NASA TECHNICAL NOTE

NASA TN D-4100

NASA TN D-4100

DESIGNS OF EXPERIMENTS AS TELESCOPING SEQUENCES OF BLOCKS FOR OPTIMUM SEEKING (AS INTENDED FOR ALLOY DEVELOPMENT)

by Arthur G. Holms

Lewis Research Center
Cleveland, Ohio

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION • WASHINGTON, D. C. • AUGUST 1967

NASA TN D-4100

DESIGNS OF EXPERIMENTS AS TELESCOPING SEQUENCES OF BLOCKS FOR OPTIMUM SEEKING (AS INTENDED FOR ALLOY DEVELOPMENT)

By Arthur G. Holms

Lewis Research Center
Cleveland, Ohio

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

For sale by the Clearinghouse for Federal Scientific and Technical Information
Springfield, Virginia 22151 – CFSTI price $3.00

DESIGNS OF EXPERIMENTS AS TELESCOPING SEQUENCES OF BLOCKS FOR OPTIMUM SEEKING (AS INTENDED FOR ALLOY DEVELOPMENT)

by Arthur G. Holms

Lewis Research Center

SUMMARY

Box-Wilson methods of design and analysis are proposed for optimum seeking experiments. These methods begin with a first-order polynomial equation fitted to empirical data to predict those modifications that should result in the greatest improvement. The predictions are usually tested with new experiments and a new first-order prediction equation is fitted. If the vicinity of the optimum has been reached, the response function will be curved and a second-order equation is fitted to a much larger experiment.

Ideally, the designs of the experiments should have two attributes:

(1) If a minimum experiment to fit a first-order equation has been performed and the model is thought to be of doubtful validity, a larger experiment should be performed that will contain the earlier experiment as a nucleus. In this way a "telescoping" sequence of experiments might be performed up to the largest size envisioned in Box-Wilson methods, namely, the experiment to estimate the full second-order equation.

(2) The constants of the larger equations should be estimated without bias by uncontrolled changes that may have occurred in the material or equipment between the performances of the experiments. Detailed designs of experiments that possess these attributes are presented.

The designs consist of full and fractionally replicated two-level factorial experiments with four to eight factors. The sizes of the experiments include 8, 16, 32, and 64 treatments.

INTRODUCTION

In seeking optimum processing conditions or optimum compositions, investigators in many fields of technology find that the experimenting is intrinsically expensive and time consuming. For example, a program of high-temperature alloy development consists of melting experimental alloys of relatively expensive metals with the use of sophisticated equipment (such as vacuum furnaces) followed by the fabrication and long-time testing of specimens. The alloys typically contain many constituents, and information on the joint effects of changes in the amounts of these constituents can be obtained only from tests of a large number of different melts. For this reason and because the process often introduces large experimental error, the most reliable of minimum-size statistically designed experiments are needed for the economic achievement of valid conclusions.

A series of small experiments could be performed to find those conditions (such as composition levels of the elements) that would optimize response (achieve maximum strength). Methods for designing particularly efficient experiments for this purpose are described in reference 1 and are known as Box-Wilson methods. Illustrations of their use in alloy development are described in reference 2.

The Box-Wilson methods assume that the optimum seeking begins with experiments chosen to estimate the constants of a first-order equation for the response. This phase of the experimenting is called the method of steepest ascents. When the optimum condition is approached, the first-order model is no longer applicable, and the constants of a second-order model must be estimated from larger experiments. This phase is called the method of local exploration. The second-order model contains expressions called interactions and they represent the fact that the response to one independent variable has become dependent on the level of one or more other independent variables.

The full factorial experiment where the response is observed for all combinations of the independent variables provides complete estimates of all possible interactions. However, such complete information is seldom required in the method of local exploration, and even less information is needed for the method of steepest ascents. For either the method of steepest ascents of the method of local exploration, a fraction of the full factorial experiment might be all that is required, and such a design is called a fractional replicate.

Parts of an experiment are sometimes performed in a sequence over differing time segments, over differing batches of raw material, or over differing pieces of equipment. These differing conditions are assumed to affect the response from one part to the next by amounts that are not readily predicted or controlled; however, the experimental units within the part are assumed to be uniform with respect to the conditions. The responses to changes between the parts of the experiment are called block effects. Experiments that are designed to estimate the constants of the model equation without contamination from block effects are called orthogonally blocked designs.

A discussion of preferred designs of experiments for seeking optimum conditions was presented by Box and Hunter (ref. 3) in which the use of sequences of blocked fractional factorial designs was introduced, but exact details of the combinations of levels of the independent variables (treatments) were not presented. A large collection of specified treatments for blocked fractional factorial designs is given by the National Bureau of Standards in reference 4; however, it does not discuss the choosing of detailed designs from reference 4 to meet the objectives of reference 3. As a matter of fact, the requirements of reference 3 seldom lead to designs in reference 4. The designs that should be used also depend on the particular situations facing the experimenter in addition to the requirements of reference 3.

The purpose of the present report is to furnish designs similar to those in reference 4 that will meet the objectives of reference 3 and which will also be appropriate for the particular situations occurring in alloy development. Conceivably, many other development situations could lead to the same family of designs.

The cost of experimental units (melts) in alloy development is very high and their number should be minimized. A central feature of the use of blocked designs in optimum seeking experiments is that a single block might be used in the method of steepest ascents. When the method of local exploration is invoked, the experimenting might be continued by using the block already completed as the first block of that series of blocks required for the method of local exploration. The present report therefore presents sequences of blocked designs such that the first block is an efficient design for the method of steepest ascents and such that completion of the blocks will result in an efficient design for the method of local exploration. Such sequences will be called <u>telescoping</u> sequences of designs. Their design has been discussed briefly in reference 5.

The scope of the investigation has been limited to situations involving four to eight factors, and the sizes of the experiments have been limited to 8, 16, 32, and 64 treatments.

The point at which the decision is made to shift from the method of steepest ascents to the method of local exploration is critical because of greatly increased sizes of experiments required by the method of local exploration. In essence, the decision requires tests of significance for the coefficients estimated in the method of steepest ascents. Procedures appropriate to such tests are discussed in references 6 to 9.

SYMBOLS

b number of blocks

E( ) value of ( ) if averaged over infinite number of observations

g number of independent variables (factors)

h fractional replicate contains $1/2^h$ times number of treatments performed in full two-level factorial experiment

i index number for trials

j, k	index number for independent variables
l	g - h
R	resolution level
S_j	scale factor (eq. (3))
U	response (dependent variable)
X_j	vector giving levels of x_{ij}, $i = 1, \ldots, n$
x_{ij}	standardized level of ξ_j defined by eq. (3)
Y	response (dependent random variable)
y	response (observed variate)
β	unknown population parameter
ϵ	error
ξ_j	independent variable, $j = 1, \ldots, g$
σ^2	variance of ϵ
φ	function of independent variables giving E(Y)
$\sum_{i=1}^{n}()$	terms in ( ) summed as i varies over 1, 2, . . ., n

BOX-WILSON METHODS AND BOX-HUNTER DESIGNS

This section presents a selective review of the Box-Wilson methods of reference 1 together with a selective review of the Box-Hunter designs of references 3 and 10. An extensive bibliography of these subjects is presented in reference 11. This review is selective in that it presents only those concepts of references 1, 3, and 10 that provide background for the contributions of the present investigation, which is discussed in terms of alloy development involving four to eight independent variables.

Model for Response

Assume that with every observation of response Y there is some error ϵ and, that aside from the error, the response is some unknown function φ of the imposed conditions $\xi_1, \ldots, \xi_g$:

$$Y = \varphi(\xi_1, \ldots, \xi_g) + \epsilon$$

If the error is averaged over a very large (infinite) number of observations, such averaging is represented by $E(\epsilon) = 0$, that is,

$$E(Y) = \varphi(\xi_1, \ldots, \xi_g) \tag{1}$$

Assume also that the observation error variance is some constant σ^2, that is,

$$E(\epsilon^2) = \sigma^2 \tag{2}$$

In some experimentation σ^2 might not be constant with changes in $\xi_1, \ldots, \xi_g$. In such cases, there often is a transformation of Y that results in approximately constant error variance. For example, if U were time to failure, then in many cases $Y = \log U$ would have an approximately constant error variance.

Equation (1) expresses the belief that the response to be optimized is some definite but unknown function of the variables that can be controlled.

An approximation to equation (1) is estimated from experiments and the estimating function is used to predict conditions of the $\xi_1, \ldots, \xi_g$ that should give a response superior to any already observed. The predictions are then checked experimentally. If they are found to be invalid, some new experimentation is performed to improve the approximation function.

The approximating function does not have to provide a map over the entire ranges of all the variables. All that is needed is a starting point for the experimentation and a procedure that will lead through a short sequence of experiments to a point at which there is high confidence that the optimum is satisfactorily close.

Sequential Experimentation

Experiments associated with alloy development are usually accompanied by two circumstances:

(1) The experimentation is expensive.

(2) The experimenter has a large body of knowledge that is more or less applicable.

These two circumstances dictate that the work should proceed by small stages where the experimenter alternately

(1) Uses his prior knowledge to plan the next stage of experimentation

(2) Performs the experiment and uses the results to revise previously held hypotheses and then suggests new hypotheses to be tested by appropriate new experiments

A variety of useful techniques is needed that consist of

(1) Efficient strategies of experimentation

(2) Informative procedures of data reduction so that, at each stage of the experimenting, the experimenter will know

 (a) What trends are indicated

 (b) How clearly these trends are distinguished from random error

Notation for Conditions of Independent Variables

Let $\xi_1, \ldots, \xi_g$ be the controlled variables. Designate the serial number of each trial and observation by the subscript i, $i = 1, 2, \ldots, n$. Define standard levels for the variables by

$$x_{ij} = \frac{\xi_{ij} - \bar{\xi}_j}{S_j} \qquad \begin{Bmatrix} i = 1, \ldots, n \\ j = 1, \ldots, g \end{Bmatrix} \tag{3}$$

For example, if ξ_1 were percentage tungsten and two levels were investigated (e.g., 10 and 20 percent) and if percentage boron ξ_2 were investigated at two levels (0 and 0.4 percent), then

$$\bar{\xi}_1 = \frac{10 + 20}{2} = 15 \text{ percent}$$

$$\bar{\xi}_2 = \frac{0 + 0.4}{2} = 0.2 \text{ percent}$$

The coordinates $\bar{\xi}_1$ and $\bar{\xi}_2$ locate the design center of the experiment in the original or natural units. The quantity S_j is a scale factor that is adjusted so that equation (3) will represent the upper levels with +1 and lower levels with -1.

For $x_{i1} = +1$:

$$\frac{20 - 15}{5} = +1$$

For $x_{i1} = -1$:

$$\frac{10 - 15}{5} = -1$$

For $x_{i2} = +1$:

$$\frac{0.4 - 0.2}{0.2} = +1$$

For $x_{i2} = -1$:

$$\frac{0.0 - 0.2}{0.2} = -1$$

that is, the scale factors are $S_1 = 5$ and $S_2 = 0.2$. (The design center is at 15 percent tungsten and 0.2 percent boron for which $x_{i1} = 0$ and $x_{i2} = 0$).

