adefg, cdf, abf, bcefg*

Block 7 *f, acefg, bdefg, abcdf, adg, cde, abe, bcg*

Block 8 *g, ace, bde, abcdg, adf, cdefg, abefg, bcf*

Block 9 *ab, bceg, adeg, cd, bdfg, abcdef, ef, acfg*

Block 10 *ac, eg, abcdeg, bd, cdfg, adef, bcef, abfg*

Block 11 *ad, cdeg, abeg, bc, fg, acef, bdef, abcdfg*

Block 12 *ae, cg, abdg, bcde, defg, acdf, bf, abcefg*

Block 13 *af, cefg, abdefg, bcdf, dg, acde, be, abcg*

Block 14 *ag, ce, abde, bcdg, df, acdefg, befg, abcf*

Block 15 *bg, abce, de, acdg, abdf, bcdefg, aefg, cf*

Block 16 *abg, bce, ade, cdg, bdf, abcdefg, efg, acf*

12-3.2 ANALYSIS OF BLOCKED FACTORIAL EXPERIMENTS WHEN EACH FACTOR IS AT TWO LEVELS

12-3.2.1 Estimation of Main Effects and Interactions. The procedure of Paragraph 12-2.2.1 (Yates' method) should be used. Remember that certain of the interactions are confounded with block effects.

12-3.2.2 Testing for Significance of Main Effects and Interactions. Before using this procedure, read Paragraph 12-1.2, and perform the computations described in Paragraph 12-2.2.1.

Procedure

(1) Choose α, the level of significance.

(2) If there is no estimate of the variation due to experimental error available*, find the sum of squares of the g's corresponding to interactions of three or more factors in the Yates' Table (omitting those interactions that are confounded with blocks).

(3) To obtain s^2, divide the sum of squares obtained in Step 2 by $2^n\nu$, where ν is the number of interactions included. If an independent estimate of the variation due to experimental error *is* available, use this s^2.

(4) Look up $t_{1-\alpha/2}$ for ν degrees of freedom in Table A-4.
If higher order interactions are used to obtain s^2, ν is the number of interactions included.
If an independent estimate of s^2 is used, ν is the degrees of freedom associated with this estimate.

(5) Compute

$$w = (2^n)^{\frac{1}{2}} t_{1-\alpha/2}\, s$$

(6) For any main effect or interaction X, if $|g_X| > w$, conclude that X is different from zero, e.g., if $|g_A| > w$, conclude that the A effect is different from zero. Otherwise, there is no reason to believe that X is different from zero.

* See Paragraph 12-1.2.

12-4 FRACTIONAL FACTORIAL EXPERIMENTS (EACH FACTOR AT TWO LEVELS)

12-4.1 THE FRACTIONAL FACTORIAL DESIGNS

If there are many factors, a complete factorial experiment (Paragraph 12-2), requiring all possible combinations of levels of the factors, involves a large number of tests. This is true even when only two levels of each factor are being investigated. In such cases, the complete factorial experiment may overtax the available facilities. In other situations, it may not be practical to plan the entire experimental program in advance, and we may wish to conduct a few smaller experiments to serve as a guide to future work. It is possible that the complete set of experiments may furnish more information or precision than is needed for the purpose in hand.

In these cases, it is useful to have a plan that requires fewer tests than the complete factorial experiment. Recent developments in statistics have considered the problem of planning multifactor experiments that require measuring only a fraction of the total number of possible combinations. The *fraction* is a carefully prescribed subset of all possible combinations; its analysis is relatively straightforward; and the use of a fractional factorial does not preclude the possibility of later completion of the full factorial experiment.

In Figures 12-2, 12-3, and 12-4, let the letters A, B, C, D, E, F, and G, stand for seven factors to be investigated, and let the subscripts zero and one denote two alternative levels of each factor. The 128 $(= 2^7)$ possible experimental conditions are represented by the 128 cells of Figure 12-2. The shaded squares represent those experimental combinations to be investigated if the experimenter wishes to measure only half the 128 possible combinations. In the same way, the shaded cells in Figures 12-3 and 12-4 illustrate plans requiring only 32 and 16 measurements, respectively, instead of the full set of 128.

Fractional factorial experiments obviously cannot produce as much information as the full factorial. Economy is achieved at the expense of assuming that certain of the interactions between factors are negligible. Some of the larger fractions (e.g., the half-replicate shown in Figure 12-2) require only that third-order (and higher) interactions be assumed negligible, and this assumption is not uncommon. However, the plan calling for one-eighth of the possible combinations, as shown in Figure 12-4, can only be used for evaluating the main effects of each of the seven factors, and will not allow the evaluation of any two-factor interactions.

In a complete factorial experiment we have 2^n tests. In the analysis of a complete factorial, we have n main effects, $2^n - n - 1$ interaction effects, and an overall average effect. The 2^n tests can be used to give independent estimates of the 2^n effects. In a fractional factorial $\left(\text{say the fraction } \frac{1}{2^b}\right)$ there will be only 2^{n-b} tests and, therefore, 2^{n-b} independent estimates. In designing the fractional plans (i.e., in selecting an optimum subset of the 2^n total combinations), the goal is to keep each of the 2^{n-b} estimates as "clean" as possible — i.e., to keep the estimates of main effects and if possible second-order interactions free of confusion with each other.

If we plan to test whether or not certain of the effects are significant, we must have an estimate of the variation due to experimental error which is independent of our estimates of the effects. See Paragraph 12-1.2.

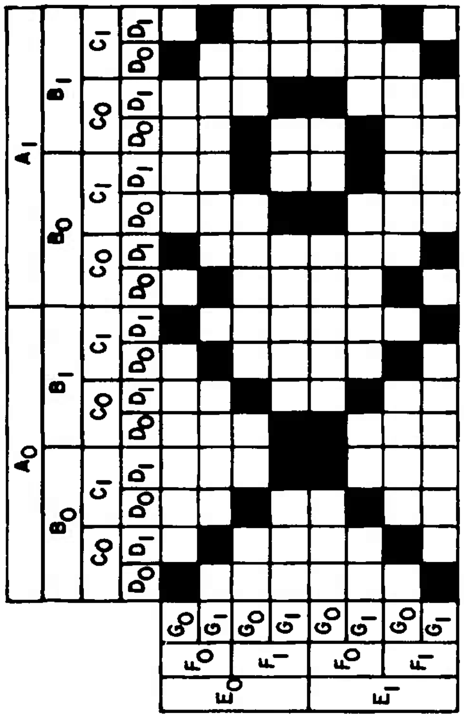

Figure 12-3. A one-quarter replicate of a 2^7 factorial.

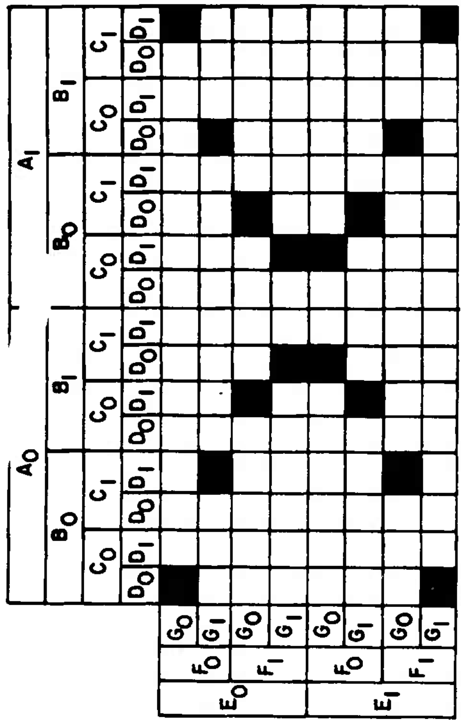

Figure 12-4. A one-eighth replicate of a 2^7 factorial.

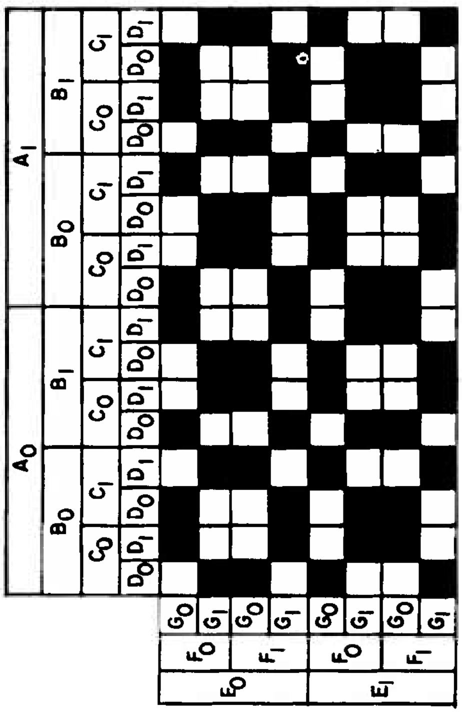

Figure 12-2. A one-half replicate of a 2^7 factorial.

Table 12-4 gives a number of useful two-level fractional factorial plans, together with the effects that can be estimated (assuming three-factor and higher-order interaction terms are negligible). The treatment combinations should be randomly allocated to the experimental material. More two-level plans may be found in reference [4], and fractional factorial plans for factors at three levels may be found in reference [5].

TABLE 12-4. SOME FRACTIONAL FACTORIAL PLANS

Plans	Treatment Combinations†	Estimated Effects‡
Plan 1:	(1)	T
Three factors ($n = 3$)	ac	$A - BC$
½ replication ($b = 1$)	bc	$B - AC$
4 observations	ab	$-C + AB$
Plan 2:	(1)	T
Four factors ($n = 4$)	ad	A
½ replication ($b = 1$)	bd	B
8 observations	ab	$AB + CD$
	cd	C
	ac	$AC + BD$
	bc	$BC + AD$
	$abcd$	D
Plan 3:	(1)	T
Five factors ($n = 5$)	ae	A
½ replication ($b = 1$)	be	B
16 observations	ab	AB
	ce	C
	ac	AC
	bc	BC
	$abce$	$-DE$
	de	D
	ad	AD
	bd	BD
	$abde$	$-CE$
	cd	CD
	$acde$	$-BE$
	$bcde$	$-AE$
	$abcd$	$-E$

† The order given is the order in which the data are to be listed in the first column of the Yates method of analysis (see Pars. 12-2.2.1 and 12-4.2.1).

‡ The order given is the order in which estimated effects come out in the last column of the Yates method of analysis. See Pars. 12-2.2.1 and 12-4.2.1.

TABLE 12-4. SOME FRACTIONAL FACTORIAL PLANS (Continued)

Plans	Treatment Combinations†	Estimated Effects‡
Plan 4:	(1)	T
Five factors ($n = 5$)	ad	$A - DE$
¼ replication ($b = 2$)	bde	$B - CE$
8 observations	abe	$AB + CD$
	cde	$C - BE$
	ace	$AC + BD$
	bc	$-E + BC + AD$
	$abcd$	$D - AE$
Plan 5:	(1)	T
Six factors ($n = 6$)	ae	A
¼ replication ($b = 2$)	bef	B
16 observations	abf	$AB + CE$
	cef	C
	acf	$AC + BE$
	bc	$BC + AE + DF$
	$abce$	E
	df	D
	$adef$	$AD + EF$
	bde	$BD + CF$
	abd	*
	cde	$CD + BF$
	acd	*
	$bcdf$	F
	$abcdef$	$AF + DE$
Plan 6:	(1)	T
Six factors ($n = 6$)	adf	$A - DE - CF$
⅛ replication ($b = 3$)	bde	$B - CE - DF$
8 observations	$abef$	$AB + CD + EF$
	$cdef$	$C - AF - BE$
	ace	$-F + AC + BD$
	bcf	$-E + AD + BC$
	$abcd$	$D - AE - BF$

† ‡ See footnote on page 12-16.

* To be used in our estimate of the variation due to experimental error.

TABLE 12-4. SOME FRACTIONAL FACTORIAL PLANS (Continued)

Plans	Treatment Combinations†	Estimated Effects‡
Plan 7:	(1)	T
Seven factors ($n = 7$)	*aeg*	A
$\frac{1}{8}$ replication ($b = 3$)	*befg*	B
16 observations	*abf*	$AB + CE + DG$
	cef	C
	acfg	$AC + BE + FG$
	bcg	$BC + AE + DF$
	abce	E
	dfg	D
	adef	$AD + EF + BG$
	bde	$BD + CF + AG$
	abdg	G
	cdeg	$CD + BF + EG$
	acd	*
	bcdf	F
	abcdefg	$AF + DE + CG$
Plan 8:	(1)	T
Eight factors ($n = 8$)	*aegh*	A
$\frac{1}{16}$ replication ($b = 4$)	*befg*	B
16 observations	*abfh*	$AB + CE + DG + FH$
	cefh	C
	acfg	$AC + BE + FG + DH$
	bcgh	$BC + AE + DF + GH$
	abce	E
	dfgh	D
	adef	$AD + EF + BG + CH$
	bdeh	$BD + AG + CF + EH$
	abdg	G
	cdeg	$CD + AH + BF + EG$
	acdh	H
	bcdf	F
	abcdefgh	$AF + DE + CG + BH$

† ‡ * See footnotes, pages 12-16 and 12-17.

Data Sample 12-4 — Flame Tests of Fire-Retardant Treatments

Using Data Sample 12-2, we assume that a fractional factorial design had been used, instead of the full factorial. From Table 12-4, we use plan 2, a one-half replicate of four factors ($n = 4$, $b = 1$). The plan is reproduced as follows:

TREATMENT COMBINATIONS	ESTIMATED EFFECTS
(1)	T
ad	A
bd	B
ab	$AB + CD$
cd	C
ac	$AC + BD$
bc	$BC + AD$
$abcd$	D

The resulting data are shown in Table 12-5.

TABLE 12-5. RESULTS OF FLAME TESTS OF FIRE-RETARDANT TREATMENTS (FRACTIONAL FACTORIAL EXPERIMENT OF DATA SAMPLE 12-4)

		A_0				A_1			
		B_0		B_1		B_0		B_1	
C_0	D_0	4.2	(1)					2.9	ab
	D_1			5.0	bd	3.0	ad		
C_1	D_0			4.6	bc	2.8	ac		
	D_1	4.0	cd					2.3	$abcd$

12-4.2 ANALYSIS

12-4.2.1 Estimates of Main Effects and Interactions. We use the Yates procedure described in Paragraph 12-2.2.1, replacing n by n' where $n' = n - b$ for the particular fractional factorial used (see Table 12-4). In other words, make a table with $n' + 2$ columns. In column 1 of the Yates table, list the treatment combinations in the order given in the plan in Table 12-4. The last column of the Yates table (column $n' + 2$) will give the g's corresponding to the effects, in the order listed in the "estimated effects" column of Table 12-4. To obtain the estimates of main effects and interactions, divide each g by $2^{n'-1}$. In Table 12-6, we show the Yates method of analysis applied to a fractional factorial experiment, using the results of Data Sample 12-4.

For fractional factorial plans that are not given in Table 12-4, see Davies[3] for the Yates method of analysis.

TABLE 12-6. YATES' METHOD OF ANALYSIS USING DATA SAMPLE 12-4

1 Treatment Combination	2 Response (Yield)	3	4	5 g	Estimated Effect
(1)	4.2	7.2	15.1	28.8	T
ad	3.0	7.9	13.7	−6.8	A
bd	5.0	6.8	−3.3	0.8	B
ab	2.9	6.9	−3.5	−2.0	$AB + CD$
cd	4.0	−1.2	0.7	−1.4	C
ac	2.8	−2.1	0.1	−0.2	$AC + BD$
bc	4.6	−1.2	−0.9	−0.6	$BC + AD$
abcd	2.3	−2.3	−1.1	−0.2	D
Total	28.8				
Sum of Squares	110.34			882.72	

Checks: (see Steps 6, 7, and 8 of Paragraph 12-2.2.1).

The sum of column 2 should equal g_T, the first entry in column 5.

The sum of squares of entries in column 2 should equal the sum of squares of the g's divided by $2^{n'} = 2^3 = 8$. $(110.34 = 882.72/8 = 110.34)$.

g_A = sum of all yields in which A is at its higher level minus sum of all yields in which A is at its lower level.

$g_A = 11.0 - 17.8 = -6.8$.

Similarly,

$g_B = 14.8 - 14.0 = 0.8$

$g_C = 13.7 - 15.1 = -1.4$.

12-4.2.2 Testing for Significance of Main Effects and Interactions. Before using this procedure, read Paragraph 12-1.2, and perform the computations specified in Paragraph 12-4.2.1.

Procedure	Example
(1) Choose α, the level of significance.	(1) Let $\alpha = .05$
(2) If no external estimate of the variation due to experimental error is available,* check the lines in the Yates table that correspond to estimated effects which are expected to be zero. Compute the sum of squares of the g's for the lines checked.	(2) See Step (3).
(3) To obtain s^2, divide the sum of squares obtained in Step (2) by $2^{n'}\nu$, where ν is the number of interactions included. If an independent estimate of the variation due to experimental error *is* available, use this s^2.	(3) In the analysis, we use an independent estimate of s^2, from 24 pairs of duplicate measurements obtained in another part of the larger program: $s^2 = .0408$, $s = .202$, $\nu = 24$
(4) Look up $t_{1-\alpha/2}$ for ν degrees of freedom in Table A-4.	(4) $t_{.975}$ for 24 d.f. $= 2.064$
(5) Compute $w = (2^{n'})^{\frac{1}{2}} t_{1-\alpha/2}\, s$	(5) $w = \sqrt{8}\,(2.064)\,(0.202) = (2.828)\,(0.417) = 1.18$
(6) For any main effect or interaction X, if the absolute value of g_X is greater than w, conclude that X is different from zero. For example, if $\lvert g_A \rvert > w$, conclude that the A effect is different from zero. Otherwise, there is no reason to believe that X is different from zero.	(6) See Table 12-6. $\lvert g_A \rvert = 6.8$, $\lvert g_C \rvert = 1.4$, and $\lvert g_{AB+CD} \rvert = 2.0$ are all greater than w; therefore, the main effect of A, the main effect of C, and the mixed interaction $AB + CD$ are believed to be significant.

* See Paragraph 12-1.2.

REFERENCES

1. F. Yates, *The Design and Analysis of Factorial Experiments,* Technical Communication No. 35, Imperial Bureau of Soil Science, Harpenden, England, 1937.
2. W. G. Cochran and G. M. Cox, *Experimental Designs* (2d. edition), John Wiley and Sons, Inc., New York, N.Y., 1957.
3. O. L. Davies (Ed.), *The Design and Analysis of Industrial Experiments,* Hafner Publishing Co., New York, N.Y., 1954.
4. National Bureau of Standards, *Fractional Factorial Experiment Designs For Factors at Two Levels,* Applied Mathematics Series, No. 48, U. S. Government Printing Office, Washington 25, D.C., 1957.
5. National Bureau of Standards, *Fractional Factorial Experiment Designs for Factors at Three Levels,* Applied Mathematics Series, No. 54, U. S. Government Printing Office, Washington 25, D.C., 1959.

CHAPTER 13

RANDOMIZED BLOCKS, LATIN SQUARES, AND OTHER SPECIAL-PURPOSE DESIGNS

13-1 INTRODUCTION

The experimental designs treated in this chapter (with a single exception) make use of the *planned grouping* discussed in Chapter 11. The exception is the completely-randomized design discussed in Paragraph 13-2, which is included here as a contrast to the *blocked designs* that follow. In Paragraph 13-3, we discuss the simplest type of blocked design, *randomized blocks,* where blocking is made with respect to one source of inhomogeneity and the block is large enough to accommodate all the treatments we wish to test. In Paragraph 13-4, *incomplete-block designs,* the blocking again is one-way, but the block size is not large enough for all treatments to be tested in every block. In one case, the designs are called *balanced incomplete-block* plans (Paragraph 13-4.2), because certain restrictions on the assignment of treatments to blocks lead to equal precision in the estimation of differences between treatments.

The *chain block* design, a special type of incomplete block design without this balance in the precision of the estimates, is discussed in Paragraph 13-4.3.

When the experimental plan is designed to eliminate two sources of inhomogeneity, two-way blocking is used. The Latin squares and Youden squares (Paragraphs 13-5 and 13-6) are examples of such designs.

13-2 COMPLETELY-RANDOMIZED PLANS

13-2.1 PLANNING

This plan is simple, and is the best choice when the experimental material is homogeneous and background conditions can be well controlled during the experiment. If there are a total of N available experimental units, and we wish to assign $n_1, n_2, \ldots, n_t$ experimental units respectively to each of the t treatments or products, then we proceed to assign the experimental units to the treatments at random. As an example, suppose we wish to test three types of ammunition of a given size and caliber, to see which type has the highest velocity. We have n_1, n_2, n_3 shells, respectively, of the three types. If the conditions under which the shells are fired are assumed to be the same for each shell, i.e., temperature, barrel conditions, etc., then the simplest plan is to choose the shells at random and fire them in that order. It is obvious that if we fired all the shells of one type first, and then fired all the shells of the next type, etc., we would have no insurance against influences on velocity such as the wearing of the gun barrel or changes in atmospheric conditions such as temperature. Randomization affords insurance against uncontrollable disturbances in the sense that such disturbances have the same chance of affecting each of the factors under study, and will be balanced out in the long run.

The results of a completely-randomized plan can be exhibited in a table such as Table 13-1.

TABLE 13-1. SCHEMATIC PRESENTATION OF RESULTS FOR COMPLETELY-RANDOMIZED PLANS

Observation	Treatment			
	1	2	...	t
1				
2				
3				
.				
.				
.				
Total				
Mean				

13-2.2 ANALYSIS

Follow the procedure of Chapter 3, Paragraph 3-4, which gives the method for comparing the averages of several products.

13-3 RANDOMIZED BLOCK PLANS

13-3.1 PLANNING

In comparing a number of treatments, it is clearly desirable that all other conditions be kept as nearly constant as possible. Often the required number of tests is too large to be carried out under similar conditions. In such cases, we may be able to divide the experiment into *blocks,* or planned homogeneous groups (see Chapter 11). When each such group in the experiment contains exactly one observation on every treatment, the experimental plan is called a *randomized block plan.*

There are many situations where a randomized block plan can be profitably utilized. For example, a testing scheme may take several days to complete. If we expect some systematic differences between days, we might plan to observe each item on each day, or to conduct one test per day on each item. A day would then represent a block. In another situation, several persons may be conducting the tests or making the observations, and differences between operators are expected. The tests or observations made by a given operator can be considered to represent a block.

The size of a block may be restricted by physical considerations. Suppose we wished to test the wearing qualities of two different synthetic substances used as shoe soles. The two feet of an individual constitute a logical block, since the kind and amount of wear usually is very nearly the same for each foot.

In general, a randomized block plan is one in which each of the treatments appears exactly once in every block. The treatments are allocated to experimental units at random within a given block.

The results of a randomized block experiment can be exhibited in a two-way table such as Table 13-2, assuming we have b blocks and t treatments.

TABLE 13-2. SCHEMATIC PRESENTATION OF RESULTS FOR RANDOMIZED BLOCK PLANS

Block	Treatment				Total	Block Mean $= B/t$
	1	2	. . .	t		
1					B_1	
2					B_2	
.					.	
.					.	
.					.	
b					B_b	
Total	T_1	T_2	. . .	T_t	G	
Treatment Mean $= T/b$						

Since each treatment occurs exactly once in every block, the treatment totals or means are directly comparable without adjustment.

13-3.2 ANALYSIS

The analysis of a randomized block experiment depends on a number of assumptions. We assume that each of the observations is the sum of three components. If we let Y_{ij} be the observation on the ith treatment in the jth block, then

$$Y_{ij} = \varphi_i + \beta_j + e_{ij},$$

where β_j is a term peculiar to a given block. It is the amount by which the response of a given treatment in the jth block differs from the response of the same treatment averaged over all blocks, assuming no experimental error.

φ_i is a term peculiar to the ith treatment, and is constant for all blocks regardless of the block in which the treatment occurs. It may be regarded as the average value of the ith treatment averaged over all blocks in the experiment, assuming no experimental error.

e_{ij} is the experimental error associated with the measurement Y_{ij}.

In order to make interval estimates for, or to make tests on, the φ_i's or the β_j's, we generally assume that the experimental errors e_{ij}'s are independently and normally distributed. However, if the experiment was randomized properly, failure of this assumption will, in general, not cause serious difficulty.

Data Sample 13-3.2 — Conversion Gain of Resistors

The following data, tabulated as outlined in Table 13-2, represent conversion gain of four resistors measured in six test sets. Conversion gain is defined as the ratio of available current-noise power to applied direct-current power expressed in decibel units, and is a measure of the efficiency with which a resistor converts direct-current power to available current-noise power.

We are interested in possible differences among treatments (test sets) and blocks (resistors).

Resistor (Blocks)	Test Set (Treatments)						Total	Mean
	1463	1506	1938	1946	1948	2140		
3	138.0	141.6	137.5	141.8	138.6	139.6	$B_1 = 837.1$	$b_1 = 139.52$
4	152.2	152.2	152.1	152.2	152.0	152.8	$B_2 = 913.5$	$b_2 = 152.25$
5	153.6	154.0	153.8	153.6	153.2	153.6	$B_3 = 921.8$	$b_3 = 153.63$
6	141.4	141.5	142.6	142.2	141.1	141.9	$B_4 = 850.7$	$b_4 = 141.78$
Total	$T_1 =$ 585.2	$T_2 =$ 589.3	$T_3 =$ 586.0	$T_4 =$ 589.8	$T_5 =$ 584.9	$T_6 =$ 587.9	$G =$ 3523.1	
Mean	$t_1 =$ 146.30	$t_2 =$ 147.32	$t_3 =$ 146.50	$t_4 =$ 147.45	$t_5 =$ 146.22	$t_6 =$ 146.98		

13-3.2.1 Estimation of the Treatment Effects. A treatment effect φ_i is estimated by the mean of the observations on the ith treatment. That is, the estimate of φ_i is $t_i = T_i/b$.

For example, see Data Sample 13-3.2. The estimate of the effect of Test Set 1463 is $t_1 = T_1/4 = 585.2/4 = 146.30$. Similarly, $t_2 = 147.32$, $t_3 = 146.50$, $t_4 = 147.45$, $t_5 = 146.22$, $t_6 = 146.98$.

13-3.2.2 Testing and Estimating Differences in Treatment Effects.

Procedure	Example
(1) Choose α, the significance level of the test.	(1) Let $\alpha = .05$
(2) Look up $q_{1-\alpha}(t, \nu)$ in Table A-10, where $\nu = (b-1)(t-1)$	(2) From Data Sample 13-3.2, $q_{.95}(6, 15) = 4.59$
(3) Compute $S_t = (T_1^2 + T_2^2 + \ldots + T_t^2)/b - G^2/tb$	(3) $S_t = 517{,}181.998 - 517{,}176.400 = 5.598$
(4) Compute $S_b = (B_1^2 + B_2^2 + \ldots + B_b^2)/t - G^2/tb$	(4) $S_b = 518{,}104.065 - 517{,}176.400 = 927.665$
(5) Compute $S = \sum_{i=1}^{t}\sum_{j=1}^{b} Y_{ij}^2 - G^2/tb$, i.e., compute the sum of the squares of all the observations, and subtract G^2/tb.	(5) $S = 518{,}123.13 - 517{,}176.40 = 946.73$
(6) Compute $s^2 = (S - S_b - S_t)/(b-1)(t-1)$ and s	(6) $s^2 = 13.467/15 = 0.8978$; $s = 0.9475$
(7) Compute $w = q_{1-\alpha}\, s/\sqrt{b}$	(7) $w = (4.59)(0.9475)/\sqrt{4} = 2.175$
(8) If the absolute difference between any two estimated treatment effects exceeds w, decide that the treatment effects differ; otherwise, the experiment gives no reason to believe the treatment effects differ.	(8) Since there is no pair of treatment means whose difference exceeds 2.175, we have no reason to conclude that test sets differ.

Note: It should be noted that for all possible pairs of treatments i and j, we can make the statements

$$t_i - t_j - w \leq \varphi_i - \varphi_j \leq t_i - t_j + w$$

with $1 - \alpha$ confidence that all the statements are simultaneously true.

13-3.2.3 Estimation of Block Effects. The block effect β_j is estimated by the mean of the observations in the jth block minus the grand mean. That is, the estimate of β_j, the jth block effect, is $b_j = B_j/t - G/bt$.

For example, using Data Sample 13-3.2, the grand average equals $G/bt = 3523.1/24 = 146.80$.

$$b_1 = 139.52 - 146.80 = -7.28 \qquad b_3 = 153.63 - 146.80 = 6.83$$

$$b_2 = 152.25 - 146.80 = 5.45 \qquad b_4 = 141.78 - 146.80 = -5.02$$

13-3.2.4 Testing and Estimating Differences in Block Effects.

Procedure	Example
(1) Choose α, the significance level of the test.	(1) Let $\alpha = .05$
(2) Look up $q_{1-\alpha}(b, \nu)$ in Table A-10, where $\nu = (b-1)(t-1)$	(2) From Data Sample 13-3.2: $\nu = (4-1)(6-1) = 15$; $q_{.95}(4, 15) = 4.08$
(3) Same as Steps (3), (4), (5), and (6), in Paragraph 13-3.2.2	(3) $S_t = 5.598$
(4)	(4) $S_b = 927.665$
(5)	(5) $S = 946.73$
(6)	(6) $s^2 = 0.8978$; $s = 0.9475$
(7) Compute $w' = q_{1-\alpha}\, s/\sqrt{t}$	(7) $w' = (4.08)(0.9475)/\sqrt{6} = 1.578$
(8) If the absolute difference between any two block effects exceeds w', conclude that the block effects differ; otherwise, the experiment gives no reason to believe that block effects differ.	(8) See Paragraph 13-3.2.3. The absolute difference between two block effects does exceed 1.578, and we conclude that resistors do differ.