The design of the experiment is required to be balanced. This requirement means that on summing over all n conditions (treatments),

$$\sum_{i=1}^{n} x_{ij} = 0 \tag{4}$$

for all independent variables, $j = 1, \ldots, g$.

Model Fitting

The knowledge gained from the experiments is expressed quantitatively by equations with constants that have been fitted to the data. The equations are then used to suggest values of the independent variables that might give responses superior to those already observed. The model fitting begins with an attempt to estimate equation (1). If there is no prior information about the form it should take, a polynomial approximation is assumed, because the method of least squares is a highly effective method of fitting polynomial equations to empirical data. If prior information justifying some particular functional form is available, the particular form could be used so that a polynomial equation in transformed variables is fitted to the data. In the standardized variables of equation (3) the polynomical approximation of equation (1) is

$$
\begin{aligned}
E(Y) = \beta_0 &+ \beta_1 x_1 + \beta_{11} x_1^2 + \beta_{111} x_1^3 + \ldots \\
&+ \beta_2 x_2 + \beta_{22} x_2^2 + \beta_{222} x_2^3 + \ldots \\
&+ \ldots \\
&+ \beta_g x_g + \beta_{gg} x_g^2 + \beta_{ggg} x_g^2 + \ldots \\
&+ \beta_{12} x_1 x_2 + \beta_{13} x_1 x_3 + \ldots \\
&+ \beta_{112} x_1^2 x_2 + \beta_{113} x_1^2 x_3 + \ldots \\
&+ \beta_{1112} x_1^3 x_2 + \beta_{1113} x_1^3 x_3 + \ldots \\
&+ \beta_{122} x_1 x_2^2 + \beta_{1122} x_1^2 x_2^2 + \ldots \\
&+ \ldots \qquad (5)
\end{aligned}
$$

If the number of variables g is large, the number of terms needed in equation (5) is extremely large, especially if the model fitting is to be valid over a wide range of the independent variables. The range of the independent variables is assumed to be restricted, so that for any one experiment, no terms higher than second degree will be needed:

$$
\begin{aligned}
E(Y) = \beta_0 &+ \beta_1 x_1 + \beta_2 x_2 + \ldots + \beta_g x_g \\
&+ \beta_{11} x_1^2 + \beta_{22} x_2^2 + \ldots \beta_{gg} x_g^2 \\
&+ \beta_{12} x_1 x_2 + \beta_{13} x_1 x_3 + \ldots + \beta_{1g} x_1 x_g \\
&\qquad + \beta_{23} x_2 x_3 + \ldots + \beta_{2g} x_2 x_g \\
&\qquad\qquad + \ldots \\
&\qquad\qquad\qquad + \beta_{g-1,g} x_{g-1} x_g \qquad (6)
\end{aligned}
$$

In other words, the path over which the experimenter travels to reach an optimum point might extend over very wide ranges of the independent variables; however, the groping along this path proceeds in small stages and some version of equation (6) is newly evaluated at each stage.

"One At a Time" Experimenting

Two strategies for the empirical attainment of optimum conditions are illustrated by figure 1. The true response on two variables is shown by the contour lines, but, of course, the experimenter begins with essentially no knowledge of these lines.

The "one at a time" strategy of experimenting is illustrated by the lines with Roman numerals in figure 1. At constant x_2, x_1 is varied along line I until the maximum of Y on line I is attained. At this maximum on x_1, the quantity x_2 is varied along line II to find the maximum which occurs at the intersection of lines II and III. This intersection is not necessarily the maximum point, but merely the maximum on line II. Only very precise experimentation on line III could lead to a new value of x_1 at line IV; that is, the response is so flat along line III that random error could easily hide the location of the true maximum along line III. Only very precise experimenting could lead through the many cycles of variation of x_1 and x_2 along lines IV, V, and so forth, needed to reach the true optimum.

Method of Steepest Ascents

The method of steepest ascents assumes that the starting point is sufficiently far away from the optimum point that the response function is not particularly curved, as at A of figure 1. In particular, assume that the response function can be represented by a first-order function of the independent variables, so that the response is represented by the equation

$$E(Y) = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + \ldots + \beta_g x_g \tag{7}$$

For g independent variables there is a need to evaluate (estimate) g + 1 parameters so that at least g + 1 trials under independent conditions must be performed to give that many observations of Y.

With the assumption that the parameters of equation (7) have been estimated and that the linear equation is decided to be adequate, the direction (with respect to the coordinates, fig. 1) that produces the steepest response of Y can be determined (ref. 12).

Assume that this direction is the direction in which the conditions of experimenting are changed as indicated by points numbered 1, 2, 3, and 4 of figure 1.

Elect the maximum point indicated by such a series to be the starting point (design center) for a second experiment capable of again evaluating the parameters of equation (7). This usage of equation (7) to determine directions of improved response gives the method its name of steepest ascents. Steepest ascents, by itself is obviously self defeating, because as the maximum point is approached, the surface curvatures (as at point B of fig. 1) will prevent equation (7) from being an adequate approximation to the true surface.

Decision Making

The strategy of steepest ascents, in addition to fitting equation (7), must provide additional information that will eventually show that equation (7) is no longer valid. This information must come from a few more observations than the minimum $g + 1$. Each additional observation provides one additional "degree of freedom" and these additional degrees of freedom are used in some sense as a measure of "lack of fit." Methods that can be used for testing the validity of equation (7) are discussed in references 6, 7, 8, and 9.

Method of Local Exploration

If the lack of fit is excessive, appropriate experiments will be needed to evaluate all the coefficients of equation (6). For experiments with four to eight independent variables (factors), the important designs of experiments are known as the <u>hypercube</u> and the <u>star</u> designs. The terminology used is illustrated by figure 2. If all combinations of all factors are investigated at two levels, there results 2^g observations and the experiment is called a hypercube design. If $1/2^h$ of such treatments are performed, the experiment is called a <u>fractional replicate</u>. It contains 2^{g-h} observations on independent treatments. Whereas a severely fractionated two-level fractorial experiment is adequate for estimating the coefficients of equation (7), a less severely fractionated (larger) experiment is needed to estimate additionally the cross product coefficients β_{ij}, $i \neq j$ of equation (6).

The estimation of the coefficients β_{ii} of the quadratic terms requires the performance of experiments with points on the coordinate axes of independent variables at distances ρ_s from the design center; that is, with coordinates

$$\left(\pm\rho_s, 0, 0, \ldots, 0\right)$$

$$\left(0, \pm\rho_s, 0, \ldots, 0\right)$$

$$\ldots$$

$$\left(0, 0, \ldots, 0, \pm\rho_s\right)$$

The design is called a star design (fig. 2). If the 2^{g-h} design is augmented by the star design plus at least one additional point at the design center, the composite experiment becomes efficient for estimating all the parameters of equation (6). (The optimal value of ρ_s and the optimal number of center points are discussed in ref. 3).

With the parameters of equation (6) estimated by the method of least squares, the usual mathematical methods can be used to find the point of horizontal tangency. This point might be a maximum, a minimum, or a saddle point. If the point of horizontal tangency is located beyond the range of the conditions of the experiment, the second-order model (eq. (6)) is probably not valid for such an extrapolation and new experiments must be performed in the direction of the indicated maximum.

An invaluable aid in drawing conclusions from the fitted equation (6) is the method of canonical reduction (refs. 1, 10, and 12). Briefly, the method shows whether the point of horizontal tangency is a maximum, a minimum, or a saddle point. For problems of more than a few variables, the point of horizontal tangency is usually some kind of saddle point from which the experimenter might proceed along one or both of two rising ridges. Physical considerations may dictate that only one of the ridges should be followed, but otherwise both directions should be explored, because the second-degree equation may have oversimplified the true situation.

Total Box-Wilson Process

In summary, the total process of Box-Wilson methods, as visualized herein for alloy development, is exhibited by figure 3. The existence of experimental error can lead to decision errors in answer to the questions, "Is the first-order model adequate?" and "Has a true maximum or acceptable solution been attained?" Accordingly, the best available statistical techniques should be used at these two branch points. The style of the diagram of figure 3 might indicate to some experimenter that his insight and judgment are to be replaced by a computer. Nothing could be further from the truth. In the terminology of equation (3) it is the experimenter who must decide the following:

(1) He must decide which factors should be varied. They are then labeled $\xi_1, \ldots, \xi_g$. In taking this step, there should be no intention that other variables will be added later. The full list of potential factors should be incorporated into the initial experiments so that interaction effects among the factors can be observed. Of course, any factors that prove to be nonsignificant may be dropped from the investigation at such time as their nonsignificance has been clearly demonstrated.

(2) He must decide in what way the factors should be varied. For example, if Y were the velocity of a fluid, then ξ_1 might be the square root of a differential pressure. In other words, the experimenter should attempt to define the ξ_j so as to achieve a linear response of Y on the ξ_j. In alloy development, the element additions are sometimes associated with a "diminishing returns" phenomenon so that very small quantities of a specific element produce large changes in strength, whereas large quantities produce relatively small changes. In such cases, setting ξ_j equal to the logarithm of the percentage composition might be an advantage. After deciding on what linearizing transformation to use, the experimenter would then pick the levels of the variables in the manner of equation (3).

(3) He must decide the starting region of experimentation. This is specified by the selection of the design center $\bar{\xi}_1, \bar{\xi}_2, \ldots, \bar{\xi}_g$.

(4) He must decide by how much the factors should be varied; that is, the experimenter chooses the magnitude of the difference between ξ_{ij} and $\bar{\xi}_j$, which determines the magnitude of the scaling constant S_j of equation (3).

After these decisions have been made, the variables are standardized to the x-values of equation (3) and such experimental points are included as will satisfy equation (4) together with other criteria that have been advanced in reference 3 and in the present investigation.

The question of the location of star points and the question of the number of center points is dealt with in reference 3. The present investigation is limited to details of the design of the fractional hypercube experiment to estimate the parameters of equation (7) and to details of the enlarged hypercube experiment to estimate the interaction parameters of equation (6). The use of Yates' method (ref. 12) will be assumed for estimating the linear and interaction coefficients. Of course, more sophisticated methods would be needed to fit all the coefficients of equation (6) following the acquisition of data corresponding to the star and center points.

Two or More Dependent Variables

Problems involving two or more dependent variables are reasonably treated by determining response functions for all such variables. Typically, one of them will deserve to be optimized while the others need merely be controlled. If, for example, the dependent variables are rupture life and ductility, the investigator might specify a minimum ductility and then maximize the rupture life under such a constraint. Contour lines of rupture life and ductility plotted on coordinates consisting of the independent variables (composition, for example) would show how to reach a condition maximizing rupture life at the specified ductility.