Note: As in the case of treatment effects, we can make simultaneous statements about the difference between pairs of blocks i and j, with confidence $1 - \alpha$ that all the statements are simultaneously true. The statements are, for all i and j,

$$b_i - b_j - w' \leq \beta_i - \beta_j \leq b_i - b_j + w'.$$

13-4 INCOMPLETE BLOCK PLANS

13-4.1 GENERAL

Incomplete block plans are similar to the randomized block plans of Paragraph 13-3, in that they make use of planned grouping. The distinguishing feature of incomplete block plans is that the block size is not large enough to accommodate all treatments in one block. For example, suppose that a *block* is one day, but that the time required for each test is so long that all experimental treatments cannot be run in one day. The limitation may be due to lack of space; such is the case in spectrographic analysis where a block may be one photographic plate, and the number of specimens to be compared may exceed the capacity of the plate.

We discuss two kinds of randomized incomplete block plans — balanced incomplete block plans in Paragraph 13-4.2, and chain block plans in Paragraph 13-4.3. The former have the advantage of easy analysis and the important property that all differences between treatment effects are estimated with the same precision. The chain block plans have an advantage when we wish to keep the number of duplicate observations on treatments to a minimum, and are very useful when the difference in treatments considered worth detecting is large in comparison to the amount of experimental error. (Experimental error may be thought of as the difference between an observed treatment and the average of a large number of similar observations under similar conditions.)

Other incomplete block designs are available if these two classes do not meet the desires of the experimenter with regard to number of blocks, size of blocks, number of treatments, etc. An important and very large class of designs is the class called the "partially-balanced incomplete block designs" (see Bose, et al.[1]). Experiments using these plans, which are not discussed here, are slightly more complicated to analyze.

13-4.2 BALANCED INCOMPLETE BLOCK PLANS

13-4.2.1 Planning. We define r, b, t, k, λ, E, and N as follows:

r = number of replications (number of times each treatment appears in the plan);
b = number of blocks in the plan;
t = number of treatments;
k = number of treatments which appear in every block;
λ = number of blocks in which a given treatment-pair appears, $\lambda = \dfrac{r\,(k-1)}{t-1}$;
E = a constant used in the analysis, $E = t\lambda/rk$;
N = total number of observations, $N = tr = bk$.

Using this nomenclature, it is possible to enumerate the situations in which it is combinatorially possible to construct a balanced incomplete block design. Plans are listed in Table 13-3 for $4 \leq t \leq 10$, $r \leq 10$. For some other balanced incomplete block plans, see Cochran and Cox.[2]

If we wish to estimate and to make tests of block effects as well as treatment effects, we should consider the plans where $b = t$, i.e., the number of blocks equals the number of treatments. In such plans, called *symmetrical balanced incomplete block designs,* differences between block effects are estimated with equal precision for all pairs of blocks.

To use a given plan from Table 13-3, proceed as follows:

(1) Rearrange the blocks at random. (In a number of the plans in Table 13-3, the blocks are arranged in groups. In these plans, rearrange the blocks at random within their respective groups).

(2) Randomize the positions of the treatment numbers within each block.

(3) Assign the treatments at random to the treatment numbers in the plan.

TABLE 13-3. BALANCED INCOMPLETE BLOCK PLANS ($4 \leq t \leq 10$, $r \leq 10$)

Index

t	k	r	b	λ	E†	Plan No.††
4	2	3	6	1	2/3	1
	3	3	4	2	8/9	*
5	2	4	10	1	5/8	2
	3	6	10	3	5/6	*
	4	4	5	3	15/16	*
6	2	5	15	1	3/5	3
	3	5	10	2	4/5	4
	3	10	20	4	4/5	5
	4	10	15	6	9/10	6
	5	5	6	4	24/25	*
7	2	6	21	1	7/12	*
	3	3	7	1	7/9	7
	4	4	7	2	7/8	8
	6	6	7	5	35/36	*
8	2	7	28	1	4/7	9
	4	7	14	3	6/7	10
	7	7	8	6	48/49	*
9	2	8	36	1	9/16	*
	3	4	12	1	3/4	11
	4	8	18	3	27/32	12
	5	10	18	5	9/10	13
	6	8	12	5	15/16	14
	8	8	9	7	63/64	*
10	2	9	45	1	5/9	15
	3	9	30	2	20/27	16
	4	6	15	2	5/6	17
	5	9	18	4	8/9	18
	6	9	15	5	25/27	19
	9	9	10	8	80/81	*

† The constant $E = t\lambda/rk$ is used in the analysis.

†† The asterisk indicates plans that may be constructed by forming all possible combinations of the t treatments in blocks of size k. The number of blocks b serves as a check that no block has been missed.

Plan 1: $t = 4$, $k = 2$, $r = 3$, $b = 6$, $\lambda = 1$, $E = 2/3$

Group I	Group II	Group III
(1) 1, 2	(3) 1, 3	(5) 1, 4
(2) 3, 4	(4) 2, 4	(6) 2, 3

Plan 2: $t = 5$, $k = 2$, $r = 4$, $b = 10$, $\lambda = 1$, $E = 5/8$

Group I	Group II
(1) 1, 2	(6) 1, 3
(2) 2, 5	(7) 2, 4
(3) 3, 4	(8) 3, 2
(4) 4, 1	(9) 4, 5
(5) 5, 3	(10) 5, 1

TABLE 13-3. BALANCED INCOMPLETE BLOCK PLANS* (Continued)
($4 \leq t \leq 10$, $r \leq 10$)

Plan 3: $t = 6$, $k = 2$, $r = 5$, $b = 15$, $\lambda = 1$, $E = 3/5$

Group I	Group II	Group III	Group IV	Group V
(1) 1, 2	(4) 1, 3	(7) 1, 4	(10) 1, 5	(13) 1, 6
(2) 3, 4	(5) 2, 5	(8) 2, 6	(11) 2, 4	(14) 2, 3
(3) 5, 6	(6) 4, 6	(9) 3, 5	(12) 3, 6	(15) 4, 5

Plan 4: $t = 6$, $k = 3$, $r = 5$, $b = 10$, $\lambda = 2$, $E = 4/5$

(1) 1, 2, 5	(5) 1, 4, 5	(8) 2, 4, 6
(2) 1, 2, 6	(6) 2, 3, 4	(9) 3, 5, 6
(3) 1, 3, 4	(7) 2, 3, 5	(10) 4, 5, 6
(4) 1, 3, 6		

Plan 5: $t = 6$, $k = 3$, $r = 10$, $b = 20$, $\lambda = 4$, $E = 4/5$

Group I	Group II	Group III	Group IV
(1) 1, 2, 3	(3) 1, 2, 4	(5) 1, 2, 5	(7) 1, 2, 6
(2) 4, 5, 6	(4) 3, 5, 6	(6) 3, 4, 6	(8) 3, 4, 5

Group V	Group VI	Group VII	Group VIII
(9) 1, 3, 4	(11) 1, 3, 5	(13) 1, 3, 6	(15) 1, 4, 5
(10) 2, 5, 6	(12) 2, 4, 6	(14) 2, 4, 5	(16) 2, 3, 6

Group IX	Group X
(17) 1, 4, 6	(19) 1, 5, 6
(18) 2, 3, 5	(20) 2, 3, 4

Plan 6: $t = 6$, $k = 4$, $r = 10$, $b = 15$, $\lambda = 6$, $E = 9/10$

Group I	Group II	Group III
(1) 1, 2, 3, 4	(4) 1, 2, 3, 5	(7) 1, 2, 3, 6
(2) 1, 4, 5, 6	(5) 1, 2, 4, 6	(8) 1, 3, 4, 5
(3) 2, 3, 5, 6	(6) 3, 4, 5, 6	(9) 2, 4, 5, 6

Group IV	Group V
(10) 1, 2, 4, 5	(13) 1, 2, 5, 6
(11) 1, 3, 5, 6	(14) 1, 3, 4, 6
(12) 2, 3, 4, 6	(15) 2, 3, 4, 5

* In the Plans, block numbers are in parentheses followed by numbers which indicate treatments. In a number of the plans given, the blocks are arranged in groups. In setting up the experiment, make the groups as homogeneous as possible — i.e., if possible there should be more difference between blocks in different groups than between blocks in the same group.

TABLE 13-3. BALANCED INCOMPLETE BLOCK PLANS* (Continued)
($4 \leq t \leq 10$, $r \leq 10$)

Plan 7: $t = 7, k = 3, r = 3, b = 7, \lambda = 1, E = 7/9$

(1) 1, 2, 4	(3) 3, 4, 6	(5) 5, 6, 1	(7) 7, 1, 3
(2) 2, 3, 5	(4) 4, 5, 7	(6) 6, 7, 2	

Plan 8: $t = 7, k = 4, r = 4, b = 7, \lambda = 2, E = 7/8$

(1) 1, 2, 3, 6	(3) 3, 4, 5, 1	(5) 5, 6, 7, 3	(7) 7, 1, 2, 5
(2) 2, 3, 4, 7	(4) 4, 5, 6, 2	(6) 6, 7, 1, 4	

Plan 9: $t = 8, k = 2, r = 7, b = 28, \lambda = 1, E = 4/7$

Group I	Group II	Group III	Group IV
(1) 1, 2	(5) 1, 3	(9) 1, 4	(13) 1, 5
(2) 3, 4	(6) 2, 8	(10) 2, 7	(14) 2, 3
(3) 5, 6	(7) 4, 5	(11) 3, 6	(15) 4, 7
(4) 7, 8	(8) 6, 7	(12) 5, 8	(16) 6, 8

Group V	Group VI	Group VII
(17) 1, 6	(21) 1, 7	(25) 1, 8
(18) 2, 4	(22) 2, 6	(26) 2, 5
(19) 3, 8	(23) 3, 5	(27) 3, 7
(20) 5, 7	(24) 4, 8	(28) 4, 6

Plan 10: $t = 8, k = 4, r = 7, b = 14, \lambda = 3, E = 6/7$

Group I	Group II	Group III	Group IV
(1) 1, 2, 3, 4	(3) 1, 2, 7, 8	(5) 1, 3, 6, 8	(7) 1, 4, 6, 7
(2) 5, 6, 7, 8	(4) 3, 4, 5, 6	(6) 2, 4, 5, 7	(8) 2, 3, 5, 8

Group V	Group VI	Group VII
(9) 1, 2, 5, 6	(11) 1, 3, 5, 7	(13) 1, 4, 5, 8
(10) 3, 4, 7, 8	(12) 2, 4, 6, 8	(14) 2, 3, 6, 7

* See footnote on page 13-9.

TABLE 13-3. BALANCED INCOMPLETE BLOCK PLANS* (Continued)
($4 \leq t \leq 10$, $r \leq 10$)

Plan 11: $t = 9$, $k = 3$, $r = 4$, $b = 12$, $\lambda = 1$, $E = 3/4$

Group I		Group II		Group III		Group IV	
(1)	1, 2, 3	(4)	1, 4, 7	(7)	1, 5, 9	(10)	1, 8, 6
(2)	4, 5, 6	(5)	2, 5, 8	(8)	7, 2, 6	(11)	4, 2, 9
(3)	7, 8, 9	(6)	3, 6, 9	(9)	4, 8, 3	(12)	7, 5, 3

Plan 12: $t = 9$, $k = 4$, $r = 8$, $b = 18$, $\lambda = 3$, $E = 27/32$

Group I		Group II	
(1)	1, 4, 6, 7	(10)	1, 2, 5, 7
(2)	2, 6, 8, 9	(11)	2, 3, 6, 5
(3)	3, 8, 9, 1	(12)	3, 4, 7, 9
(4)	4, 1, 3, 2	(13)	4, 9, 2, 1
(5)	5, 7, 1, 8	(14)	5, 1, 9, 6
(6)	6, 9, 4, 5	(15)	6, 8, 1, 3
(7)	7, 3, 2, 6	(16)	7, 6, 4, 8
(8)	8, 2, 5, 4	(17)	8, 5, 3, 4
(9)	9, 5, 7, 3	(18)	9, 7, 8, 2

Plan 13: $t = 9$, $k = 5$, $r = 10$, $b = 18$, $\lambda = 5$, $E = 9/10$

Group I		Group II	
(1)	1, 2, 3, 7, 8	(10)	1, 2, 3, 5, 9
(2)	2, 6, 8, 4, 1	(11)	2, 6, 5, 1, 8
(3)	3, 8, 5, 9, 2	(12)	3, 5, 1, 4, 6
(4)	4, 3, 9, 2, 6	(13)	4, 3, 2, 8, 7
(5)	5, 1, 7, 3, 4	(14)	5, 7, 9, 2, 4
(6)	6, 4, 2, 5, 7	(15)	6, 8, 7, 3, 5
(7)	7, 9, 1, 6, 3	(16)	7, 4, 8, 9, 1
(8)	8, 5, 4, 1, 9	(17)	8, 9, 4, 6, 3
(9)	9, 7, 6, 8, 5	(18)	9, 1, 6, 7, 2

Plan 14: $t = 9$, $k = 6$, $r = 8$, $b = 12$, $\lambda = 5$, $E = 15/16$

Group I		Group II	
(1)	1, 2, 4, 5, 7, 8	(4)	1, 2, 5, 6, 7, 9
(2)	2, 3, 5, 6, 8, 9	(5)	1, 3, 4, 5, 8, 9
(3)	1, 3, 4, 6, 7, 9	(6)	2, 3, 4, 6, 7, 8

Group III		Group IV	
(7)	1, 3, 5, 6, 7, 8	(10)	4, 5, 6, 7, 8, 9
(8)	1, 2, 4, 6, 8, 9	(11)	1, 2, 3, 4, 5, 6
(9)	2, 3, 4, 5, 7, 9	(12)	1, 2, 3, 7, 8, 9

* See footnote on page 13-9.

TABLE 13-3. BALANCED INCOMPLETE BLOCK PLANS* (Continued)
($4 \leq t \leq 10$, $r \leq 10$)

Plan 15: $t = 10, k = 2, r = 9, b = 45, \lambda = 1, E = 5/9$

Group I		Group II		Group III		Group IV		Group V	
(1)	1, 2	(6)	1, 3	(11)	1, 4	(16)	1, 5	(21)	1, 6
(2)	3, 4	(7)	2, 7	(12)	2, 10	(17)	2, 8	(22)	2, 9
(3)	5, 6	(8)	4, 8	(13)	3, 7	(18)	3, 10	(23)	3, 8
(4)	7, 8	(9)	5, 9	(14)	5, 8	(19)	4, 9	(24)	4, 10
(5)	9, 10	(10)	6, 10	(15)	6, 9	(20)	6, 7	(25)	5, 7

Group VI		Group VII		Group VIII		Group IX	
(26)	1, 7	(31)	1, 8	(36)	1, 9	(41)	1, 10
(27)	2, 6	(32)	2, 3	(37)	2, 4	(42)	2, 5
(28)	3, 9	(33)	4, 6	(38)	3, 5	(43)	3, 6
(29)	4, 5	(34)	5, 10	(39)	6, 8	(44)	4, 7
(30)	8, 10	(35)	7, 9	(40)	7, 10	(45)	8, 9

Plan 16: $t = 10, k = 3, r = 9, b = 30, \lambda = 2, E = 20/27$

(1)	1, 2, 3	(11)	1, 2, 4	(21)	1, 3, 5
(2)	2, 5, 8	(12)	2, 3, 6	(22)	2, 7, 6
(3)	3, 7, 4	(13)	3, 4, 8	(23)	3, 8, 9
(4)	4, 1, 6	(14)	4, 9, 5	(24)	4, 2, 10
(5)	5, 8, 7	(15)	5, 7, 1	(25)	5, 6, 3
(6)	6, 4, 9	(16)	6, 8, 9	(26)	6, 1, 8
(7)	7, 9, 1	(17)	7, 10, 3	(27)	7, 9, 2
(8)	8, 10, 2	(18)	8, 1, 10	(28)	8, 4, 7
(9)	9, 3, 10	(19)	9, 5, 2	(29)	9, 10, 1
(10)	10, 6, 5	(20)	10, 6, 7	(30)	10, 5, 4

Plan 17: $t = 10, k = 4, r = 6, b = 15, \lambda = 2, E = 5/6$

(1)	1, 2, 3, 4	(6)	1, 6, 8, 10	(11)	3, 5, 9, 10
(2)	1, 2, 5, 6	(7)	2, 3, 6, 9	(12)	3, 6, 7, 10
(3)	1, 3, 7, 8	(8)	2, 4, 7, 10	(13)	3, 4, 5, 8
(4)	1, 4, 9, 10	(9)	2, 5, 8, 10	(14)	4, 5, 6, 7
(5)	1, 5, 7, 9	(10)	2, 7, 8, 9	(15)	4, 6, 8, 9

* See footnote on page 13-9.

TABLE 13-3. BALANCED INCOMPLETE BLOCK PLANS* (Continued)
($4 \leq t \leq 10$, $r \leq 10$)

Plan 18: $t = 10, k = 5, r = 9, b = 18, \lambda = 4, E = 8/9$

(1) 1, 2, 3, 4, 5	(7) 1, 4, 5, 6; 10	(13) 2, 5, 6, 8, 10
(2) 1, 2, 3, 6, 7	(8) 1, 4, 8, 9, 10	(14) 2, 6, 7, 9, 10
(3) 1, 2, 4, 6, 9	(9) 1, 5, 7, 9, 10	(15) 3, 4, 6, 7, 10
(4) 1, 2, 5, 7, 8	(10) 2, 3, 4, 8, 10	(16) 3, 4, 5, 7, 9
(5) 1, 3, 6, 8, 9	(11) 2, 3, 5, 9, 10	(17) 3, 5, 6, 8, 9
(6) 1, 3, 7, 8, 10	(12) 2, 4, 7, 8, 9	(18) 4, 5, 6, 7, 8

Plan 19: $t = 10, k = 6, r = 9, b = 15, \lambda = 5, E = 25/27$

(1) 1, 2, 4, 5, 8, 9	(6) 2, 3, 4, 6, 8, 10	(11) 1, 4, 5, 7, 8, 10
(2) 5, 6, 7, 8, 9, 10	(7) 1, 2, 6, 7, 9, 10	(12) 1, 2, 3, 5, 7, 10
(3) 2, 4, 5, 6, 9, 10	(8) 1, 3, 5, 6, 8, 9	(13) 2, 3, 5, 6, 7, 8
(4) 1, 2, 4, 6, 7, 8	(9) 1, 2, 3, 8, 9, 10	(14) 1, 3, 4, 5, 6, 10
(5) 3, 4, 7, 8, 9, 10	(10) 2, 3, 4, 5, 7, 9	(15) 1, 3, 4, 6, 7, 9

* See footnote on page 13-9.

For analysis, the results of a balanced incomplete block design may be exhibited in a table such as Table 13-4, which shows the arrangement for Plan 7 of Table 13-3.

TABLE 13-4. SCHEMATIC REPRESENTATION OF RESULTS FOR A BALANCED INCOMPLETE BLOCK PLAN

Plan 7 of Table 13-3 is used here for illustration.

Block	Treatment							Total
	A	B	C	D	E	F	G	
1	X	X		X				B_1
2		X	X		X			B_2
3			X	X		X		B_3
4				X	X		X	B_4
5	X				X	X		B_5
6		X				X	X	B_6
7	X		X				X	B_7
Total	T_A	T_B	T_C	T_D	T_E	T_F	T_G	G

13-4.2.2 Analysis. In the analysis of the balanced incomplete block plans the same model is used and the same assumptions are made as in the randomized block plans. The only difference is that, in the present case, the blocks do not each contain all of the treatments.

The analysis described here is sometimes called the intra-block analysis.

Data Sample 13-4.2.2 — Noise Measurement of Resistors

A certain film-type composition resistor used in electronic equipment is of the type which is mounted on a ceramic plate. An investigation was designed to determine the effects of four different geometrical shapes of resistors on the current-noise of these resistors. Since only three resistors could be mounted on one plate, an incomplete block design was used. The plan required a total of 12 resistors (three of each of the four shapes). In the plan, the ceramic plates are blocks ($b = 4$); the resistor shapes are treatments ($t = 4$) and the plan is summarized by the following parameters: $t = 4$, $b = 4$, $k = 3$, $r = 3$, $\lambda = 2$, $E = 8/9$, $N = 12$. Note that this is a symmetrical balanced incomplete block design; i.e., the number of blocks equals the number of treatments.

The following entries are logarithms of the noise measurement.

Plates (Blocks)	Shapes (Treatments)				Total
	A	B	C	D	
1	1.11		.95	.82	$B_1 = 2.88$
2	1.70	1.22		.97	$B_2 = 3.89$
3	1.60	1.11	1.52		$B_3 = 4.23$
4		1.22	1.54	1.18	$B_4 = 3.94$
Total	$T_1 = 4.41$	$T_2 = 3.55$	$T_3 = 4.01$	$T_4 = 2.97$	$G = 14.94$

$t = 4,\ k = 3,\ b = 4,\ r = 3,\ \lambda = 2,\ E = \frac{8}{9},\ N = 12.$

13-4.2.2.1 Estimating Treatment Effects. We assume that the observations have been exhibited in a table such as Table 13-4. The treatment effects cannot be estimated directly from the treatment averages, and must be adjusted for possible block effects. The estimate of φ_i, the effect of the ith treatment, is

$$t_i = Q_i/Er + G/rt,$$

where

$$Q_i = T_i - [(\text{Sum of totals of all blocks containing treatment } i)/k].$$

For example, using Data Sample 13-4.2.2,

$$\begin{aligned} Q_1 &= T_1 - \left(\frac{B_1 + B_2 + B_3}{3}\right) \\ &= 4.41 - \frac{11.00}{3} \\ &= 4.41 - 3.6667 \\ &= 0.7433 \end{aligned}$$

Similarly,

$$\begin{aligned} Q_2 &= 3.55 - \frac{12.06}{3} \\ &= 3.55 - 4.0200 \\ &= -0.4700 \end{aligned}$$

$$\begin{aligned} Q_3 &= 4.01 - \frac{11.05}{3} \\ &= 4.01 - 3.6833 \\ &= 0.3267 \end{aligned}$$

$$\begin{aligned} Q_4 &= 2.97 - \frac{10.71}{3} \\ &= 2.97 - 3.5700 \\ &= -0.6000 \end{aligned}$$

$$\begin{aligned} E &= 8/9,\ r = 3,\ Er = 2.6667,\ t = 4,\ rt = 12, \\ G/rt &= 14.94/12 \\ &= 1.2450 \end{aligned}$$

$$\begin{aligned} t_1 &= \frac{Q_1}{Er} + \frac{G}{rt} \\ &= \frac{0.7433}{2.6667} + 1.2450 \\ &= 1.5237 \end{aligned}$$

$$\begin{aligned} t_3 &= \frac{0.3267}{2.6667} + 1.2450 \\ &= 1.3675 \end{aligned}$$

$$\begin{aligned} t_2 &= \frac{-0.4700}{2.6667} + 1.2450 \\ &= 1.0688 \end{aligned}$$

$$\begin{aligned} t_4 &= \frac{-0.6000}{2.6667} + 1.2450 \\ &= 1.0200 \end{aligned}$$

13-4.2.2.2 Testing and Estimating Differences in Treatment Effects.

Procedure	Example
(1) Choose α, the significance level of the test.	(1) Let $\alpha = .05$
(2) Look up $q_{1-\alpha}(t, \nu)$ in Table A-10, where $\nu = tr - t - b + 1$	(2) From Data Sample 13-4.2.2: $t = 4$, $\nu = 5$, $q_{.95}(4, 5) = 5.22$
(3) Compute Q_i and t_i for each treatment. (The sum of the Q_i should equal zero.)	(3) See Paragraph 13-4.2.2.1
(4) Compute $S_t = \dfrac{Q_1^2 + Q_2^2 + \ldots + Q_t^2}{Er}$	(4) $S_t = \dfrac{1.24012778}{2.6667} = 0.46504$
(5) Compute $S_b = \dfrac{B_1^2 + B_2^2 + \ldots + B_b^2}{k} - \dfrac{G^2}{rt}$	(5) $S_b = \dfrac{56.8430}{3} - 18.60030 = 0.34737$
(6) Compute $S = \Sigma Y_{ij}^2 - G^2/rt$; i.e., compute the sum of the squares of all the observations and subtract G^2/rt.	(6) $S = 19.4812 - 18.6003 = 0.88090$
(7) Compute $s^2 = \dfrac{S - S_t - S_b}{tr - t - b + 1}$	(7) $s^2 = \dfrac{0.06849}{5} = 0.0137$; $s = 0.117$
(8) Compute $w = q_{1-\alpha}\, s/\sqrt{Er}$	(8) $w = \dfrac{(5.22)\ (0.117)}{1.63} = \dfrac{0.611}{1.63} = 0.375$
(9) If the absolute difference between two estimated treatment effects exceeds w, conclude that the treatment effects differ; otherwise, conclude that the experiment gives no reason to believe that the treatment effects differ.	(9) Since there are differences between pairs of treatment effects that do exceed 0.375, we conclude that resistor shapes differ with regard to their effect on current noise.

Note: We can make simultaneous confidence interval statements about the differences between pairs of treatments i and j, with confidence $1 - \alpha$ that all statements are simultaneously true. The statements are, for all i and j,

$$t_i - t_j - w \leq \varphi_i - \varphi_j \leq t_i - t_j + w .$$

13-4.2.2.3 Estimating Block Effects. Like the treatment effects, block effects cannot be estimated directly from block averages, but must be adjusted according to which treatments occur in them. We discuss estimation of the block effects for symmetrical plans only, i.e., where $b = t$, the number of blocks equals the number of treatments. If it is required to estimate or test block effects in a balanced incomplete block plan which is not symmetric, a statistical text book such as Cochran and Cox(2) or Fisher and Yates(3) should be consulted.

For symmetric plans, the estimate of β_j, the jth block effect, is

$$b_j = Q'_j/Er$$

where

$$Q'_j = B_j - (\text{sum of totals of all treatments occurring in the } j\text{th block }/r).$$

For example, using Data Sample 13-4.2.2.

$$\begin{aligned} Q'_1 &= B_1 - \left(\frac{T_1 + T_3 + T_4}{3}\right) \\ &= 2.88 - \frac{11.39}{3} \\ &= 2.88 - 3.7967 \\ &= -0.9167. \end{aligned}$$

Similarly,

$$\begin{aligned} Q'_2 &= 3.89 - \frac{10.93}{3} \\ &= 3.89 - 3.6433 \\ &= 0.2467 \end{aligned}$$

$$\begin{aligned} Q'_3 &= 4.23 - \frac{11.97}{3} \\ &= 4.23 - 3.9900 \\ &= 0.2400 \end{aligned}$$

$$\begin{aligned} Q'_4 &= 3.94 - \frac{10.53}{3} \\ &= 3.94 - 3.5100 \\ &= 0.4300 \end{aligned}$$

$$\begin{aligned} Er &= 2.6667 \\ b_1 &= -0.9167/2.6667 \\ &= -0.34376 \\ b_2 &= 0.2467/2.6667 \\ &= 0.09251 \\ b_3 &= 0.2400/2.6667 \\ &= 0.09000 \\ b_4 &= 0.4300/2.6667 \\ &= 0.16125 \end{aligned}$$

13-4.2.2.4 Testing and Estimating Differences in Block Effects. The procedure described applies to *symmetrical* balanced incomplete block plans only.

Procedure	Example
(1) Choose α, the significance level of the test.	(1) Let $\alpha = .05$
(2) Look up $q_{1-\alpha}(b, \nu)$ in Table A-10, where $\nu = tr - t - b + 1$	(2) See Data Sample 13-4.2.2 $t = 4$ $b = 4$ $r = 3$ $\nu = 5$ $q_{.95}(4, 5) = 5.22$
(3) Compute Q'_i and b_i for each block. (The sum of the Q'_i should equal zero.)	(3) See Paragraph 13-4.2.2.3
(4) Compute $S'_b = (Q'^2_1 + Q'^2_2 + \ldots + Q'^2_t)/Er$	(4) $Er = 2.6667$ $S'_b = 1.14369978/2.6667$ $= 0.42888$
(5) Compute $S'_t = (T^2_1 + T^2_2 + \ldots + T^2_t)/r - G^2/rt$	(5) $S'_t = 56.9516/3 - 18.60030$ $= 18.98387 - 18.60030$ $= 0.38357$
(6) Compute $S = \Sigma Y_{ij}^2 - G^2/rt$; i.e., compute the sum of the squares of all individual observations and subtract G^2/rt.	(6) $S = 19.4812 - 18.60030$ $= 0.88090$
(7) Compute $s^2 = (S - S'_t - S'_b)/(tr - t - b + 1)$ and s	(7) $s^2 = 0.06845/5$ $= 0.0137$ $s = 0.117$

Note: $S'_t + S'_b$ (as computed in steps (4) and (5) above) should equal $S_t + S_b$ (as computed in Paragraph 13-4.2.2.2), and therefore the s^2 here should equal s^2 computed in Paragraph 13-4.2.2.2.

Note: $S'_t + S'_b = 0.81245$ from steps (4) and (5) above. $S_t + S_b = 0.81241$ from Paragraph 13-4.2.2.2. The discrepancy is due to rounding error, and would be larger if fewer decimal places were carried in the computation.

Procedure	Example
(8) Compute $w' = q_{1-\alpha}\, s/\sqrt{Er}$	(8) $w' = (5.22)\,(0.117)/1.63$ $= 0.611/1.63$ $= 0.375$

Procedure	Example
(9) If the absolute difference between any two estimated block effects exceed w', conclude that the block effects differ; otherwise, conclude that the experiment gives no reason to believe the block effects differ.	(9) Since there are differences between pairs of block effects that exceed 0.375, we conclude that blocks (plates) do differ.