Two or More Maximum Points

A serious error could be made in locating the optimum point if the response function contained two or more significantly unequal maximum points and if these points were sufficiently distant from each other so that the larger maximum was not discovered. If the possibility of two or more maximum points is suspected from prior physical considerations, then early experimenting should consist of factorial experiments on more than two levels; the experimentation should be on a grid of points spanning the entire range of interest of the independent variables. Such experimentation would be far more expensive than the Box-Wilson experimenting, which only defines a path leading to a single optimum condition.

CRITERIA FOR SELECTING DESIGNS OF EXPERIMENTS

Telescoping Designs

The experiment used to estimate the coefficients of the first-order model (eq. (7) in the method of steepest ascents) might or might not be large enough to estimate all the two-factor interaction coefficients of the second-order model (eq. (6) in the method of local exploration). If the factorial experiment was severely fractionated for the first-order model, then additional fractions of the full factorial experiment must be performed for the second-order model. As already mentioned, the additional fractions are ideally performed as parts of a blocked experiment, which means that the block effects will not decrease the accuracy of (will be <u>orthogonal</u> to) the estimates of the first-order and interaction coefficients. When a sequence of orthogonal blocks is designed so that observations from the first block may be used to estimate the coefficients of a simple model, and then be retained and combined with observations from new blocks so that all acquired observations are used cumulatively to estimate models of successively greater and greater generality, the blocks will be said to form a <u>telescoping</u> design.

The experiments in the method of steepest ascents are typically performed at more than one design center before the more elaborate experiments are performed to estimate the interactions for the method of local exploration. At any given design center, the experimenter seldom has complete prior information as to just which interactions need to be evaluated. Economy in the total program is therefore to be achieved if the experimenter can "feel his way" into the more complex models. This is to be done with the use of the telescoping sequences of designs.

Resolution Levels

The factorial experiment with conditions fixed at just two levels of g independent variables (factors) permits the estimation of parameters representing the grand mean over the experiment, the first-order effects of the factors, and the results of factors interacting two at a time, three at a time, and in all combinations up to g at a time. If a fraction $1/2^h$ of this experiment is performed, not all these parameters can be estimated. True response functions in physical investigations are typically smooth enough that the higher order coefficients of an approximating polynomial may be assumed to be negligible over a small enough range of the experimentation. Accordingly, only the lower order coefficients need be estimated; however, they are allowed to be biased by (confounded with) coefficients of higher order interactions because such coefficients are assumed to be negligible.

Let the number of factors in the highest order interaction requiring estimation be e, and let the number of factors in the lowest order interaction with which it is allowed to be confounded be c; then the required resolution R of the design is defined (ref. 13) to be

$$R = e + c$$

As a minimum requirement on the first-order experiments, the coefficients will be allowed to be confounded with only the coefficients of two-factor or higher order interactions. This requires that $R = e + c = 1 + 2 = 3$. A somewhat improved design occurs if the first-order coefficients are estimated clear of two-factor interactions. This requires that $R = e + c = 1 + 3 = 4$.

For the interaction experiments, the estimates of two factor interaction coefficients should be allowed to be confounded only with higher order interaction coefficients. This requires that $R = e + c = 2 + 3 = 5$.

The design of the interaction experiment (of resolution 5) is now specified to be blocked into b blocks such that any one block will provide a design of resolution 3 for the first-degree model. As a consequence of this requirement, the experimenter may switch at any time from the method of steepest ascents to the method of local exploration by completing the $b - 1$ uncompleted blocks of the resolution 5 experiment.

Occasions could arise in which the experimenter would not wish to proceed immediately from a minimum-size first-order design to the design for estimating all interaction coefficients. For example, a design of only eight treatments hardly provides enough information to test the validity of the first-order model. The performance of a second block of eight treatments could lead to a much better decision. Also, the experimenter may have prior knowledge that certain interactions are negligible so that he can stop short of the experiment that estimates all two-factor interactions. For these reasons, the designs and parameter estimates are given for such intermediate size experiments.

Sizes of Experiments

The lower limit of the size of the interaction experiment has been set at 16 experimental units. The presumption is that experiments with less than 16 experimental units are too small for any determination of statistical validity. With 16 treatments the smallest number of factors in the (efficient) unreplicated experiment is four, and this will be the lower limit on the number of factors for which designs will be presented.

For the interaction experiment, the number of first-order and two-factor interaction coefficients needing estimation (aside from the grand mean) includes the g first-order coefficients and the $g(g - 1)/2$ two-factor interactions for a total of $g(g + 1)/2$. The number of treatments is 2^{g-h} and subtracting one degree of freedom for the grand mean, the number of degrees of freedom available for estimating these coefficients is $2^{g-h} - 1$. The ratio of the number of coefficients estimated to the available degrees of freedom has been defined by Daniel (ref. 5) as the efficiency of the design:

$$\text{Efficiency} = \frac{g(g + 1)}{2(2^{g-h} - 1)}$$

As given in reference 5, the efficiency of minimum size resolution 5 designs varies with the number of factors as follows:

Number of factors	Number of treatments	Efficiency	Number of factors	Number of treatments	Efficiency
5	16	1.00	11	128	0.52
6	32	.68	12	256	.31
7	64	.44	13	256	.36
8	64	.56	14	256	.41
9	128	.35	15	256	.47
10	128	.43			

The 2^{g-h} designs possess the desirable properties of rotatability and orthogonal estimates of first-order and two-factor interaction coefficients as discussed in reference 3. These properties can reasonably be insisted upon where the number of treatments is not excessive (64 or less) and where they are being effectively utilized. (The preceding table shows generally low efficiencies for 2^{g-h} designs of resolution 5 for nine or more factors). The use of more efficient designs for nine or more factors is highly desirable and the resulting sacrifice of rotatability and orthogonality might be tolerable because the number of treatments would still be quite large. Such designs are discussed in reference 14 and also in reference 15. The present investigation is limited to the 2^{g-h} designs of resolution 5, and as indicated by the preceding discussion, such designs are appropriate for situations involving up to eight factors. The largest number of treatments is therefore limited to 64.

The preceding limitation to experiments with 16 to 64 treatments does not count the star and the center points used in estimating the coefficients of the quadratic terms. The number of such experimental units depends on the criteria used to decide the number of center points, but with g factors, the number of these extra experimental units is relatively small, being only slightly in excess of $2g + 1$.

The fractional factorial first-order experiment on four factors requires a minimum of eight treatments, whereas the fractional factorial first-order experiment with eight factors requires a minimum of 16 treatments. Correspondingly, the sizes of the blocks are limited to 8 and 16 treatments.

The Principal Block

In some cases the experimenter will prefer to include a condition that he calls "standard conditions" in the first block of a blocked experiment. Typically, an experimenter would choose to have all independent variables at their low levels. In any event, the experimenter is free to invert scales so that the treatment he elects as standard conditions will contain all independent variables at naturally or at artificially defined "low" conditions. The designs to be presented have all been arranged such that the first block will contain the treatment with all factors at their low conditions. This block is called the principal block.

PROPERTIES OF RECOMMENDED DESIGNS

Tabular Presentations

In general, the estimation of all the coefficients of equation (6) is to be done by matrix inversion methods (ref. 16) or by a modified Doolittle method (ref. 17). Such computations are usually performed by a large programed machine or by a skilled operator using a desk calculator. On the other hand, the estimation of just the coefficients of the linear and interaction terms can be performed very simply by a procedure called Yates' algorithm (ref. 12, pp. 263-265). Its use permits the rapid evaluation of experiments performed in the method of steepest ascents and also for those experiments performed to evaluate interactions prior to the introduction of star points.

A necessary condition for using Yates' algorithm is that the observations of the fractional hypercube experiment must be written in Yates' order. A special notation is of assistance in establishing Yates' order. The notation is defined in terms of treatment combinations. The independent variables are named A, B, C, D, E, F, and so forth. For two levels of such a variable the symbol 1 is used for the lower level, and the associated lower case letter is used for the upper level. A particular treatment is then represented by the product of these symbols; for example, the treatment A, lower; B, upper; C, upper; D, lower; E, lower; F, upper; would be written

$$1bc11f = bcf$$

The standardized variables assign a +1 to a variable at the upper level and a -1 to a variable at the lower level, and the treatment would be specified by such coordinates. The preceding example with x_1 associated with A, x_2 with B, and so forth, is

$$(x_1, x_2, x_3, x_4, x_5, x_6) = (-1, 1, 1, -1, -1, 1) = bcf$$

With observations identified by the treaments that produced them, the Yates' order for observations can be indicated by stating their order using the Yates' notation for treatments as in the first column of table 1. The tabular presentations of the designs will give the treatments in Yates' order.

Application of Yates' algorithm to the 2^{g-h} observations produces 2^{g-h} numbers called contrasts, and division of the contrasts by 2^{g-h} produces 2^{g-h} estimates of the parameters of equation (5). Just which parameters are estimated depends on the details of the design. The techniques used to establish the details of the designs and to identify the parameters estimated are given in the appendix. The details of the design cause some of the parameters to be aliased with each other; that is, some of the parameters cannot be separately estimated but only a linear combination of them is equal to a contrast. However, with respect to all the parameters of equation (5), only the single-factor and two-factor interaction coefficients are of direct interest.

In the case of the first-order experiments, if a two-factor interaction coefficient is aliased with a single-factor coefficient (if the sum of a two-factor coefficient and a single-factor coefficient is estimated by a single contrast), then the two-factor coefficient is assumed to be zero. If a contrast does not estimate any combination of two-factor or lower order coefficients, the contrast will be given a name by listing the lowest order set of interaction coefficients that it does estimate. For example, table 17 lists a treatment bcde, and the Yates' computation would give an estimator of β_{234} in the same row. From table 15 the full set of aliased parameters can be shown to be β_{234}, $-\beta_{1245}$, β_{147}, β_{126}, $-\beta_{3457}$, $-\beta_{2356}$, β_{367}, and $-\beta_{1567}$ of which the lowest order set is β_{234}, $+\beta_{147}$, $+\beta_{126}$, $+\beta_{367}$. Those parameters, the estimates of which are confounded with block effects, will be identified by attaching an asterisk to the parameters.

The designs are identified by code numbers. For example, Plan 1/8; 7f, 8t/b; 2b means that the design is a 1/8 replicate of a full factorial experiment with 7 factors, employing 8 treatments per block, and using 2 blocks. The order of presentation of the designs (tables 2 to 29) is the order of increasing numbers of factors. For a given number of factors, a sequence of designs with blocks of 8 treatments is presented first, followed by a sequence of designs with blocks of 16 treatments. Within any sequence, the order is the order of increasing numbers of blocks.

Use of Resolution 4 Designs in Fitting First-Order Model

In general, the use of the first-order model as a prediction equation, with coefficients estimated from an experiment, requires the assumption that all second-order parameters are zero. However, circumstances might arise where the experimenter desired an approximate first-order predicting equation and ignored the existence of possible nonzero two-factor interactions. He might then prefer a resolution 4 design to a resolution 3 design because the estimates of the first-order coefficients would not then be confounded with (biased by) two-factor interactions.