Note: We can make simultaneous statements about the differences between pairs of blocks i and j, with confidence $1 - \alpha$ that all the statements are simultaneously true. The statements are, for all i and j,

$$b_i - b_j - w' \leq \beta_i - \beta_j \leq b_i - b_j + w'.$$

13-4.3 CHAIN BLOCK PLANS

13-4.3.1 Planning. The chain block plan is useful when observations are expensive and the experimental error is small. Such a plan can handle a large number of treatments relative to the total number of observations. We need make only a few more observations than we have treatments to compare. Before using a chain block plan, however, we should be confident that the important differences in treatment effects are substantially larger than experimental error.

In a chain block design, some treatments are observed once and some treatments are observed twice. Schematically, the plan can be represented as in Table 13-5.

TABLE 13-5. SCHEMATIC REPRESENTATION OF A CHAIN BLOCK PLAN

	Blocks					
	1	**2**	**...**	**$b-1$**	**b**	
	A_1'	A_2'	...	A_{b-1}'	A_b'	
	A_2''	A_3''	...	A_b''	A_1''	
	x	x			x	
	x	x			x	
	.	.			.	
	.	.			.	
	.	.			.	
	x	x	...		x	
Total	B_1	B_2	...	B_{b-1}	B_b	G (= Grand Total)

In Table 13-5, A_i' represents either a treatment or a group of treatments, and A_i'' represents the same treatment or group of treatments. The x's represent treatments for which we have only one observation, and we need not have the same number of such treatments in every block.

When the experimental conditions are appropriate for their use, chain blocks are a flexible and efficient design. They are easy to construct. After following through the example below, and with the help of Cochran and Cox,[2] the user should be able to produce a chain block plan suitable to his own needs. For a given number of blocks b and a given number of treatments t, various different plans may be constructed. The analysis is not too difficult, but is not as straightforward as the analysis of some simpler designs.

Two examples of chain block designs (Plan 1 and Plan 2) are given here. The numbers in each block represent treatments.

Plan 1:

4 Blocks ($b = 4$)
13 Treatments ($t = 13$)

Block

1	2	3	4
$\left\{\begin{matrix}1\\2\end{matrix}\right\}$	$\left\{\begin{matrix}3\\4\end{matrix}\right\}$	$\left\{\begin{matrix}5\\6\end{matrix}\right\}$	$\left\{\begin{matrix}7\\8\end{matrix}\right\}$
$\left\{\begin{matrix}3\\4\end{matrix}\right\}$	$\left\{\begin{matrix}5\\6\end{matrix}\right\}$	$\left\{\begin{matrix}7\\8\end{matrix}\right\}$	$\left\{\begin{matrix}1\\2\end{matrix}\right\}$
9	10	11	12
13			

Schematically, Plan 1 may be written:

Block

1	2	3	4
A_1'	A_2'	A_3'	A_4'
A_2''	A_3''	A_4''	A_1''
x	x	x	x
x			

In Plan 1, treatments 1 and 2 constitute the group A_1, which appears in block 1 and block 4; treatments 3 and 4 constitute the group A_2 (in block 1 and block 2); treatments 5 and 6 constitute the group A_3 (in block 2 and block 3); and treatments 7 and 8 constitute the group A_4 (in block 3 and block 4). The remaining treatments (9 through 13) are distributed among the blocks to make the number of treatments per block as equal as possible.

Treatments 1 through 8 appear twice each; treatments 9 through 13 appear once only. Treatment 1 never occurs without treatment 2, treatment 3 never occurs without treatment 4, etc. Thus, the treatments which are replicated twice fall into four groups (schematically A_1, A_2, A_3, A_4), and these groups are the links in the chain of blocks. Treatments 3 and 4 link blocks 1 and 2, treatments 5 and 6 link blocks 2 and 3, treatments 7 and 8 link blocks 3 and 4, and treatments 1 and 2 complete the chain by linking blocks 4 and 1.

Plan 2:

3 Blocks $(b = 3)$
11 Treatments $(t = 11)$

Block		
1	2	3
$\left\{\begin{matrix}1\\2\\3\end{matrix}\right\}$	$\left\{\begin{matrix}4\\5\\6\end{matrix}\right\}$	$\left\{\begin{matrix}7\\8\\9\end{matrix}\right\}$
$\left\{\begin{matrix}4\\5\\6\end{matrix}\right\}$	$\left\{\begin{matrix}7\\8\\9\end{matrix}\right\}$	$\left\{\begin{matrix}1\\2\\3\end{matrix}\right\}$
10	11	

Schematically, Plan 2 may be written:

Block		
1	2	3
A_1'	A_2'	A_3'
A_2''	A_3''	A_1''
x	x	

In Plan 2, the group of treatments 1, 2, and 3 are group A_1; treatments 4, 5, 6 constitute the group A_2; and treatments 7, 8, 9 constitute the group A_3. The remaining two treatments (10 and 11) are assigned to blocks 1 and 2. Treatments 1 through 9 appear twice each, and treatments 10 and 11 appear once each. Treatments 1, 2, and 3 always occur together as a group; treatments 4, 5, and 6 always occur together; and treatments 7, 8, and 9 always occur together. Thus, the treatments which are replicated twice fall into three groups (schematically A_1, A_2, A_3). Group A_2 links blocks 1 and 2, group A_3 links blocks 2 and 3, and group A_1 completes the chain by linking blocks 3 and 1.

To use a given chain block plan, the numbers should be allocated to the treatments at random.

13-4.3.2 Analysis. For purposes of analysis, the observations should be recorded in the form shown in Table 13-5.

The parameters of the plan are:

b = number of blocks in the plan;
k_i = number of observations in the ith block;
t = number of treatments;
m = number of treatments in each group A_i' and A_i'';
N = total number of observations.

Data Sample 13-4.3.2 — Spectrographic Determination of Nickel

The data are spectrographic determinations of nickel content of 42 rods prepared from the same ingot. Only about 18 determinations could be made on the same photographic plate, and there were 42 "treatments" to be compared; therefore, a chain block plan was used. In the experiment, there are three blocks (the photographic plates involved in the determinations) and 42 treatments (the rods). The selected chain block plan is shown schematically in Table 13-6. The parameters of this plan are: $b = 3$, $k = 18$, $t = 42$, $m = 4$, and $N = 54$.

The amounts of nickel were recorded as logarithms (base 10) of the ratio of the intensity of the nickel spectral line to the iron spectral line. In Table 13-7, these determinations have been coded by multiplying by 10^3 and then subtracting 170.

The primary question to be answered is: Are there significant differences among rods (treatments)?

TABLE 13-6. SCHEMATIC REPRESENTATION OF THE CHAIN BLOCK DESIGN DESCRIBED IN DATA SAMPLE 13-4.3.2

	Block 1		Block 2		Block 3
A_1'	1, 2, 3, 4	A_2'	5, 6, 7, 8	A_3'	9, 10, 11, 12
A_2''	5, 6, 7, 8	A_3''	9, 10, 11, 12	A_1''	1, 2, 3, 4
	13		23		33
	14		24		34
	15		25		35
	16		26		36
	17		27		37
	18		28		38
	19		29		39
	20		30		40
	21		31		41
	22		32		42

The numbers in the blocks represent treatments. The parameters of this plan are: $b = 3$, $k = 18$, $t = 42$, $m = 4$, $N = 54$.

TABLE 13-7. SPECTROGRAPHIC DETERMINATION OF NICKEL (DATA SAMPLE 13-4.3.2)

	Plates (Blocks) 1		Plates (Blocks) 2		Plates (Blocks) 3		
	A_1'	8 t_1	A_2'	4 t_5	A_3'	−1 t_9	
		7 t_2		3 t_6		0 t_{10}	
		14 t_3		10 t_7		−3 t_{11}	
		9 t_4		6 t_8		−8 t_{12}	
	A_2''	13 t_5	A_3''	5 t_9	A_1''	1 t_1	
		15 t_6		7 t_{10}		5 t_2	
		12 t_7		2 t_{11}		2 t_3	
		9 t_8		6 t_{12}		0 t_4	
		11		10		5	
		5		9		−1	
		17		6		−3	
		14		7		−6	
		12		6		2	
		13		4		−2	
		14		7		−2	
		12		7		0	
		8		9		1	
		21		10		2	
Total		$B_1 = 214$		$B_2 = 118$		$B_3 = -8$	$G = 324$

13-4.3.2.1 Estimating Treatment and Block Effects. Since the method of estimating treatment effects requires calculation of the estimated block effects, we compute the block effects first.

Procedure

(1) Compute the sum of the observations for each of the groups A'_i, A''_i. Call the totals X'_i, X''_i.

(2) Record the totals X'_1, X''_1, etc., as shown:

X'_1	X'_2	....	X'_{b-1}	X'_b	G'
X''_1	X''_2	...	X''_{b-1}	X''_b	G''
D_1	D_2	...	D_{b-1}	D_b	

Compute:

$$\begin{aligned} D_i &= X'_i - X''_i \\ G' &= X'_1 + X'_2 + \ldots + X'_b \\ G'' &= X''_1 + X''_2 + \ldots + X''_b \\ G''' &= \text{sum of all observations on treatments which occur once only.} \\ G &= G' + G'' + G''' \end{aligned}$$

(3) Compute

$$L_1 = (b - 1)\,(D_1 - D_2) + (b - 3)\,(D_b - D_3) + (b - 5)\,(D_{b-1} - D_4) + \ldots$$

where the sum is over $b/2$ terms if b is even, and $(b - 1)/2$ terms if b is odd.

(4) Compute

$$H = (G'' - G')/mb$$

(5) If there are m treatments in each group A'_i or A''_i, then we may estimate the first block effect as

$$b_1 = L_1/2mb\,.$$

(6) Compute:

$$\begin{aligned} b_2 &= b_1 + D_2/m + H \\ b_3 &= b_2 + D_3/m + H \\ &\ \ \vdots \\ b_b &= b_{b-1} + D_1/m + H\,. \end{aligned}$$

$b_1, b_2, \ldots, b_b$ are the estimated block effects.

Check: The sum of the estimated block effects should equal zero.

(7) The estimated treatment effects t_i are computed as follows:

If the treatment occurs twice, the estimated treatment effect is the average of the two observations minus the average of the estimated block effects for the two blocks in which the observations occur.

If the treatment occurs once, the estimated treatment effect is the observation on the treatment minus the estimate of block effect for the block in which the treatment occurs.

Check: The sum of the estimated treatment effects should equal $G - \frac{1}{2}(G' + G'')$.

Example

(1) See Table 13-7.

Sum of group $A_1' = 38 = X_1'$
Sum of group $A_1'' = 8 = X_1''$

Sum of group $A_2' = 23 = X_2'$
Sum of group $A_2'' = 49 = X_2''$

Sum of group $A_3' = -12 = X_3'$
Sum of group $A_3'' = 20 = X_3''$

(2)

$X_1' = 38$	$X_2' = 23$	$X_3' = -12$	$G' = 49$
$X_1'' = 8$	$X_2'' = 49$	$X_3'' = 20$	$G'' = 77$
$D_1 = 30$	$D_2 = -26$	$D_3 = -32$	

$G' = 49$
$G'' = 77$
$G''' = 198$ (from Table 13-7)
$G = 324$ (from Table 13-7)

(3) In the example, $b = 3$ (odd), and there will be only one term.

$$L_1 = (3 - 1)(30 + 26) = (2)(56) = 112$$

(4)

$$H = \frac{77 - 49}{4(3)} = \frac{28}{12} = 2.33$$

(5)

$$b_1 = \frac{L_1}{2mb} = \frac{112}{(2)(4)(3)} = 4.67$$

(6)

$$b_2 = 4.67 + \frac{-26}{4} + 2.33 = 0.50$$

$$b_3 = 0.50 + \frac{-32}{4} + 2.33 = -5.17$$

Check: $b_1 + b_2 + b_3 = 0$.

Example (cont)

(7) Treatments 1 through 12 occur twice. In estimating these treatments, we need the following averages of block effects:

$$\frac{b_1 + b_3}{2} = \frac{-0.50}{2}$$
$$= -0.25$$

$$\frac{b_1 + b_2}{2} = \frac{5.17}{2}$$
$$= 2.58$$

$$\frac{b_2 + b_3}{2} = \frac{-4.67}{2}$$
$$= -2.33$$

Treatments 1 through 4 (occurring in Groups A_1' and A_1'', in blocks 1 and 3) are estimated as follows:

$$t_1 = \frac{8 + 1}{2} + 0.25$$
$$= 4.75$$

$$t_2 = \frac{7 + 5}{2} + 0.25$$
$$= 6.25$$

$$t_3 = \frac{14 + 2}{2} + 0.25$$
$$= 8.25$$

$$t_4 = \frac{9 + 0}{2} + 0.25$$
$$= 4.75$$

Treatments 5 through 8 (occurring in Groups A_2' and A_2'', in blocks 1 and 2) are estimated as follows:

$$t_5 = \frac{4 + 13}{2} - 2.58$$
$$= 5.92$$

$$t_6 = \frac{3 + 15}{2} - 2.58$$
$$= 6.42$$

$$t_7 = \frac{10 + 12}{2} - 2.58$$
$$= 8.42$$

$$t_8 = \frac{6 + 9}{2} - 2.58$$
$$= 4.92$$

Example (cont)

Treatments 9 through 12 (occurring in Groups A_3' and A_3'', in blocks 2 and 3) are estimated as follows:

$$t_9 = \frac{-1+5}{2} + 2.33$$
$$= 4.33$$

$$t_{10} = \frac{0+7}{2} + 2.33$$
$$= 5.83$$

$$t_{11} = \frac{-3+2}{2} + 2.33$$
$$= 1.83$$

$$t_{12} = \frac{-8+6}{2} + 2.33$$
$$= 1.33$$

Treatments 13 through 42 occur only once, and are estimated as follows:

11 − 4.67 = 6.33	10 − 0.50 = 9.50	5 − (− 5.17) = 10.17
5 − 4.67 = 0.33	9 − 0.50 = 8.50	−1 − (− 5.17) = 4.17
17 − " = 12.33	6 − " = 5.50	−3 − " = 2.17
14 − " = 9.33	7 − " = 6.50	−6 − " = − 0.83
12 − " = 7.33	6 − " = 5.50	2 − " = 7.17
13 − " = 8.33	4 − " = 3.50	−2 − " = 3.17
14 − " = 9.33	7 − " = 6.50	−2 − " = 3.17
12 − " = 7.33	7 − " = 6.50	0 − " = 5.17
8 − " = 3.33	9 − " = 8.50	1 − " = 6.17
21 − " = 16.33	10 − " = 9.50	2 − " = 7.17

Check: $\sum_{i=1}^{42} t_i = 261.00$; $G - \frac{1}{2}(G' + G'') = 324 - 63 = 261$.

13-4.3.2.2 Testing and Estimating Differences in Treatment Effects. To test for differences in treatment effects, we proceed as follows:

Procedure.

(1) Choose α, the significance level of the test.

(2) Look up $F_{1-\alpha}(t-1, N-b-t+1)$, in Table A-5.