Minimum-size designs of resolution 4 are shown for 4 factors by table 2, for 5 factors by table 5, and for 6 factors by table 10. Minimum-size designs of resolution 4 for 7 and 8 factors were given by Natrella (ref. 18, p. 12-18), and these designs are also given in tables 28 and 29. Unfortunately, no success was achieved in trying to include the designs of tables 28 and 29 in the telescoping sequences of 7- and 8-factor blocked designs, that is, tables 21 to 27. However, the designs of tables 28 and 29 might therefore be used for the very first trial of a Box-Wilson procedure, when the experimenter believed that he would be so far from an optimum condition that a first-order model would be a good enough approximation. After such a trial he could move to a new design center and then elect a design capable of being sequentially expanded by blocks into designs of higher order, that is, tables 21 or 25.

Conditions for Using Resolution 3 and Resolution 4 Designs in Estimating the Second-Order Model

If the experimenter has prior knowledge that some of the two-factor interactions are zero, he may be able to choose the labels for his factors so that the nonzero interaction parameters can be estimated from designs of less than resolution 5. The specific cases are listed:

Table 2. - Plan 1/2; 4f; 8t/b; 1b. - If one of the factors (for example X_1) does not interact with the other factors, then all the remaining interactions are estimable (table 2). If X_1 is noninteracting, the estimated parameters are β_0, β_1, β_2, β_{34}, β_3, β_{24}, β_{23}, and β_4.

Table 5. - Plan 1/2; 5f; 8t/b; 2b. - The factor believed most likely to interact with other factors should be labeled X_4 because the plan (table 5) gives unconfounded estimates of β_{14}, β_{24}, β_{34}, and β_{45}. If any one of X_1, X_2, X_3, or X_5 does not interact (for example, X_1) then all the remaining two-factor interactions are estimable and the estimated parameters are β_0, β_1, β_2, β_{35}, β_3, β_{25}, β_{23}, β_5, β_4, β_{14}, β_{24}, β_{345}, β_{34}, β_{245}, $(\beta^*_{234} + \beta^*_{145})$, and β_{45}. Under previously stated assumptions, the estimates of β_{14}, β_{345}, and β_{245} are assumed to be nothing more than random error.

Table 10. - Plan 1/4; 6f; 8t/b; 2b. - If x_1 does not interact with any other factor, and if x_2 does not interact with x_4, x_5, and x_6, then the parameters estimated are as follows: β_0, β_1, β_2, β_{36}, β_3, β_{45}, β_{23}, β_6, β_4, β_{35}, β_{56}, $(\beta^*_{124} + \beta^*_{156} + \beta^*_{235} + \beta^*_{346})$, β_{34}, β_5, β_{46}, and the estimate of $(\beta_{125} + \beta_{146} + \beta_{234} + \beta_{356})$ is assumed to be random error (table 10).

Table 11. - Plan 1/2; 6f; 8t/b; 4b. - If the label X_1 had been given to the most likely noninteracting factor in the design of table 10, the performance of the two augmenting blocks of table 11 would result in a design with all interactions estimable under the minimal assumptions that β_{12}, β_{13}, and β_{16} are zero.

Table 13. - Plan 1/4; 6f; 16t/b; 1b. - Assume that there are two groups of three factors and that each factor does not interact within its group. Give the factors within one group the labels X_1, X_2, and X_6 and label the factors of the other group X_3, X_4, and X_5. Then all the nonzero two-factor interaction coefficients (one factor from each group) are estimable and are β_{13}, β_{14}, β_{15}, β_{23}, β_{24}, β_{25}, β_{36}, β_{46}, and β_{56} (table 13).

Table 18. - Plan 1/4; 7f; 8t/b; 4b. - This plan (table 18) becomes a suitable second-order design under the assumptions that X_1 does not interact with X_3, X_4, or X_6, and that X_2, X_5, and X_7 do not interact with each other.

Table 21. - Plan 1/8; 7f; 16t/b; 1b. - This plan (table 21) estimates two-factor interactions if X_7 is noninteracting with X_1, X_2, X_3, X_4, and X_6, if X_5 is noninteracting with X_1, X_2, X_4, and X_6, if X_1 is noninteracting with X_2, X_4, and X_6, and if X_2 is noninteracting with X_6.

Table 22. - Plan 1/4; 7f; 16t/b; 2b. - This plan (table 22) estimates all two-factor interactions if any one of X_1, X_2, X_4, or X_6 does not interact with the other factors of this group.

Table 26. - Plan 1/8; 8f; 16t/b; 2b. - This plan (table 26) estimates all interactions if X_8 is noninteracting with X_1, X_2, X_3, X_5, and X_7, and if X_3 is noninteracting with X_1, X_2, X_4, and X_6. Thus the label X_8 should be given to the least interacting variable and the label X_3 should be given to the next least interacting variable.

Choice of Block Size

The present investigation assumes that the experimenter will wish to perform a block of treatments, analyze the data, and then perform another block of treatments, and that the block effects arise during the interuption of the experimenting for analyzing data (furnaces are overhauled, instruments are newly calibrated, etc.). Under these assumptions, block sizes 8 and 16 are particularly appropriate for experiments on 4 to 8 factors. On the other hand, the physical situation could limit the experimenter to smaller block sizes. Under such limitations, other designs would have to be synthesized, and the synthesis could be done according to rules presented in reference 12.

Another reason for using small block sizes is to protect against the hazard of missing values. If through accident, the observations from one or more treatments are missing from a block, the whole block could be rerun, especially if it is small. On the other hand, only the missing treatments need be run, if the experimenter can say that no block effect will arise between the new runs and the block from which observations are missing. If the design is not severely fractionated (if the number of treatments is significantly larger than the number of parameters estimated), methods of estimating for missing values may be used (ref. 12 or 19).

Some attributes of the proposed designs are summarized in table 30. In the case of 4 factors, all coefficients are estimable from two blocks of size 8 and a single block of size 16 is of no advantage in estimating the parameters of a second-order model. In the case of 7 factors, the attainment of a resolution 5 design requires 64 treatments for either blocks of size 8 or size 16, so that there is no clear advantage in using blocks of size 16. With 8 factors, the minimum first-order design requires 16 treatments, and this is the only block size presented for the problem with 8 factors. In the cases of 5 and 6 factors, the choice of a block size of 8 or 16 is particularly complex.

A comparison of the number of experimental units required in experimenting with block sizes of 8 and 16 for 5 and 6 factors is given in table 31. The column headed "Total number of units required" shows that for five factors, the break-even point for the two block sizes occurs at three repetitions of the first-order experiments. For six factors, the break-even point occurs for five repetitions of the first-order experiments. In other words, if the experimenter believes that he will perform many cycles of experimenting with the method of steepest ascents, he should use a block size of 8 because it uses a relatively smaller number of experimental units. On the other hand, the block of size 16 uses a relatively smaller number of experimental units in the method of local exploration. The block size of 16 should be used if the experimenter believes he will spend relatively few cycles of experiments with the method of steepest ascents, less than three cycles with 5 factors or less than five cycles with 6 factors.

Maximum economy could be sought with a mixed strategy. The experimenter could use the block of size 8 until his intuition told him that the first-order model might not be appropriate. He could then switch to the block of size 16. Its greater number of degrees of freedom for "lack of fit" would provide better information about the validity of the first-order model, and on switching to the method of local exploration, fewer experimental units would be needed to complete the interaction model than if the smaller block had been used. Thus with five factors, one or two experiments of the method of steepest ascents should be performed with the small block size followed by a switch to the larger block. With six factors, the break-even point is not reached until the fifth design center. Furthermore, two blocks of size 8 (table 10) provide a resolution 4 design, whereas the single block of size 16 (table 13) is only a resolution 3 design. With six factors, the best strategy might consist of using blocks of size 8 (table 9) until interactions were suspected, at which point the design could be enlarged to that of table 10. If no new design center were desired, the design could then be augmented to that of table 11. If the design of table 10 had not shown significant interactions, experimenting at a new design center could continue with the design of table 9, but if significant interactions had been shown, the new experimenting should begin with the design of table 13.

CONCLUDING REMARKS

The possibility of using rationally designed experiments with optimal statistical properties was considered in terms of alloy development. The Box-Wilson methods and Box-Hunter designs of experiments are believed to be appropriate to the problem of finding optimum conditions. Within these concepts, the appropriate designs for the estimation of the coefficients of first-order terms in the method of steepest ascents and for the estimation of the coefficients of the two-factor interactions in the method of local exploration consist of blocked fractional two-level factorial designs. Within these considerations the following concepts were demonstrated:

1. The total experiment needed to estimate two-factor interactions can be a two-level fractional factorial blocked design that is minimally adequate to the purpose.

2. The specific detailed design of the blocks can be such that just one of them is minimally adequate for estimating the coefficients of the first-order model.

3. The blocks can be arranged in a "telescoping" sequence such that observations from the first block are retained and combined with observations from new blocks so that all acquired observations are used cumulatively to estimate models of successively greater generality.

4. The designs presented are appropriate to these criteria for situations involving four to eight independent variables. Lesser or greater numbers of variables might require that experimental strategies be basically different from those of the present investigation.

Lewis Research Center,
National Aeronautics and Space Administration,
Cleveland, Ohio, March 31, 1967,
129-03-01-03-22.

APPENDIX - CONSTRUCTION OF DESIGNS AND IDENTIFICATION OF PARAMETERS ESTIMATED BY YATES' CONTRASTS

Contrast Vectors

The construction of a design begins with a listing of the treatments of a full 2^3 design for a block size of 8, or with a listing of the treatments of a full 2^4 design for a block size of 16. In either case, these treatments are listed in Yates' standard order (ref. 12). The standard order for a two-level factorial experiment on factors A, B, C, . . . is written in terms of the symbols for treatments, (1), a, abc, and so forth. The standard order is determined by writing treatment symbols in the order, (1), a, b, ab. The treatments for which C is at the high level are then ordered by multiplying the preceding symbols by c and adding on the new list. The total list becomes (1), a, b, ab, c, ac, bc, abc. The treatments for which D is at the high level are ordered by mutiplying the preceding list by d and appending the new symbols to the old list: (1), a, b, ab, c, ac, bc, abc, d, ad, bd, abd, cd, acd, bcd, abcd.

The design and results of an experiment can be exhibited by an array such as table 1. The first column presents the treatment in Yates' notation and order. The second column stands for the observed responses that correspond to treatments in the same row of the table. The third column presents a dummy variable that takes the value one. The array consisting of those columns headed by X_1, X_2, X_3, and X_4 is called the design matrix. It gives the same information as is given by the column headed "Treatments".

The array beginning with the column headed X_0 and including all columns to the right is called the matrix of independent variables. It gives the levels of those variables in design units and is therefore derivable from the treatment column as given in Yates' notation.

As listed in table 1, the columns under X_0, X_1, X_1X_2, and so forth, can be regarded as column vectors. A column headed by X_1X_2 is the result of multiplying elements together from like rows of X_1 and X_2 (This rule of multiplying X_1 by X_2 to generate the column vector X_1X_2 differs from the definitions of scalar product and vector product in conventional vector analysis.)

Inspection of table 1 shows that when any column X_j is multiplied by itself, the result is $X_j^2 = X_0$. This result means that far more complicated multiplications can lead to simple results; for example,

$$(X_1X_2X_3)(X_1X_3X_4) = X_1^2X_2X_3^2X_4 = X_0X_2X_0X_4 = X_2X_4$$

The preceding rule for the multiplication of the column vectors is used in constructing the detailed designs.

The column vectors give the linear combination of observations (provided that they are in Yates' order) that estimates the coefficient indicated by each column heading. Obviously, the grand mean is estimated by multiplying the observations by the quantities under X_0 and summing and dividing by 2^l, where in table 1, $l = 4$.

If the observations are multiplied by the quantities under X_1, their sum divided by 2^{l-1} represents the average change in response between the upper and the lower levels of X_1. Dividing the sum by 2^l gives the change in response for a unit change in X_1, and this quotient is the estimator b_1 of the coefficient β_1 of equation (5).

In a similar manner, multiplying the responses by the quantities of any column of table 1 and dividing the sum by 2^l results in an estimate of the coefficient of the term in equation (5) that is identified by the column heading. This work is done automatically by Yates' algorithm with results presented in the order of the column headings of table 1.

With A associated with X_1, B associated with X_2, and so forth, the sums resulting from the Yates' computation will be called A, B, ABC, and so forth. The sum associated with column X_0 with be called T; with X_1, A; with X_2, B; with X_1X_2, AB; with $X_2X_3X_4$, BCD; and so forth. Thus BCD is the dot or scalar product of the two vectors Y and $X_2X_3X_4$. These sums (such as BCD) are also called contrasts. Performance of the full 2^4 experiment and computation according to Yates' algorithm thus furnishes estimates of coefficients of equation (5) as follows:

$$b_0 = (1/2^l)T$$

$$b_1 = (1/2^l)A$$

$$b_2 = (1/2^l)B$$

$$\vdots \qquad \vdots$$

$$b_{234} = (1/2^l)BCD, \text{ and so forth}$$

5 Factors - Blocks of 8 Treatments

If the experiment is to consist of a one-fourth replicate, then X_4 and X_5 can be assigned only combinations of levels that constitute a three-factor full factorial experiment, namely, the levels of X_1, X_2, X_3 and their interactions in table 1. Because all first-order coefficients must be estimated, X_4 and X_5 should be set equal only to two-factor or high order combinations. Such combinations may be chosen arbitrarily, except that each member of the resulting full set of defining contrasts must have a number of factors equal to or greater than the desired resolution level R. Set $X_4 = -X_2X_3$ and $X_5 = X_1X_2X_3$. Then the full set of defining contrasts is

$$X_0 = X_4^2 = -X_2X_3X_4$$

$$X_0 = X_5^2 = X_1X_2X_3X_5$$

$$X_0 = X_4^2X_5^2 = -X_1X_4X_5$$

Negative signs are attached to all defining contrasts containing an odd number of treatments and positive signs are attached to all defining contrasts containing an even number of treatments. This convention ensures that the first block will be the principal block, that is, will contain the treatment with all factors at their low level.

The properties of the design have now been fixed by the establishment of the four identical contrast vectors, namely,

$$X_0 = -X_2X_3X_4 = X_1X_2X_3X_5 = -X_1X_4X_5$$

The full set of contrast vectors that thus fix the design are called <u>defining contrasts</u>. They show what parameters are confounded. For example, in table 4, the parameters that are obviously estimated by the Yates' contrasts (on division by the number of treatments) are β_0, β_1, β_2, β_{12}, β_3, β_{13}, β_{23}, and β_{123}. Multiplying the contrasts that provide estimates of these coefficients by the full set of defining contrasts and neglecting all interactions of order higher than two factor shows that the confounded sets of parameters are as given in the following table:

Product with defining contrasts	Confounded coefficients
$(X_1)(-X_1X_4X_5) = -X_4X_5$	$\beta_1 - \beta_{45}$,
$(X_2)(-X_2X_3X_4) = -X_3X_4$	$\beta_2 - \beta_{34}$
$(X_1X_2)(X_1X_2X_3X_5) = X_3X_5$	$\beta_{12} + \beta_{35}$
$(X_3)(-X_2X_3X_4) = -X_2X_4$	$\beta_3 - \beta_{24}$
$(X_1X_3)(X_1X_2X_3X_5) = X_2X_5$	$\beta_{13} + \beta_{25}$
$(X_2X_3)(X_1X_2X_3X_5) = X_1X_5$	$\beta_{23} + \beta_{15} - \beta_4$
$(X_2X_3)(-X_2X_3X_4) = -X_4$	
$(X_1X_2X_3)(-X_2X_3X_4) = -X_1X_4$	$-\beta_{14} + \beta_5$
$(X_1X_2X_3)(X_1X_2X_3X_5) = X_5$	

Although there are 2^h defining contrasts in the full set, only h are independent, that is, starting with any h defining contrasts that are independent, the full set can be generated by multiplying the independent ones in all combinations. For example, if $-X_2X_3X_4$ and $X_1X_2X_3X_5$ are taken as the h = 2 independent defining contrasts, then the $2^h = 4$ are obtained by annexing the dependent contrasts:

$$(-X_2X_3X_4)(-X_2X_3X_4) = X_0$$

and

$$(-X_2X_3X_4)(X_1X_2X_3X_5) = -X_1X_4X_5$$

Under the specifications $X_4 = -X_2X_3$ and $X_5 = X_1X_2X_3$, the levels of X_4 and X_5 to be attached to the Yates' ordered treatments of X_1, X_2, and X_3 can be obtained from table 1. The signs under X_2X_3 are to be reversed, and they then show that X_4 in Yates' order should take on the levels 1, 1, d, d, d, d, 1, 1. The signs under $X_1X_2X_3$ of table 1 are left unchanged, and the levels of X_5 are 1, e, e, 1, e, 1, 1, e. These treatment levels are then affixed to the Yates' levels for X_1, X_2, and X_3, to obtain the treatments 1, ae, bde, abd, cde, acd, bc, and abce as listed in table 4.

In other words, Yates' computation is performed as if the 2^{g-h} experiment were only a 2^l experiment with h factors ignored. The independent defining contrasts were formed by setting the ignored factors, one at a time, equal to some interactions among the factors not ignored. This procedure fulfills a rule announced by Daniel in reference 5, namely, "The ignored letters must be ones occurring in only one alias subgroup generator [independent defining contrast]."

If the experiment is to consist of a one-half replicate in two blocks, set $X_5 = X_1X_2X_3$. Then $X_0 = X_5^2 = X_1X_2X_3X_5$. The design and estimated effects are given as Plan 1/2; 5f; 8t/b; 2b (table 5). The defining contrasts of the single block were $-X_1X_4X_5$, $-X_2X_3X_4$, and $X_1X_2X_3X_5$, whereas the defining contrast for the half replicate is $X_1X_2X_3X_5$; therefore, $-X_1X_4X_5$ and $(-X_1X_4X_5)(X_1X_2X_3X_5) = -X_2X_3X_4$ represent block effects. Thus in Plan 1/2; 5f; 8t/b; 2b, the contrast that estimates $\beta_{145} + \beta_{234}$ also estimates the block effect.

The full interaction experiment is given as Plan 1; 5f; 8t/b; 4b (table 6). Because it is a blocked design using blocks that were partitioned according to the fractional replicate contrasts of Plan 1/4; 5f; 8t/b; 1b, the parameter estimates confounded with block effects are $-\beta_{234}$, $-\beta_{145}$, and β_{1235}.

5 Factors - Blocks of 16 Treatments

In table 1 consider a 2^4 experiment on X_1, X_2, X_3, and X_4 and set

$$X_5 = -X_1X_2X_3X_4$$

Therefore

$$X_0 = -X_1X_2X_3X_4X_5$$

The design is given as Plan 1/2; 5f; 16t/b; 1b (table 7). The interaction model contains 16 parameters, and these parameters can be estimated from the 16 responses of the design, if it is performed in a single block.

6 Factors - Blocks of 8 Treatments

The first-order model contains 7 parameters and the minimum fractional factorial design contains 8 treatments. Consider the first 8 treatments of table 1 corresponding to a 2^3 experiment on X_1, X_2, and X_3. Let $X_4 = -X_1X_2$, $X_5 = -X_2X_3$ and $X_6 = +X_1X_2X_3$. Then the the full set of defining contrasts is as given for the 1/8 replicate in table 8. These defining contrasts lead to the treatments and estimated effects given as Plan 1/8; 6f; 8t/b; 1b of table 9.

The sequence of defining contrasts used to obtain the next two larger of the telescoping fractional factorial designs is given in table 8. The corresponding treatments and estimated parameters are given in tables 10 and 11. The interactions confounded with blocks in the full 2^6 experiment (table 12) are the same as the defining contrasts used to to construct the first block, namely, $-X_1X_2X_4$, $-X_2X_3X_5$, $X_1X_2X_3X_6$, $X_1X_3X_4X_5$, $-X_3X_4X_6$, $-X_1X_5X_6$, and $X_2X_4X_5X_6$. The coefficients confounded with blocks are therefore the corresponding coefficients (with asterisks) in table 12.

6 Factors - Blocks of 16 Treatments

Assume that the experiment is performed on one block of 16 treatments. With reference to table 1 for an experiment on X_1, X_2, X_3, and X_4, let $X_5 = -X_3X_4$, and $X_6 = -X_1X_2$. Then

$$X_0 = X_5^2 = -X_3X_4X_5$$

$$X_0 = X_6^2 = -X_1X_2X_6$$

$$X_0 = X_5^2X_6^2 = X_1X_2X_3X_4X_5X_6$$

The required treatments are given by Plan 1/4; 6f; 16t/b; 1b (table 13), and the parameters estimated on dividing Yates' contrasts by 2^4 are also listed in table 13.

Performance of a second block of 16 treatments according to Plan 1/2; 6f; 16t/b; 2b (table 14) results in 32 Yates' contrasts, which on dividing by 2^5 result in estimates of the the parameters listed in table 14.

In table 14, the defining contrast of the fractional replicate is $X_1X_2X_3X_4X_5X_6$. In table 13, the defining contrasts were $-X_1X_2X_6$, $-X_3X_4X_5$, and their product $X_1X_2X_3X_4X_5X_6$. Therefore, in table 14 where the only defining contrast is $X_1X_2X_3X_4X_5X_6$, the interactions $-X_1X_2X_6$ and $-X_3X_4X_5$ are confounded with the block effect and aliased with each other, so that the estimator of the block effect is $(\beta^*_{345} + \beta^*_{126})$.