(3) Compute $S_b = B_1^2/k_1 + B_2^2/k_2 + \ldots + B_b^2/k_b - G^2/N$.

(4) Compute $S' = (G' - G'')^2/2bm$.

(5) From each of the observations in A_1' subtract the observation on the same treatment in A_1''. Call these differences $d_{11}, d_{12}, \ldots, d_{1m}$, and compute

$$S_1 = (d_{11}^2 + d_{12}^2 + \ldots + d_{1m}^2)/2 - D_1^2/2m.$$

Compute the comparable quantities $S_2, S_3, \ldots, S_b$.

(6) Compute: $S_e = S' + S_1 + S_2 + \ldots + S_b$
and

$$s^2 = S_e/(N - b - t + 1).$$

(7) Compute $S =$ (sum of squares of all the observations) $- G^2/N$.

(8) Compute $S_t = S - S_b - S_e$.

(9) Compute $F = (N - b - t + 1)S_t/(t - 1)S_e$.

(10) If $F > F_{1-\alpha}$, conclude that the treatments differ; otherwise, conclude that the experiment gives no reason to believe that the treatments differ.

Example

(1) Let $\alpha = .01$

(2) $t = 42,\ b = 3,\ N = 54$ (see Table 13-6).
$t - 1 = 41,\ N - b - t + 1 = 10$
$F_{.99}\ (41\ ,\ 10) = 4.17$

(3) See Table 13-7.

$$S_b = \frac{(214)^2}{18} + \frac{(118)^2}{18} + \frac{(-8)^2}{18} - \frac{(324)^2}{54}$$
$$= \frac{59784}{18} - \frac{104976}{54} = 3321.333 - 1944.0$$
$$= 1377.333$$

(4)
$$S' = \frac{(49 - 77)^2}{(2)\ (3)\ (4)} = \frac{784}{24}$$
$$= 32.667$$

(5) $d_{11} = 7$ $\quad d_{13} = 12$
$d_{12} = 2$ $\quad d_{14} = 9$ $\quad D_1 = 30$

$$S_1 = \frac{278}{2} - \frac{900}{8} = 139 - 112.5$$
$$= 26.5$$

$d_{21} = -9$ $\quad d_{32} = -2$
$d_{22} = -12$ $\quad d_{34} = -3$ $\quad D_2 = -26$

$$S_2 = \frac{238}{2} - 84.5 = 119 - 84.5$$
$$= 34.5$$

$d_{31} = -6$ $\quad d_{33} = -5$
$d_{32} = -7$ $\quad d_{34} = -14$ $\quad D_3 = -32$

$$S_3 = \frac{306}{2} - 128 = 153 - 128$$
$$= 25$$

(6)
$$S_e = 32.667 + 26.5 + 34.5 + 25$$
$$= 118.667$$
$$s^2 = \frac{118.667}{10}$$
$$= 11.8667$$

(7)
$$S = 3862 - \frac{(324)^2}{54} = 3862 - 1944$$
$$= 1918$$

(8)
$$S_t = 1918 - 1377.333 - 118.667$$
$$= 422$$

(9)
$$F = \frac{(10)\ (422)}{(41)\ (118.667)} = \frac{4220}{4865.347}$$
$$= 0.8674$$

(10) Since F is not greater than $F_{.99}$, we say there is not sufficient evidence to conclude that treatments (rods) differ.

13-5 LATIN SQUARE PLANS

13-5.1 PLANNING

A Latin square plan (or the Youden square plans in Paragraph 13-6) is useful when it is necessary or desirable to allow for two specific sources of non-homogeneity in the conditions affecting test results. Such designs were originally applied in agricultural experimentation when the two directional sources of non-homogeneity were simply the two directions on the field, and the "square" was literally a square plot of ground. Its usage has been extended to many other applications where there are two sources of non-homogeneity that may affect experimental results — for example, machines, positions, operators, runs, days. A third variable, the experimental treatment, is then associated with the two source variables in a prescribed fashion. The use of Latin squares is restricted by two conditions:

(1) the number of rows, columns, and treatments must all be the same;
(2) there must be no interactions between row and column factors (see Chapter 12, Paragraph 12-1.1, for definition of interaction).

Youden square plans (Paragraph 13-6) are less restrictive than Latin squares; the number of rows, columns, and treatments need not be the same, but only certain number combinations are possible.

As an example of a Latin square, suppose we wish to compare four materials with regard to their wearing qualities. Suppose further that we have a wear-testing machine which can handle four samples simultaneously. Two sources of inhomogeneity might be the variations from run to run, and the variation among the four positions on the wear machine. In this situation, a 4 × 4 Latin square will enable us to allow for both sources of inhomogeneity if we can make four runs. The Latin square plan is as follows: (The four materials are labelled A, B, C, D).

A 4×4 Latin Square

	Position Number			
Run	(1)	(2)	(3)	(4)
1	A	B	C	D
2	B	C	D	A
3	C	D	A	B
4	D	A	B	C

Examples of Latin squares from size 3 × 3 to 12 × 12 are given in Table 13-8. In the case of the 4 × 4 Latin square, four are given; when a 4 × 4 Latin square is needed, one of the four should be selected at random. The procedure to be followed in using a given Latin square is as follows:

(a) Permute the columns at random;
(b) Permute the rows at random;
(c) Assign letters randomly to the treatments.

TABLE 13-8. SELECTED LATIN SQUARES

3 × 3

A	B	C
B	C	A
C	A	B

4 × 4

1

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

2

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

3

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

4

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

5 × 5

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

6 × 6

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

7 × 7

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

8 × 8

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

9 × 9

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

10 × 10

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

11 × 11

A	B	C	D	E	F	G	H	I	J	K
B	C	D	E	F	G	H	I	J	K	A
C	D	E	F	G	H	I	J	K	A	B
D	E	F	G	H	I	J	K	A	B	C
E	F	G	H	I	J	K	A	B	C	D
F	G	H	I	J	K	A	B	C	D	E
G	H	I	J	K	A	B	C	D	E	F
H	I	J	K	A	B	C	D	E	F	G
I	J	K	A	B	C	D	E	F	G	H
J	K	A	B	C	D	E	F	G	H	I
K	A	B	C	D	E	F	G	H	I	J

12 × 12

A	B	C	D	E	F	G	H	I	J	K	L
B	C	D	E	F	G	H	I	J	K	L	A
C	D	E	F	G	H	I	J	K	L	A	B
D	E	F	G	H	I	J	K	L	A	B	C
E	F	G	H	I	J	K	L	A	B	C	D
F	G	H	I	J	K	L	A	B	C	D	E
G	H	I	J	K	L	A	B	C	D	E	F
H	I	J	K	L	A	B	C	D	E	F	G
I	J	K	L	A	B	C	D	E	F	G	H
J	K	L	A	B	C	D	E	F	G	H	I
K	L	A	B	C	D	E	F	G	H	I	J
L	A	B	C	D	E	F	G	H	I	J	K

(If squares of 5×5 and higher are used very frequently, then, strictly speaking, each time we use one we should choose a square at random from the set of all possible squares. Fisher and Yates[3] give complete representation of the squares from 4×4 to 6×6, and sample squares up to the 12×12.

The results of a Latin square experiment are recorded in a two-way table similar to the plan itself. The treatment totals and the row and column totals of the Latin square plan are each directly comparable without adjustment.

13-5.2 ANALYSIS

The analysis of Latin and Youden Squares (see Paragraph 13-6) is based on essentially the same assumptions as the analysis of randomized blocks. The essential difference is that in the case of randomized blocks we allow for one source of inhomogeneity (represented by blocks) while in the case of Latin and Youden squares we are simultaneously allowing for two kinds of inhomogeneity (represented by rows and columns). If we let Y_{ijm} be the observation on the ith treatment which occurs in the jth row and mth column, then we assume that Y_{ijm} is made up of four components; i.e.,

$$Y_{ijm} = \varphi_i + \rho_j + \kappa_m + e_{ijm},$$

where ρ_j is a term peculiar to the jth row, and is constant regardless of column or treatment effects.

κ_m is a term peculiar to the mth column, and is defined similarly to ρ_j.

φ_i is a term peculiar to the ith treatment, and is the same regardless of the row or column in which the treatment occurs. It may be regarded as the average value of the ith treatment for any given row (or column) averaged over all columns (or rows), assuming there is no experimental error.

e_{ijm} is the experimental error involved in the observation Y_{ijm}.

As in the case of randomized blocks, in order to make interval estimates, or to make tests, we generally assume that the experimental errors (e_{ijm}'s) are each independently and normally distributed. However, provided the experiment was randomized properly, failure of the latter assumption will in general not cause serious difficulty.

In the analysis, we assume the data are exhibited in a two-way table following the plan. We use the following notation for the various totals:

T_i = Sum of the observations on the ith treatment;

R_i = Sum of the observations in the ith row;

C_i = Sum of the observations in the ith column;

G = Sum of all the observations.

Data Sample 13-5.2 — Temperature Reference Cells

This is a study of chemical cells used as a means of setting up a reference temperature. For various reasons, only one thermometer could be applied to a cell at one time. The columns are the four thermometers and the rows are the four cells investigated. The letters refer to four runs, each run made on a separate day. The readings are converted to degrees Centigrade; only the third and fourth decimal places are recorded, because all the readings agreed up to the last two places.

Cells	Thermometers I	II	III	IV	Total	Mean
1	A 36	B 38	C 36	D 30	$R_1 = 140$	35.0
2	C 17	D 18	A 26	B 17	$R_2 = 78$	19.5
3	B 30	C 39	D 41	A 34	$R_3 = 144$	36.0
4	D 30	A 45	B 38	C 33	$R_4 = 146$	36.5
Total	$C_1 = 113$	$C_2 = 140$	$C_3 = 141$	$C_4 = 114$	$G = 508$	
Mean	28.25	35.0	35.25	28.5		

13-5.2.1 Estimation of Treatment Effects. The estimate t_i of the ith treatment effect φ_i can be obtained directly by the treatment average T_i/r, where r is the number of times the treatment occurs (r also equals the number of treatments, the number of rows, and the number of columns).

For example, from Data Sample 13-5.2:

$$T_A = 141 \qquad T_C = 125$$

$$T_B = 123 \qquad T_D = 119$$

$r = 4$, and

$$t_A = 141/4 = 35.25 \qquad t_C = 125/4 = 31.25$$

$$t_B = 123/4 = 30.75 \qquad t_D = 119/4 = 29.75$$

13-5.2.2 Testing and Estimating Differences in Treatment Effects.

Procedure	Example
(1) Choose α, the significance level of the test.	(1) Let $\alpha = .05$
(2) Look up $q_{1-\alpha}(r, \nu)$ in Table A-10, where $\nu = (r-2)(r-1)$.	(2) From Data Sample 13-5.2: $r = 4$, $\nu = 6$, $q_{.95}(4, 6) = 4.90$
(3) Compute $S_t = \frac{T_1^2 + T_2^2 + \ldots + T_r^2}{r} - \frac{G^2}{r^2}$	(3) $S_t = \frac{64796}{4} - \frac{258064}{16} = 16199 - 16129 = 70$
(4) Compute $S_r = \frac{R_1^2 + R_2^2 + \ldots + R_r^2}{r} - \frac{G^2}{r^2}$	(4) $S_r = \frac{67736}{4} - 16129 = 805$
(5) Compute $S_c = \frac{C_1^2 + C_2^2 + \ldots + C_r^2}{r} - \frac{G^2}{r^2}$	(5) $S_c = \frac{65246}{4} - 16129 = 182.5$
(6) Compute S = (sum of squares of all the observations) $- G^2/r^2$	(6) $S = 17230 - 16129 = 1101$
(7) Compute: $s^2 = \frac{S - S_t - S_r - S_c}{(r-2)(r-1)}$ and s	(7) $s^2 = \frac{43.5}{6} = 7.25$; $s = 2.693$
(8) Compute $w = q_{1-\alpha}\, s/\sqrt{r}$	(8) $w = (4.90)(2.693)/\sqrt{4} = 6.60$
(9) If the absolute difference between any two estimated treatment effects exceeds w, decide that the treatment effects differ; otherwise, decide that the experiment gives no reason to believe the treatment effects differ.	(9) The largest difference between pairs of treatment effects is 5.50, which does not exceed 6.60. We conclude that treatments (runs) do not differ.

Note: We can make simultaneous statements about the differences between pairs of treatments i and j, with confidence $1 - \alpha$ that all the statements are true simultaneously. The statements are, for all i and j,

$$t_i - t_j - w \le \varphi_i - \varphi_j \le t_i - t_j + w.$$

13-5.2.3 Estimation of Row (or Column) Effects. The row (or column) effects can be estimated directly by subtracting G/r^2 from the row (or column) averages. That is, we estimate ρ_i by $r_i = R_i/r - G/r^2$, and κ_i by $c_i = C_i/r - G/r^2$.

For example, from Data Sample 13-5.2:

$$G/r^2 = 508/16 = 31.75$$

$$r_1 = \frac{140}{4} - 31.75 = 3.25 \qquad c_1 = \frac{113}{4} - 31.75 = -3.50$$

$$r_2 = \frac{78}{4} - 31.75 = -12.25 \qquad c_2 = \frac{140}{4} - 31.75 = 3.25$$

$$r_3 = \frac{144}{4} - 31.75 = 4.25 \qquad c_3 = \frac{141}{4} - 31.75 = 3.50$$

$$r_4 = \frac{146}{4} - 31.75 = 4.75 \qquad c_4 = \frac{114}{4} - 31.75 = -3.25$$

13-5.2.4 Testing and Estimating Differences in Row (or Column) Effects.

Procedure	**Example**
(1) through (7) Same as in Paragraph 13-5.2.2	(1) through (7) Using Data Sample 13-5.2: $s = 2.693$, and ordinarily would have already been computed for the test of Paragraph 13-5.2.2.
(8) Compute $w = qs/\sqrt{r}$	(8) $w = 6.60$
(9) If the absolute difference between any two estimated row effects r_i exceeds w, conclude that the row effects differ; otherwise, there is no reason to believe that row effects differ.	(9) See Paragraph 13-5.2.3. There is at least one pair of row effects that differ by more than 6.60. We therefore conclude that rows (cells) do differ.
If the absolute difference between any two estimated column effects c_i exceeds w, conclude that the column effects differ; otherwise there is no reason to believe that column effects differ.	There is at least one pair of column effects that differ by more than 6.60. We therefore conclude that columns (thermometers) do differ.

Note: We can make simultaneous statements about the differences between pairs of rows i and j with confidence $1 - \alpha$ that all the statements are simultaneously true. The statements are, for all i and j,

$$r_i - r_j - w \leq \rho_i - \rho_j \leq r_i - r_j + w.$$

(For a similar set of statements about the columns, replace

r_i, r_j, ρ_i, ρ_j, by $c_i, c_j, \kappa_i, \kappa_j$).