7 Factors - Blocks of 8 Treatments

The first-order model contains 8 parameters and the minimum fractional factorial design therefore contains 8 treatments. Consider the first 8 treatments of table 1 as an experiment on X_1, X_2, and X_3. Let $X_4 = -X_1X_2$, $X_5 = -X_1X_3$, $X_6 = -X_2X_3$, and $X_7 = X_1X_2X_3$. The corresponding complete set of defining contrasts for the 1/16 replicate is listed in table 15. The defining contrasts for a telescoping sequence of designs consisting of the 1/16, 1/8, 1/4, and 1/2 replicates are also given in table 15. The corresponding designs and estimated parameters are given in tables 16 to 19.

7 Factors - Blocks of 16 Treatments

Let $X_5 = -X_1X_4$, $X_6 = X_1X_2X_4$, $X_7 = X_2X_3X_4$; then the complete set of defining contrasts are as listed for the 1/8 replicate of table 20. The defining contrasts for telescoping designs consisting of 1/4 and 1/2 replicates are also given in table 20. The associated designs and estimated parameters are given in tables 21 to 23.

8 Factors - Blocks of 16 Treatments

The first-order model contains nine parameters and the minimum fractional factorial design therefore contains 16 treatments. Let $X_5 = -X_1X_4$, $X_6 = X_1X_3X_4$, $X_7 = X_2X_3X_4$, $X_8 = -X_2X_3$; then the complete set of defining contrasts for the 1/16 replicate is given by table 24. The complete sets of defining contrasts for the telescoping designs consisting if 1/8 and 1/4 replicates are also given in table 24. The associated designs and estimated parameters are given by tables 25 to 27.

REFERENCES

1. Box, G. E. P.; and Wilson, K. B.: On The Experimental Attainment of Optimum Conditions. J. Roy. Statist. Soc., Ser. B., vol. 13, 1951, pp. 1-45.

2. Brickner, K. G.; and Geissler, J. J.: The Application of Computers and Statistically Designed Experiments to Obtain Models Used in Alloy Development. Paper presented at the ASM Metals Materials Congress, Philadelphia, Oct. 19-23, 1964. (ASM T.R. No. P14-2-64).

3. Box, G. E. P.; and Hunter, J. S.: Multi-Factor Experimental Designs for Exploring Response Surfaces. Ann. Math. Statist., vol. 28, 1957, pp. 195-241.

4. N. B. S. Statistical Eng. Lab.: Fractional Factorial Experiment Designs for Factors at Two Levels. Applied Mathematics Series, 48. NBS, 1957. (Reprinted 1962).

5. Daniel, Cuthbert: Fractional Replication in Industrial Research. Proceedings of the Third Berkeley Symposium on Mathematical Statistics and Probability. Vol. 5, University of California Press, 1956, pp. 87-98.

6. Daniel, Cuthbert: Use of Half-Normal Plots in Interpreting Factorial Two-Level Experiments. Technometrics, vol. 1, no. 4, Nov. 1959, pp. 311-341.

7. Birnbaum, Allen: On the Analysis of Factorial Experiments Without Replication. Technometrics, vol. 1, no. 4, Nov. 1959, pp. 343-357.

8. Wilk, M. B.; Gnanadesikan, R.; and Freeny, Anne E.: Estimation of Error Variance from Smallest Ordered Contrasts. J. Am. Statist. Assoc., vol. 58, 1963, pp. 152-160.

9. Holms, Arthur G.: Multiple-Decision Procedures for the ANOVA of Two-Level Factorial Replication-Free Experiments. PhD Thesis, Western Reserve University, June 1966. (Available from University Microfilms, Inc., Ann Arbor, Mich.)

10. Box, G. E. P.; and Hunter, J. S.: Experimental Designs for the Exploration and Explotation of Response Surfaces. Experimental Designs in Industry. Victor Chew, ed., John Wiley and Sons, Inc., 1958, pp. 138-190.

11. Hill, William J.; and Hunter, William G.: A Review of Response Surface Methodology: A Literature Survey. Technometrics, vol. 8, no. 4, Nov. 1966, pp. 571-590.

12. Davies, Owen L., ed.: The Design and Analysis of Industrial Experiments. Second ed., Hafner Publishing Co., 1960.

13. Box, G. E. P.; and Hunter, J. S.: The 2^{k-p} Fractional Factorial Designs. I. Technometrics, vol. 3, no. 3, Aug. 1961, pp. 311-351.

14. Whitwell, John C.; and Morbey, Graham K.: Reduced Designs of Resolution Five. Technometrics, vol. 3, no. 4, Nov. 1961, pp. 459-477.

15. Addelman, Sidney: Irregular Fractions of the 2^n Factorial Experiments. Technometrics, vol. 3, no. 4, Nov. 1961, pp. 479-496.

16. Faddeeva, V. N. (Curtis D. Benster, trans.): Computational Methods of Linear Algebra. Dover Publications, Inc., 1959.

17. Ezekiel, Mordecai; and Fox, Karl A.: Methods of Correlation and Regression Analysis. Third ed., John Wiley & Sons, Inc., 1959.

18. Natrella, Mary G.: Experimental Statistics. Handbook 91, National Bureau of Standards, Aug. 1, 1963.

19. Draper, Norman R.: Missing Values in Response Surface Designs. Technometrics, vol. 3, 1961, pp. 389-398.

TABLE 1. - FULL 2^4 EXPERIMENT

Treat-ment	Response	Matrix of independent variables															
		X_0	X_1	X_2	X_1X_2	X_3	X_1X_3	X_2X_3	$X_1X_2X_3$	X_4	X_1X_4	X_2X_4	$X_1X_2X_4$	X_3X_4	$X_1X_3X_4$	$X_2X_3X_4$	$X_1X_2X_3X_4$
(1)	y_1	+1	-1	-1	+1	-1	+1	+1	-1	-1	+1	+1	-1	+1	-1	-1	+1
a	y_2	+1	+1	-1	-1	-1	-1	+1	+1	-1	-1	+1	+1	+1	+1	-1	-1
b	y_3	+1	-1	+1	-1	-1	+1	-1	+1	-1	+1	-1	+1	+1	-1	+1	-1
ab	y_4	+1	+1	+1	+1	-1	-1	-1	-1	-1	-1	-1	-1	+1	+1	+1	+1
c	y_5	+1	-1	-1	+1	+1	-1	-1	+1	-1	+1	+1	-1	-1	+1	+1	-1
ac	y_6	+1	+1	-1	-1	+1	+1	-1	-1	-1	-1	+1	+1	-1	-1	+1	+1
bc	y_7	+1	-1	+1	-1	+1	-1	+1	-1	-1	+1	-1	+1	-1	+1	-1	+1
abc	y_8	+1	+1	+1	+1	+1	+1	+1	+1	-1	-1	-1	-1	-1	-1	-1	-1
d	y_9	+1	-1	-1	+1	-1	+1	+1	-1	+1	-1	-1	+1	-1	+1	+1	-1
ad	y_{10}	+1	+1	-1	-1	-1	-1	+1	+1	+1	+1	-1	-1	-1	-1	+1	+1
bd	y_{11}	+1	-1	+1	-1	-1	+1	-1	+1	+1	-1	+1	-1	-1	+1	-1	+1
abd	y_{12}	+1	+1	+1	+1	-1	-1	-1	-1	+1	+1	+1	+1	-1	-1	-1	-1
cd	y_{13}	+1	-1	-1	+1	+1	-1	-1	+1	+1	-1	-1	+1	+1	-1	-1	+1
acd	y_{14}	+1	+1	-1	-1	+1	+1	-1	-1	+1	+1	-1	-1	+1	+1	-1	-1
bcd	y_{15}	+1	-1	+1	-1	+1	-1	+1	-1	+1	-1	+1	-1	+1	-1	+1	-1
abcd	y_{16}	+1	+1	+1	+1	+1	+1	+1	+1	+1	+1	+1	+1	+1	+1	+1	+1

Table 2.[a] - PLAN 1/2; 4f; 8t/b; 1b -

R = 4

$[X_0 = X_1X_2X_3X_4.]$

Block	Treatment	Estimated effects
1	(1)	β_0
1	ad	β_1
1	bd	β_2
1	ab	$\beta_{12} + \beta_{34}$
1	cd	β_3
1	ac	$\beta_{13} + \beta_{24}$
1	bc	$\beta_{23} + \beta_{14}$
1	abcd	β_4

[a]Refs. 12 (p. 484) and 18 (p. 12-16).

TABLE 3.[a] - PLAN 1; 4f; 8t/b; 2b -

R = 5

[Block confounding, $X_1X_2X_3X_4$.]

Block	Treatment	Estimated effects (b)
1	(1)	β_0
2	a	β_1
2	b	β_2
1	ab	β_{12}
2	c	β_3
1	ac	β_{13}
1	bc	β_{23}
2	abc	β_{123}
2	d	β_4
1	ad	β_{14}
1	bd	β_{24}
2	abd	β_{124}
1	cd	β_{34}
2	acd	β_{134}
2	bcd	β_{234}
1	abcd	β_{1234}^*

[a]Refs. 12 (p. 429) and 18 (p. 12-10).
[b]Asterisk denotes confounding with blocks.

TABLE 4. - PLAN 1/4; 5f; 8t/b; 1b -

R = 3

[$X_0 = -X_2X_3X_4 = X_1X_2X_3X_5 = -X_1X_4X_5$.]

Block	Treatment	Estimated effects
1	(1)	β_0
1	ae	$\beta_1 - \beta_{45}$
1	bde	$\beta_2 - \beta_{34}$
1	abd	$\beta_{12} + \beta_{35}$
1	cde	$\beta_3 - \beta_{24}$
1	acd	$\beta_{13} + \beta_{25}$
1	bc	$-\beta_4 + \beta_{23} + \beta_{15}$
1	abce	$\beta_5 - \beta_{14}$

TABLE 5. - PLAN 1/2; 5f; 8t/b; 2b -

R = 4

[$X_0 = X_1X_2X_3X_5$; block confounding, $-X_2X_3X_4$.]

Block	Treatment	Estimated effects (a)
1	(1)	β_0
1	ae	β_1
2	be	β_2
2	ab	$\beta_{12} + \beta_{35}$
2	ce	β_3
2	ac	$\beta_{13} + \beta_{25}$
1	bc	$\beta_{23} + \beta_{15}$
1	abce	β_5
2	d	β_4
2	ade	β_{14}
1	bde	β_{24}
1	abd	$\beta_{124} + \beta_{345}$
1	cde	β_{34}
1	acd	$\beta_{134} + \beta_{245}$
2	bcd	$\beta^*_{234} + \beta^*_{145}$
2	abcde	β_{45}

[a]Asterisk denotes confounding with blocks.

TABLE 6. - PLAN 1; 5f; 8t/b; 4b

[Block confounding, $-X_2X_3X_4$, $-X_1X_4X_5$, $X_1X_2X_3X_5$.]