13-6 YOUDEN SQUARE PLANS

13-6.1 PLANNING

The Youden square, like the Latin square, is used when we wish to allow for two kinds of inhomogeneity. The conditions for the use of a Youden square, however, are less restrictive than for the Latin square. The use of Latin square plans is restricted by the fact that the number of rows, columns, and treatments must all be the same. Youden squares have the same number of rows and treatments, but a fairly wide choice in the number of columns is possible. We use the following notation:

t = number of treatments to be compared;

b = number of levels of one source of inhomogeneity (rows);

k = number of levels of the other source of inhomogeneity (columns);

r = number of replications of each treatment.

In a Youden square, $t = b$ and $k = r$.

In Paragraph 13-5 (Latin Square plans), an example was shown in which we wished to test four materials with regard to their wearing qualities. There were two sources of inhomogeneity; these were the variation among the four positions on the machine, and the variations from run to run. In order to use the Latin square plan, we had to make 4 runs. A Youden square arrangement for this case would require only 3 runs. In all the plans given in Table 13-9, the analysis is essentially the same; and for each of the designs, all differences between treatment effects are estimated with the same precision.

The procedure to be followed in using a given Youden square is as follows:

(a) Permute the rows at random;

(b) Permute the columns at random;

(c) Assign letters at random to the treatments.

The results of an experiment using a Youden square plan are recorded in a two-way table which looks like the plan itself. See the plans shown in Table 13-9.

In some instances where there are two sources of inhomogeneity, a suitable Latin or Youden square may not exist. For a number of sets of values of t, b, and k, other plans or arrangements do exist which enable the experimenter to allow for the two sources of heterogeneity, in a fairly simple manner. Because the analysis and interpretation is more complicated than for the plans given in this Chapter, a statistician should be consulted.

TABLE 13-9. YOUDEN SQUARE ARRANGEMENTS ($r \leq 10$)

Index

Plan Number	$t = b$	$r = k$	λ	$E = t\lambda/rk$	Remarks
1	3	2	1	3/4	*
2	4	3	2	8/9	*
3	5	4	3	15/16	*
4	6	5	4	24/25	*
5	7	3	1	7/9	
6	7	4	2	7/8	†Complement of Plan 5
7	7	6	5	35/36	*
8	8	7	6	48/49	*
9	9	8	7	63/64	*
10	10	9	8	80/81	*
11	11	5	2	22/25	
12	11	6	3	11/12	Complement of Plan 11
13	11	10	9	99/100	*
14	13	4	1	13/16	
15	13	9	6	26/27	Complement of Plan 14
16	15	7	3	45/49	
17	15	8	4	15/16	Complement of Plan 16
18	16	6	2	8/9	
19	16	10	6	24/25	
20	19	9	4	76/81	
21	19	10	5	19/20	Complement of Plan 20
22	21	5	1	21/25	
23	25	9	3	25/27	See Cochran and Cox[2] pp. 529-535.
24	31	6	1	31/36	
25	31	10	3	93/100	
26	37	9	2	74/81	
27	57	8	1	57/64	
28	73	9	1	73/81	
29	91	10	1	91/100	

* Blocks in these Plans are columns of Latin squares with one row deleted.

† The "complement" of a plan is developed as follows: Construct the first block (column) by writing all treatments that did not appear in the first block of the original plan. With these letters as starting points, complete each row by writing in alphabetical order all remaining treatment letters followed by A, B, C, . . . until every treatment letter appears once in each row. For example, Plan 6 is developed from Plan 5 as follows: The first block of Plan 5 is ABD; its complement and therefore the first block of Plan 6 is CEFG. The complete layout for Plan 6 is:

Row	Block						
	1	2	3	4	5	6	7
1	C	D	E	F	G	A	B
2	E	F	G	A	B	C	D
3	F	G	A	B	C	D	E
4	G	A	B	C	D	E	F

Note: The detailed plans given are only those which are not easily derivable from other designs — see Index at beginning of this Table.

Plan 5: $t = b = 7, r = k = 3$

Row	Block						
	1	2	3	4	5	6	7
1	A	B	C	D	E	F	G
2	B	C	D	E	F	G	A
3	D	E	F	G	A	B	C

TABLE 13-9. YOUDEN SQUARE ARRANGEMENTS ($r \leq 10$) (Continued)

Plan 11: $t = b = 11, r = k = 5$

Row	Block 1	2	3	4	5	6	7	8	9	10	11
1	A	B	C	D	E	F	G	H	I	J	K
2	E	F	G	H	J	J	K	A	B	C	D
3	F	G	H	I	[illegible]	K	A	B	C	D	E
4	G	H	I	J	K	A	B	C	D	E	F
5	I	J	K	A	B	C	D	E	F	G	H

Plan 14: $t = b = 13, r = k = 4$

Row	Block 1	2	3	4	5	6	7	8	9	10	11	12	13
1	A	B	C	D	E	F	G	H	I	J	K	L	M
2	B	C	D	E	F	G	H	I	J	K	L	M	A
3	D	E	F	G	H	I	J	K	L	M	A	B	C
4	J	K	L	M	A	B	C	D	E	F	G	H	I

Plan 16: $t = b = 15, r = k = 7$

Row	Block 1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1	A	B	C	D	E	F	G	H	I	J	K	L	M	N	O
2	B	C	D	E	F	G	H	I	J	K	L	M	N	O	A
3	C	D	E	F	G	H	I	J	K	L	M	N	O	A	B
4	E	F	G	H	I	J	K	L	M	N	O	A	B	C	D
5	F	G	H	I	J	K	L	M	N	O	A	B	C	D	E
6	I	J	K	L	M	N	O	A	B	C	D	E	F	G	H
7	K	L	M	N	O	A	B	C	D	E	F	G	H	I	J

Plan 18: $t = b = 16, r = k = 6$

Row	Block 1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
1	A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P
2	B	C	D	A	F	G	H	E	J	K	L	I	N	O	P	M
3	C	D	A	B	G	H	E	F	K	L	I	J	O	P	M	N
4	E	F	G	H	I	J	K	L	M	N	O	P	A	B	C	D
5	L	I	J	K	P	M	N	O	D	A	B	C	H	E	F	G
6	M	N	O	P	A	B	C	D	E	F	G	H	I	J	K	L

TABLE 13-9. YOUDEN SQUARE ARRANGEMENTS ($r \leq 10$) (Continued)

Plan 19: $t = b = 16, r = k = 10$

Row	Block															
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
1	A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P
2	C	A	B	E	F	D	J	I	H	G	M	K	L	P	N	O
3	D	C	A	K	M	G	H	E	L	I	J	B	P	O	F	N
4	N	E	P	A	H	B	D	C	F	K	O	G	I	J	L	M
5	M	N	O	P	B	A	F	D	E	C	G	I	J	H	K	L
6	B	J	H	G	A	I	L	O	M	N	D	C	E	F	P	K
7	L	K	I	B	O	P	N	A	D	F	C	H	G	E	M	J
8	J	H	F	L	G	M	A	P	K	O	B	N	C	D	E	I
9	I	P	L	O	N	K	C	M	J	A	H	E	F	B	D	G
10	O	M	K	J	L	N	P	G	A	E	F	D	B	I	C	H

Plan 20: $t = b = 19, r = k = 10$

Row	Block																		
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
1	A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S
2	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S	A	B
3	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S	A	B	C	D
4	F	G	H	I	J	K	L	M	N	O	P	Q	R	S	A	B	C	D	E
5	G	H	I	J	K	L	M	N	O	P	Q	R	S	A	B	C	D	E	F
6	H	I	J	K	L	M	N	O	P	Q	R	S	A	B	C	D	E	F	G
7	K	L	M	N	O	P	Q	R	S	A	B	C	D	E	F	G	H	I	J
8	N	O	P	Q	R	S	A	B	C	D	E	F	G	H	I	J	K	L	M
9	O	P	Q	R	S	A	B	C	D	E	F	G	H	I	J	K	L	M	N

Plan 22: $t = b = 21, r = k = 5$

Row	Block																				
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
1	A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S	T	U
2	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S	T	U	A
3	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S	T	U	A	B	C	D
4	O	P	Q	R	S	T	U	A	B	C	D	E	F	G	H	I	J	K	L	M	N
5	Q	R	S	T	U	A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P

13-6.2 ANALYSIS

The same model is used, and the same assumptions are made, as in the Latin square analysis in Paragraph 13-5.2. The analysis presented here is sometimes called the intrablock analysis.

In the analysis we assume that the data are exhibited in a two-way table following the plan. (See the plans given in Table 13-9). We label the various totals as follows:

T_i = sum of the observations on the ith treatment;

R_i = sum of the observations in the ith row;

C_i = sum of the observations in the ith column;

G = sum of all observations.

Data Sample 13-6.2 — Intercomparison of Thermometers*

The example involves an intercomparison of thermometers. Seven thermometers, designated by the letters A, B, C, D, E, F, G, were set up in a bath. The bath temperature could not be kept exactly constant, and the experiment was designed so that valid comparisons could be made among the thermometers, despite the variations in bath temperature.

The seven thermometers were read in sets of three, as follows:

	Order of Reading Within a Set		
Set	1	2	3
1	A	B	D
2	E	F	A
3	B	C	E
4	F	G	B
5	C	D	F
6	G	A	C
7	D	E	G

The two sources of inhomogeneity here are the order of reading within a set, and the set-to-set variation.

Number of treatments (thermometers) . . . . $t = 7$
Number of rows (sets) . . . . $b = 7$
Number of columns (order) . . . . $k = 3$
Number of replications of each treatment . . . . . . $r = 3$.

* Adapted with permission from *Statistical Methods for Chemists* (pp. 102-105) by W. J. Youden, copyright, 1951, John Wiley and Sons, Inc.

Data Sample 13-6.2 — Intercomparison of Thermometers (cont)

The thermometers had scale divisions of one-tenth of a degree, and were read to the third place with optical aid. The readings were made just above 30°C; for convenience, only the last two places are entered in the following tabulation, i.e., the entry 56 represents a reading of 30.056°C.

	Order of Reading Within a Set			
Set	1	2	3	Total
1	A 56	B 31	D 35	$R_1 = 122$
2	E 16	F 41	A 58	$R_2 = 115$
3	B 41	C 53	E 24	$R_3 = 118$
4	F 46	G 32	B 46	$R_4 = 124$
5	C 54	D 43	F 50	$R_5 = 147$
6	G 34	A 68	C 60	$R_6 = 162$
7	D 50	E 32	G 38	$R_7 = 120$
Total	$C_1 = 297$	$C_2 = 300$	$C_3 = 311$	$G = 908$

13-6.2.1 Estimation of Treatment Effects. The estimate t_i, of the ith treatment effect φ_i is

$$t_i = Q_i/Er + G/bk,$$

where

$$Q_i = T_i - (n_{i1} R_1 + n_{i2} R_2 + \ldots + n_{ib} R_b)/r$$

T_i = total for the ith treatment

R = total for the row

n_{ij} = the number of times the ith treatment occurs in the jth row.

13-6.2.1 (cont)

For example, using Data Sample 13-6.2,

where $E = 7/9$, $k = r = 3$, $b = 7$, $Er = 21/9$

$$T_A = 182 \qquad T_E = 72$$
$$T_B = 118 \qquad T_F = 137$$
$$T_C = 167 \qquad T_G = 104$$
$$T_D = 128$$

$$\begin{aligned} Q_A &= 182 - \frac{122 + 115 + 162}{3} \\ &= 182 - 133 \\ &= 49 \end{aligned} \qquad \begin{aligned} Q_E &= 72 - \frac{115 + 118 + 120}{3} \\ &= 72 - 117.66667 \\ &= -45.66667 \end{aligned}$$

$$\begin{aligned} Q_B &= 118 - \frac{122 + 118 + 124}{3} \\ &= 118 - 121.33333 \\ &= -3.33333 \end{aligned} \qquad \begin{aligned} Q_F &= 137 - \frac{115 + 124 + 147}{3} \\ &= 137 - 128.66667 \\ &= 8.33333 \end{aligned}$$

$$\begin{aligned} Q_C &= 167 - \frac{118 + 147 + 162}{3} \\ &= 167 - 142.33333 \\ &= 24.66667 \end{aligned} \qquad \begin{aligned} Q_G &= 104 - \frac{124 + 162 + 120}{3} \\ &= 104 - 135.33333 \\ &= -31.33333 \end{aligned}$$

$$\begin{aligned} Q_D &= 128 - \frac{122 + 147 + 120}{3} \\ &= 128 - 129.66667 \\ &= -1.66667 \end{aligned}$$

$$Er = \frac{21}{9},\ bk = 21,\ \frac{G}{bk} = \frac{908}{21} = 43.238095$$

$$\begin{aligned} t_A &= \frac{9\,(49)}{21} + 43.238095 \\ &= 21 + 43.238095 \\ &= 64.238095 \end{aligned} \qquad \begin{aligned} t_E &= \frac{9\,(-45.66667)}{21} + 43.238095 \\ &= -19.571430 + 43.238095 \\ &= 23.666665 \end{aligned}$$

$$\begin{aligned} t_B &= \frac{9\,(-3.33333)}{21} + 43.238095 \\ &= -1.428570 + 43.238095 \\ &= 41.809525 \end{aligned} \qquad \begin{aligned} t_F &= \frac{9\,(8.33333)}{21} + 43.238095 \\ &= 3.571427 + 43.238095 \\ &= 46.809522 \end{aligned}$$

$$\begin{aligned} t_C &= \frac{9\,(24.66667)}{21} + 43.238095 \\ &= 10.571430 + 43.238095 \\ &= 53.809525 \end{aligned} \qquad \begin{aligned} t_G &= \frac{9\,(-31.33333)}{21} + 43.238095 \\ &= -13.428570 + 43.238095 \\ &= 29.809525 \end{aligned}$$

$$\begin{aligned} t_D &= \frac{9\,(-1.66667)}{21} + 43.238095 \\ &= -0.714287 + 43.238095 \\ &= 42.523808 \end{aligned}$$

13-6.2.2 Testing and Estimating Differences in Treatment Effects.

Procedure

(1) Choose α, the significance level of the test.

(2) Look up $q_{1-\alpha}(t, \nu)$ in Table A-10, where

$$\nu = (b-1)(r-2)$$

(3) Compute

$$S_t = \frac{Q_1^2 + Q_2^2 + \ldots + Q_t^2}{Er}$$

(4) Compute

$$S_r = \frac{R_1^2 + R_2^2 + \ldots + R_b^2}{k} - \frac{G^2}{bk}$$

(5) Compute

$$S_c = \frac{C_1^2 + C_2^2 + \ldots + C_r^2}{b} - \frac{G^2}{bk}$$

(6) Compute

S = (sum of squares of all observations) $- G^2/bk$

(7) Compute:

$$s^2 = \frac{S - S_t - S_r - S_c}{(b-1)(r-2)}$$

and

s

(8) Compute

$$w = q_{1-\alpha}\, s/\sqrt{Er}$$

(9) If the absolute difference between any two estimated treatment effects exceeds w, decide that the treatment effects differ; otherwise, decide that the experiment gives no reason to believe the treatment effects differ.