Block	Treatment	Estimated effects (a)	Block	Treatment	Estimated effects (a)
1	(1)	β_0	4	e	β_5
4	a	β_1	1	ae	β_{15}
3	b	β_2	2	be	β_{25}
2	ab	β_{12}	3	abe	β_{125}
3	c	β_3	2	ce	β_{35}
2	ac	β_{13}	3	ace	β_{135}
1	bc	β_{23}	4	bce	β_{235}
4	abc	β_{123}	1	abce	β^*_{1235}
2	d	β_4	3	de	β_{45}
3	ad	β_{14}	2	ade	β^*_{145}
4	bd	β_{24}	1	bde	β_{245}
1	abd	β_{124}	4	abde	β_{1245}
4	cd	β_{34}	1	cde	β_{345}
1	acd	β_{134}	4	acde	β_{1345}
2	bcd	β^*_{234}	3	bcde	β_{2345}
3	abcd	β_{1234}	2	abcde	β_{12345}

[a]Asterisk denotes confounding with blocks.

TABLE 7.[a] - PLAN 1/2; 5f; 16t/b; 1b - R = 5

[$X_0 = -X_1X_2X_3X_4X_5$.]

Block	Treatment	Estimated effects
1	(1)	β_0
1	ae	β_1
1	be	β_2
1	ab	β_{12}
1	ce	β_3
1	ac	β_{13}
1	bc	β_{23}
1	abce	$-\beta_{45}$
1	de	β_4
1	ad	β_{14}
1	bd	β_{24}
1	abde	$-\beta_{35}$
1	cd	β_{34}
1	acde	$-\beta_{25}$
1	bcde	$-\beta_{25}$
1	abcd	$-\beta_5$

[a]Refs. 12 (p. 485) and 18 (p. 12-16).

TABLE 8. - DEFINING CONTRASTS, 6 FACTORS ON BLOCKS OF 8 TREATMENTS

Source	Defining contrasts		
	1/8 Replicate	1/4 Replicate	1/2 Replicate
X_4^2	$-X_1X_2X_4$		
X_5^2	$-X_2X_3X_5$		
X_6^2	$X_1X_2X_3X_6$	$X_1X_2X_3X_6$	$X_1X_2X_3X_6$
$X_4^2X_5^2$	$X_1X_3X_4X_5$	$X_1X_3X_4X_5$	
$X_4^2X_6^2$	$-X_3X_4X_6$		
$X_5^2X_6^2$	$-X_1X_5X_6$		
$X_4^2X_5^2X_6^2$	$X_2X_4X_5X_6$	$X_2X_4X_5X_6$	

TABLE 9. - PLAN 1/8; 6f; 8t/b; 1b - R = 3

$$[X_0 = -X_1X_2X_4 = -X_2X_3X_5 = X_1X_2X_3X_6 = X_1X_3X_4X_5 = -X_3X_4X_6 = -X_1X_5X_6 = X_2X_4X_5X_6.]$$

Block	Treatment	Estimated effects
1	(1)	β_0
1	adf	$\beta_1 - \beta_{24} - \beta_{56}$
1	bdef	$\beta_2 - \beta_{35} - \beta_{14}$
1	abe	$-\beta_4 + \beta_{12} + \beta_{36}$
1	cef	$\beta_3 - \beta_{25} - \beta_{46}$
1	acde	$\beta_{13} + \beta_{26} + \beta_{45}$
1	bcd	$-\beta_5 + \beta_{23} + \beta_{16}$
1	abcf	$\beta_6 - \beta_{15} - \beta_{34}$

TABLE 10. - PLAN 1/4; 6f; 8t/b; 2b - R=4

[$X_0 = X_1X_2X_3X_6 = X_1X_3X_4X_5 = X_2X_4X_5X_6$; block confounding, $-X_1X_2X_4$.]

Block	Treatment	Estimated effects (a)
1	(1)	β_0
2	aef	β_1
2	bf	β_2
1	abe	$\beta_{12} + \beta_{36}$
1	cef	β_3
2	ac	$\beta_{13} + \beta_{45} + \beta_{26}$
2	bce	$\beta_{23} + \beta_{16}$
1	abcf	β_6
2	de	β_4
1	adf	$\beta_{14} + \beta_{35}$
1	bdef	$\beta_{24} + \beta_{56}$
2	abd	$\beta^*_{124} + \beta^*_{156} + \beta^*_{235} + \beta^*_{346}$
2	cdf	$\beta_{15} + \beta_{34}$
1	acde	β_5
1	bcd	$\beta_{125} + \beta_{146} + \beta_{234} + \beta_{356}$
2	abcdef	$\beta_{25} + \beta_{46}$

[a]Asterisk denotes confounding with blocks.

TABLE 11. - PLAN 1/2; 6f; 8t/b; 4b - R = 4

[$X_0 = X_1X_2X_3X_6$; block confounding, $-X_1X_2X_4$, $-X_2X_3X_5$, $X_1X_3X_4X_5$.]

Block	Treatment	Estimated effects (a)	Block	Treatment	Estimated effects (a)
1	(1)	β_0	4	e	β_5
3	af	β_1	2	aef	β_{15}
2	bf	β_2	3	bef	β_{25}
4	ab	$\beta_{12} + \beta_{36}$	1	abe	$\beta_{125} + \beta_{356}$
4	cf	β_3	1	cef	β_{35}
2	ac	$\beta_{13} + \beta_{26}$	3	ace	$\beta_{135} + \beta_{256}$
3	bc	$\beta_{23} + \beta_{16}$	2	bce	$\beta^*_{235} + \beta^*_{156}$
1	abcf	β_6	4	abcef	β_{56}
3	d	β_4	2	de	β_{45}
1	adf	β_{14}	4	adef	β_{145}
4	bdf	β_{24}	1	bdef	β_{245}
2	abd	$\beta^*_{124} + \beta^*_{346}$	3	abde	$\beta_{1245} + \beta_{3456}$
2	cdf	β_{34}	3	cdef	β_{345}
4	acd	$\beta_{134} + \beta_{246}$	1	acde	$\beta^*_{1345} + \beta^*_{2456}$
1	bcd	$\beta_{234} + \beta_{146}$	4	bcde	$\beta_{2345} + \beta_{1456}$
3	abcdf	β_{46}	2	abcdef	β_{456}

[a]Asterisk denotes confounding with blocks.

TABLE 12. - PLAN 1; 6f; 8t/b; 8b

[Block confounding, $-X_1X_2X_4$, $-X_2X_3X_5$, $X_1X_2X_3X_6$, $X_1X_3X_4X_5$, $-X_3X_4X_6$, $-X_1X_5X_6$, $X_2X_4X_5X_6$.]

Block	Treatment	Estimated effects (a)	Block	Treatment	Estimated effects (a)	Block	Treatment	Estimated effects (a)	Block	Treatment	Estimated effects (a)
1	1	β_0	4	e	β_5	8	f	β_6	7	ef	β_{56}
5	a	β_1	6	ae	β_{15}	3	af	β_{16}	2	aef	β^*_{156}
6	b	β_2	5	be	β_{25}	2	bf	β_{26}	3	bef	β_{256}
4	ab	β_{12}	1	abe	β_{125}	7	abf	β_{126}	8	abef	β_{1256}
7	c	β_3	8	ce	β_{35}	4	cf	β_{36}	1	cef	β_{356}
2	ac	β_{13}	3	ace	β_{135}	6	acf	β_{136}	5	acef	β_{1356}
3	bc	β_{23}	2	bce	β^*_{235}	5	bcf	β_{236}	6	bcef	β_{2356}
8	abc	β_{123}	7	abce	β_{1235}	1	abcf	β^*_{1236}	4	abcef	β_{12356}
3	d	β_4	2	de	β_{45}	5	df	β_{46}	6	def	β_{456}
8	ad	β_{14}	7	ade	β_{145}	1	adf	β_{146}	4	adef	β_{1456}
7	bd	β_{24}	8	bde	β_{245}	4	bdf	β_{246}	1	bdef	β^*_{2456}
2	abd	β^*_{124}	3	abde	β_{1245}	6	abdf	β_{1246}	5	abdef	β_{12456}
6	cd	β_{34}	5	cde	β_{345}	2	cdf	β^*_{346}	3	cdef	β_{3456}
4	acd	β_{134}	1	acde	β^*_{1345}	7	acdf	β_{1346}	8	acdef	β_{13456}
1	bcd	β_{234}	4	bcde	β_{2345}	8	bcdf	β_{2346}	7	bcdef	β_{23456}
5	abcd	β_{1234}	6	abcde	β_{12345}	3	abcdf	β_{12346}	2	abcdef	β_{123456}

[a]Asterisk denotes confounding with blocks.

TABLE 13. - PLAN 1/4; 6f; 16t/b; 1b - R = 3

$[X_0 = -X_3X_4X_5 = -X_1X_2X_6 = X_1X_2X_3X_4X_5X_6.]$

Block	Treatment	Estimated effects
1	(1)	β_0
1	af	$\beta_1 - \beta_{26}$
1	bf	$\beta_2 - \beta_{16}$
1	ab	$-\beta_6 + \beta_{12}$
1	ce	$\beta_3 - \beta_{45}$
1	acef	β_{13}
1	bcef	β_{23}
1	abce	$-\beta_{36}$
1	de	$\beta_4 - \beta_{35}$
1	adef	β_{14}
1	bdef	β_{24}
1	abde	$-\beta_{46}$
1	cd	$-\beta_5 + \beta_{34}$
1	acdf	$-\beta_{15}$
1	bcdf	$-\beta_{25}$
1	abcd	β_{56}

TABLE 14.[a] - PLAN 1/2; 6f; 16t/b; 2b - R = 5

$[X_0 = X_1X_2X_3X_4X_5X_6;$ block confounding, $-X_3X_4X_5.]$

Block	Treatment	Estimated effects	Block	Treatment	Estimated effects (b)
1	(1)	β_0	2	ef	β_5
1	af	β_1	2	ae	β_{15}
1	bf	β_2	2	be	β_{25}
1	ab	β_{12}	2	abef	$\beta_{125} + \beta_{346}$
2	cf	β_3	1	ce	β_{35}
2	ac	β_{13}	1	acef	$\beta_{135} + \beta_{246}$
2	bc	β_{23}	1	bcef	$\beta_{235} + \beta_{146}$
2	abcf	$\beta_{123} + \beta_{456}$	1	abce	β_{46}
2	df	β_4	1	de	β_{45}
2	ad	β_{14}	1	adef	$\beta_{145} + \beta_{236}$
2	bd	β_{24}	1	bdef	$\beta_{245} + \beta_{136}$
2	abdf	$\beta_{124} + \beta_{356}$	1	abde	β_{36}
1	cd	β_{34}	2	cdef	$\beta^*_{345} + \beta^*_{126}$
1	acdf	$\beta_{134} + \beta_{256}$	2	acde	β_{26}
1	bcdf	$\beta_{234} + \beta_{156}$	2	bcde	β_{16}
1	abcd	β_{56}	2	abcdef	β_6

[a]Ref. 4 (p. 7).