Example

(1) Let $\alpha = .05$

(2) Using Data Sample 13-6.2,

$$\nu = 6\ (1) = 6$$
$$q_{.95}(7, 6) = 5.90$$

(3)

$$S_t = \frac{9}{21}(6160.00019) = 2640.000$$

(4)

$$S_r = \frac{119662}{3} - \frac{824464}{21} = 627.143$$

(5)

$$S_c = \frac{274930}{7} - 39260.190 = 15.524$$

(6)

$$S = 42558 - 39260.190 = 3297.810$$

(7)

$$s^2 = 15.143/6 = 2.524$$
$$s = 1.589$$

(8)

$$w = \frac{5.90\ (1.589)}{1.528} = 6.136$$

(9) See the estimated treatment effects in Paragraph 13-6.2.1. Taken in pairs, there are differences which exceed 6.136, and we conclude that thermometers do differ.

Note: We can make simultaneous statements about the differences between pairs of treatments i and j, with confidence $1 - \alpha$ that all the statements are simultaneously true. The statements are, for all i and j,

$$t_i - t_j - w \le \varphi_i - \varphi_j \le t_i - t_j + w .$$

13-6.2.3 Estimation of Column Effects. The column effects can be estimated directly from the column means; i.e., the estimate of the ith column effect is

$$c_i = C_i/b - G/bk.$$

For example, using Data Sample 13-6.2,

$$\begin{aligned} C_1 &= \frac{297}{7} - \frac{908}{21} \\ &= 42.43 - 43.24 \\ &= -0.81 \end{aligned}$$

$$\begin{aligned} C_2 &= \frac{300}{7} - 43.24 \\ &= 42.86 - 43.24 \\ &= -0.38 \end{aligned}$$

$$\begin{aligned} C_3 &= \frac{311}{7} - 43.24 \\ &= 44.43 - 43.24 \\ &= 1.19 \end{aligned}$$

13-6.2.4 Testing and Estimating Differences in Column Effects.

Procedure	Example
(1) Choose α, the significance level of the test.	(1) Let $\alpha = .05$
(2) Look up $q_{1-\alpha}(k, \nu)$ in Table A-10, where $\nu = (b-1)(r-2)$.	(2) $\nu = 6(1) = 6$ $q_{.95}(3, 6) = 4.34$
(3) through (7) Same as Steps (3) through (7) of Paragraph 13-6.2.2.	(3) through (7) See Paragraph 13-6.2.2. $s = 1.589$
(8) Compute $w_c = q_{1-\alpha}\, s/\sqrt{b}$	(8) $w_c = \frac{4.34\,(1.589)}{2.646} = 2.61$
(9) If the absolute difference between any two estimated column effects exceeds w_c, decide that the column effects differ; otherwise, decide that the experiment gives no reason to believe the column effects differ.	(9) There are no differences between pairs of column effects that exceed 2.61. We conclude that the column effects (order of reading within set) do not differ.

Note: As in the case of treatment effects, we can make a set of simultaneous statements about the difference between pairs of columns i and j. The statements are, for all i and j,

$$c_i - c_j - w_c \leq \kappa_i - \kappa_j \leq c_i - c_j + w_c.$$

13-6.2.5 Estimation of Row Effects. The estimate of the jth row effect ρ_j is $r_j = Q'_j/Er$, where

$$Q'_j = R_j - (n_{1j}T_1 + n_{2j}T_2 + \ldots + n_{bj}T_b)/r$$

and, as before, n_{ij} is the number of times the ith treatment occurs in the jth row.

For example, using Data Sample 13-6.2:

$$\begin{aligned} Q'_1 &= 122 - \frac{182 + 118 + 128}{3} \\ &= 122 - 142.67 \\ &= -20.67 \end{aligned}$$

$$\begin{aligned} Q'_2 &= 115 - \frac{72 + 137 + 182}{3} \\ &= 115 - 130.33 \\ &= -15.33 \end{aligned}$$

$$\begin{aligned} Q'_3 &= 118 - \frac{118 + 167 + 72}{3} \\ &= 118 - 119.00 \\ &= -1.00 \end{aligned}$$

$$\begin{aligned} Q'_4 &= 124 - \frac{137 + 104 + 118}{3} \\ &= 124 - 119.67 \\ &= 4.33 \end{aligned}$$

$$\begin{aligned} Q'_5 &= 147 - \frac{167 + 128 + 137}{3} \\ &= 147 - 144.00 \\ &= 3.00 \end{aligned}$$

$$\begin{aligned} Q'_6 &= 162 - \frac{104 + 182 + 167}{3} \\ &= 162 - 151.00 \\ &= 11.00 \end{aligned}$$

$$\begin{aligned} Q'_7 &= 120 - \frac{128 + 72 + 104}{3} \\ &= 120 - 101.33 \\ &= 18.67 \end{aligned}$$

$$Er = \frac{21}{9}, \frac{1}{Er} = \frac{9}{21}$$

$$\begin{aligned} r_1 &= \frac{9\,(-20.67)}{21} \\ &= -8.86 \end{aligned}$$

$$\begin{aligned} r_2 &= \frac{9\,(-15.33)}{21} \\ &= -6.57 \end{aligned}$$

$$\begin{aligned} r_3 &= \frac{9\,(-1.00)}{21} \\ &= -0.43 \end{aligned}$$

$$\begin{aligned} r_4 &= \frac{9\,(4.33)}{21} \\ &= 1.86 \end{aligned}$$

$$\begin{aligned} r_5 &= \frac{9\,(3.00)}{21} \\ &= 1.29 \end{aligned}$$

$$\begin{aligned} r_6 &= \frac{9\,(11.00)}{21} \\ &= 4.71 \end{aligned}$$

$$\begin{aligned} r_7 &= \frac{9\,(18.67)}{21} \\ &= 8.00 \end{aligned}$$

13-6.2.6 Testing and Estimating Differences in Row Effects.

Procedure	Example
(1) Choose α, the significance level of the test.	(1) Let $\alpha = .05$
(2) Look up $q_{1-\alpha}(b, \nu)$ in Table A-10, where $\nu = (b-1)(r-2)$.	(2) From Data Sample 13-6.2: $(b-1)(r-2) = 6(1) = 6$; $q_{.95}(7, 6) = 5.90$
(3) through (7) Same as Steps (3) through (7) of Paragraph 13-6.2.2.	(3) through (7) See Paragraph 13-6.2.2. $s = 1.589$
(8) Compute $w_r = q_{1-\alpha}\, s/\sqrt{k}$	(8) $w_r = \dfrac{5.90\,(1.589)}{1.732} = 5.41$
(9) If the absolute difference between any two estimated row effects exceeds w_r, decide that the row effects differ; otherwise, decide that the experiment gives no reason to believe that row effects differ.	(9) There are differences between pairs of row effects that exceed 5.41. Therefore, we conclude that rows (sets) do differ.

Note: As in the case of the treatment and column effects, we can make a set of simultaneous statements about the differences between pairs of columns i and j. The statements are, for all i and j,

$$r_i - r_j - w_r \leq \rho_i - \rho_j \leq r_i - r_j + w_r.$$

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CHAPTER 14

EXPERIMENTS TO DETERMINE OPTIMUM CONDITIONS OR LEVELS

14-1 INTRODUCTION

In many industrial-type processes, there is a measurable end-property whose value is of primary interest and which we would like to have attain some optimum value. This end-property is called *yield* or *response* in the language of experimental design. For example, the end-property might be:

(a) the *actual yield* of the process, which we would like to maximize;

(b) a strength property, which we would like to maximize;

(c) cost, which we would like to minimize; or,

(d) some chemical or physical characteristic that would be most desirable at a maximum or at a minimum, as specified.

The value of this primary end-property will depend on the values or settings of a number of factors in the process which affect the end-property. In such cases, the goal of experimentation is to find the settings of the factors which result in an optimum response. Often, we are interested in knowing not only the values of the variables that result in optimum response, but also how much change in response results from small deviations from the optimum settings — i.e., we would like to know the nature of the response function in the vicinity of this optimum.

14-2 THE RESPONSE FUNCTION

In a factorial experiment where the levels of all factors are quantitative (e.g., time, temperature, pressure, amount of catalyst, purity of ingredients, etc.), we can think of the response y as a function of the levels of the experimental factors. For an n-factor experiment, we could write:

True yield

$$y = \Phi(x_1, x_2, \ldots, x_n)$$

where

x_1 = level of factor 1
x_2 = level of factor 2
etc.

For observed values of y, we can write:

$$Y_u = \Phi(x_{1u}, x_{2u}, \ldots, x_{nu}) + e_u$$

where

Y_u = the uth observation of y, where $u = 1, 2, \ldots, N$ represent the N observations in the factorial experiment;
x_{1u} = level of factor 1 for the uth observation;
x_{2u} = level of factor 2 for the uth observation;
etc.;

and

e_u = the experimental error of the uth observation.

The function Φ can be called the response function. If we could determine the function Φ, we could describe the results of the experiment completely, and could even predict y for values of the factors that were not included in the experiment (but the function *should not* be used for prediction outside the range of experiment). Ordinarily, the mathematical form of the function is completely unknown, but often it can be satisfactorily approximated within a limited region by a polynomial in x_{iu}. Just as the relation $y = \Phi(x)$ can be represented by a curve, the relation between y and two factors x_1 and x_2, i.e., $y = \Phi(x_1, x_2)$, can be represented by a surface called the response surface, as shown in Figure 14-1; or, alternatively, by a contour diagram which traces contours of equal response as shown in Figure 14-2.

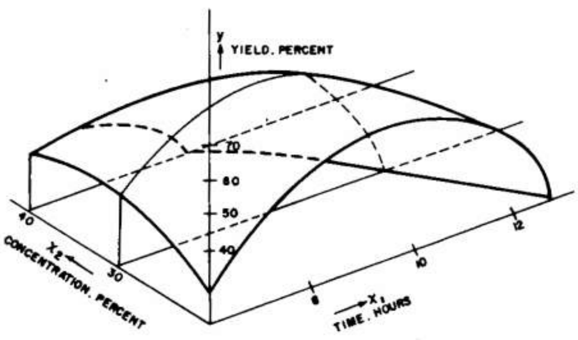

Figure 14-1. A response surface.

Adapted with permission from *The Design and Analysis of Industrial Experiments*, edited by Owen L. Davies, Copyright, 1954, Oliver and Boyd, Ltd., Edinburgh.

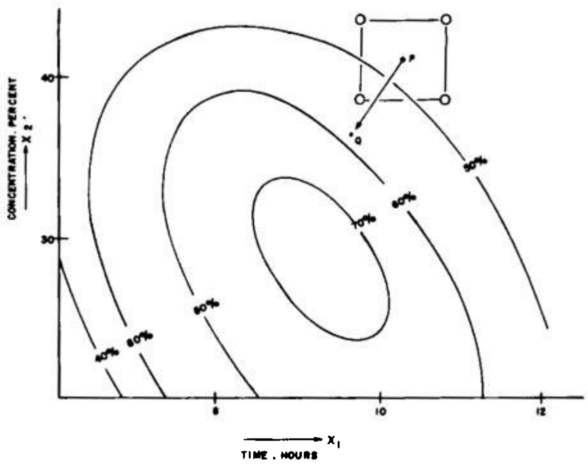

Figure 14-2. Yield contours for the surface of Figure 14-1 with 2^2 factorial design.

Adapted with permission from *The Design and Analysis of Industrial Experiments*, edited by Owen L. Davies, Copyright, 1954, Oliver and Boyd, Ltd., Edinburgh.

The study of response surfaces is a very complex topic. A general notion of possible applications is given here, but no details are provided. An extensive bibliography is given at the end of this Chapter. Since this is a relatively new field, the bibliography is fairly complete at the time of preparation.

14-3 EXPERIMENTAL DESIGNS

Experimental designs and methods of analysis have been developed for fitting polynomials of the first and second degree; these designs are called first and second order designs, respectively. One will hear these designs described as, for example, "a first order design in 2 dimensions" or "a second order design in 4 dimensions" — in general, a kth order design in n dimensions. The dimension n refers to the number of independent variables (x_i) in the response function, and the order k refers to the degree of the fitted polynomial function.

A design in which only one variable is controlled is a one-dimensional design, and we observe y as a function of the single variable x, i.e., $y = \Phi(x)$. The first approach in describing such a relationship may be that of fitting a first order equation, i.e., a straight line $y = \beta_0 + \beta_1 x$, as detailed in AMCP 706-110, Chapter 5. If it has been determined that the relationship cannot be adequately represented by a straight line, a second-degree (or higher degree) polynomial may be fitted as detailed in AMCP 706-110, Chapter 6. A one-dimensional design, however, is not usual in this kind of experimentation and, ordinarily, more variables will be involved.

If we are interested in studying response y as a function of two variables (x_1 and x_2), we represent the function as

$$y = \Phi(x_1, x_2).$$

Again, as a first step, we could fit a first order model (now the equation of a plane)

$$y = \beta_0 + \beta_1 x_1 + \beta_2 x_2.$$

Where three or more variables are controlled, we have a function of the type

$$y = \Phi(x_1, x_2, \ldots, x_n).$$

A general aim in selecting and constructing experimental designs when observing a function of several quantitative variables, is that the selected design should permit relatively simple and straightforward estimation of the coefficients of the fitted equation. Two-level factorial designs are important designs for fitting first order models — particularly in the two-dimensional case. New designs, with special advantageous properties, have been developed by G.E.P. Box and followers. Most first order designs will provide information about the adequacy of the first order model, and second order designs are available when first order models are inadequate.

14-4 FINDING THE OPTIMUM

In general, experimentation proceeds sequentially. Initial levels of the variables are chosen so that the levels are either near present operating conditions or are believed to be near optimum response. A design is chosen, and experimental observations are made at values of the variables which are specified by the design. In general, first order designs will provide information on the adequacy of the first order model, will indicate whether the response is near the optimum, and will indicate the direction to move to approach closer to the optimum. Another first order design may then be run at a new position, or a second order design may be run at the original position. The methods are extremely flexible and useful. A complete description of the methods cannot be included here, and the reader is advised to consult the references described in Paragraph 14-5.

14-5 RECOMMENDED SOURCES FOR FURTHER STUDY

The bibliography contains references which have been classified into three groups:

I. Elementary and Introductory Reading
II. Advanced Reading
III. Applications.

Group I contains those articles that will be most helpful to the novice. For the reader who is completely unacquainted with the techniques, the following reading program is suggested. First, read the series of articles by Bradley(1) and Hunter(2) which appeared in *Industrial Quality Control*. Follow this by reading the appropriate chapter in Cochran and Cox(3) or Davies(4), or by reading the Hunter article(5). Another introductory article, which requires a higher level of mathematical background, is by Box and Hunter(6). From these introductory readings, proceed to the articles in Group II or III which are of particular interest.

The classification into the three groups had to be somewhat arbitrary. In particular, the reader will notice some anomalies in Group II where some articles are not highly mathematical, but have been included for historical reasons. The level of mathematics required for the Group II references varies a great deal, but one can ordinarily predict the level by knowledge of the journal in which the article appears.

Group III contains articles that deal primarily with applications.

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