[b]Asterisk denotes confounding with blocks.

TABLE 15. - DEFINING CONTRASTS WITH 7 FACTORS ON BLOCKS OF 8 TREATMENTS

Source	Defining contrasts			
	1/16 Replicate	1/8 Replicate	1/4 Replicate	1/2 Replicate
X_4^2	$-X_1X_2X_4$			
X_5^2	$-X_1X_3X_5$	$-X_1X_3X_5$		
X_6^2	$-X_2X_3X_6$			
X_7^2	$X_1X_2X_3X_7$	$X_1X_2X_3X_7$		
$X_4^2X_5^2$	$X_2X_3X_4X_5$			
$X_4^2X_6^2$	$X_1X_3X_4X_6$	$X_1X_3X_4X_6$	$X_1X_3X_4X_6$	
$X_4^2X_7^2$	$-X_3X_4X_7$			
$X_5^2X_6^2$	$X_1X_2X_5X_6$			
$X_5^2X_7^2$	$-X_2X_5X_7$	$-X_2X_5X_7$	$-X_2X_5X_7$	
$X_6^2X_7^2$	$-X_1X_6X_7$			
$X_4^2X_5^2X_6^2$	$-X_4X_5X_6$	$-X_4X_5X_6$		
$X_4^2X_5^2X_7^2$	$X_1X_4X_5X_7$			
$X_4^2X_6^2X_7^2$	$X_2X_4X_6X_7$	$X_2X_4X_6X_7$		
$X_5^2X_6^2X_7^2$	$X_3X_5X_6X_7$			
$X_4^2X_5^2X_6^2X_7^2$	$-X_1X_2X_3X_4X_5X_6X_7$	$-X_1X_2X_3X_4X_5X_6X_7$	$-X_1X_2X_3X_4X_5X_6X_7$	$-X_1X_2X_3X_4X_5X_6X_7$

TABLE 16. - PLAN 1/16; 7f; 8t/b; 1b -

R = 3

[Defining contrasts given by table 15.]

Block	Treatment	Estimated effects
1	(1)	β_0
1	adeg	$\beta_1 - \beta_{24} - \beta_{35} - \beta_{67}$
1	bdfg	$\beta_2 - \beta_{14} - \beta_{36} - \beta_{57}$
1	abef	$-\beta_4 + \beta_{12} + \beta_{37} + \beta_{56}$
1	cefg	$\beta_3 - \beta_{15} - \beta_{26} - \beta_{47}$
1	acdf	$-\beta_5 + \beta_{13} + \beta_{46} + \beta_{27}$
1	bcde	$-\beta_6 + \beta_{23} + \beta_{17} + \beta_{45}$
1	abcg	$\beta_7 - \beta_{34} - \beta_{25} - \beta_{16}$

TABLE 17. - PLAN 1/8; 7f; 8t/b; 2b - R = 3

[Defining contrasts given by table 15; block confounding, $-X_1X_2X_4$.]

Block	Treatment	Estimated effects (a)
1	(1)	β_0
2	aefg	$\beta_1 - \beta_{35}$
2	bg	$\beta_2 - \beta_{57}$
1	abef	$\beta_{12} + \beta_{37}$
1	cefg	$\beta_3 - \beta_{15}$
2	ac	$-\beta_5 + \beta_{13} + \beta_{46} + \beta_{27}$
2	bcef	$\beta_{23} + \beta_{17}$
1	abcg	$\beta_7 - \beta_{25}$
2	df	$\beta_4 - \beta_{56}$
1	adeg	$\beta_{14} + \beta_{36}$
1	bdfg	$\beta_{24} + \beta_{67}$
2	abde	$\beta^*_{124} + \beta^*_{347} + \beta^*_{236} + \beta^*_{167}$
2	cdeg	$\beta_{34} + \beta_{16}$
1	acdf	$\beta_6 - \beta_{45}$
1	bcde	$\beta_{147} + \beta_{126} + \beta_{367} + \beta_{234}$
2	abcdfg	$\beta_{47} + \beta_{26}$

[a]Asterisk denotes confounding with blocks.

TABLE 18. - PLAN 1/4; 7f; 8t/b; 4b - R = 3

[Defining contrasts given by table 15; block confounding, $-X_1X_2X_4$, $-X_1X_3X_5$, $X_2X_3X_4X_5$.]

Block	Treatment	Estimated effects (a)	Block	Treatment	Estimated effects (a)
1	(1)	β_0	4	eg	$\beta_5 - \beta_{27}$
3	af	β_1	2	aefg	β_{15}
2	bg	$\beta_2 - \beta_{57}$	3	be	$-\beta_7 + \beta_{25}$
4	abfg	β_{12}	1	abef	$-\beta_{17}$
4	cf	β_3	1	cefg	β_{35}
2	ac	$\beta_{13} + \beta_{46}$	3	aceg	$\beta^*_{135} + \beta^*_{456}$
3	bcfg	β_{23}	2	bcef	$-\beta_{37}$
1	abcg	$\beta_{123} + \beta_{246}$	4	abce	$-\beta_{137} - \beta_{467}$
2	df	β_4	3	defg	β_{45}
4	ad	$\beta_{14} + \beta_{36}$	1	adeg	$\beta_{145} + \beta_{356}$
1	bdfg	β_{24}	4	bdef	$-\beta_{47}$
3	abdg	$\beta^*_{124} + \beta^*_{236}$	2	abde	$-\beta_{147} - \beta_{367}$
3	cd	$\beta_{34} + \beta_{16}$	2	cdeg	$\beta_{345} + \beta_{156}$
1	acdf	β_6	4	acdefg	β_{56}
4	bcdg	$\beta_{234} + \beta_{126}$	1	bcde	$-\beta^*_{347} - \beta^*_{167}$
2	abcdfg	β_{26}	3	abcdef	$-\beta_{67}$

[a]Asterisk denotes confounding with blocks.

TABLE 19. - PLAN 1/2; 7f; 8t/b, 8b

[$X_0 = -X_1X_2X_3X_4X_5X_6X_7$; block confounding, $-X_1X_2X_4$, $-X_1X_3X_5$, $-X_2X_3X_6$, $X_2X_3X_4X_5$, $X_1X_3X_4X_6$, $X_1X_2X_5X_6$, $-X_4X_5X_6$.]

Block	Treatment	Estimated effects (a)	Block	Treatment	Estimated effects (a)	Block	Treatment	Estimated effects (a)	Block	Treatment	Estimated effects (a)
1	(1)	β_0	4	eg	β_5	7	fg	β_6	5	ef	β_{56}
6	ag	β_1	8	ae	β_{15}	3	af	β_{16}	2	aefg	β_{156}
2	bg	β_2	3	be	β_{25}	8	bf	β_{26}	6	befg	β_{256}
5	ab	β_{12}	7	abeg	β_{125}	4	abfg	β_{126}	1	abef	$-\beta^*_{347}$
5	cg	β_3	7	ce	β_{35}	4	cf	β_{36}	1	cefg	β_{356}
2	ac	β_{13}	3	aceg	β^*_{135}	8	acfg	β_{136}	6	acef	$-\beta_{247}$
6	bc	β_{23}	8	bceg	β_{235}	3	bcfg	β^*_{236}	2	bcef	$-\beta_{147}$
1	abcg	β_{123}	4	abce	$-\beta_{467}$	7	abcf	$-\beta_{457}$	5	abcefg	$-\beta_{47}$
8	dg	β_4	6	de	β_{45}	2	df	β_{46}	3	defg	β^*_{456}
4	ad	β_{14}	1	adeg	β_{145}	5	adfg	β_{146}	7	adef	$-\beta_{237}$
7	bd	β_{24}	5	bdeg	β_{245}	1	bdfg	β_{246}	4	bdef	$-\beta_{137}$
3	abdg	β^*_{124}	2	abde	$-\beta_{367}$	6	abdf	$-\beta_{357}$	8	abdefg	$-\beta_{37}$
3	cd	β_{34}	2	cdeg	β_{345}	6	cdfg	β_{346}	8	cdef	$-\beta_{127}$
7	acdg	β_{134}	5	acde	$-\beta_{267}$	1	acdf	$-\beta^*_{257}$	4	acdefg	$-\beta_{27}$
4	bcdg	β_{234}	1	bcde	$-\beta^*_{167}$	5	bcdf	$-\beta_{157}$	7	bcdefg	$-\beta_{17}$
8	abcd	$-\beta_{567}$	6	abcdeg	$-\beta_{67}$	2	abcdfg	$-\beta_{57}$	3	abcdef	$-\beta_7$

[a]Asterisk denotes confounding with blocks.

TABLE 20. - DEFINING CONTRASTS WITH 7 FACTORS

ON BLOCKS OF 16 TREATMENTS

Source	Defining contrasts		
	1/8 Replicate	1/4 Replicate	1/2 Replicate
X_5^2	$-X_1X_4X_5$		
X_6^2	$X_1X_2X_4X_6$	$X_1X_2X_4X_6$	
X_7^2	$X_2X_3X_4X_7$		
$X_5^2X_6^2$	$-X_2X_5X_6$		
$X_5^2X_7^2$	$-X_1X_2X_3X_5X_7$	$-X_1X_2X_3X_5X_7$	
$X_6^2X_7^2$	$X_1X_3X_6X_7$		
$X_5^2X_6^2X_7^2$	$-X_3X_4X_5X_6X_7$	$-X_3X_4X_5X_6X_7$	$-X_3X_4X_5X_6X_7$

TABLE 21. - PLAN 1/8; 7f; 16t/b; 1b -

R = 3

[Defining contrasts given in table 20.]

Block	Treatment	Estimated effects
1	(1)	β_0
1	aef	$\beta_1 - \beta_{45}$
1	bfg	$\beta_2 - \beta_{56}$
1	abeg	$\beta_{12} + \beta_{46}$
1	cg	β_3
1	acefg	$\beta_{13} + \beta_{67}$
1	bcf	$\beta_{23} + \beta_{47}$
1	abce	$-\beta_{57}$
1	defg	$\beta_4 - \beta_{15}$
1	adg	$-\beta_5 + \beta_{14} + \beta_{26}$
1	bde	$\beta_{24} + \beta_{16} + \beta_{37}$
1	abdf	$\beta_6 - \beta_{25}$
1	cdef	$\beta_{34} + \beta_{27}$
1	acd	$-\beta_{35}$
1	bcdeg	β_7
1	abcdfg	$\beta_{36} + \beta_{17}$

TABLE 22.[a] - PLAN 1/4; 7f; 16t/b; 2b - R = 4

[Defining contrasts given in table 20; block confounding, $-X_1X_4X_5$.]

Block	Treatment	Estimated effects	Block	Treatment	Estimated effects (b)
1	(1)	β_0	2	eg	β_5
2	afg	β_1	1	aef	β_{15}
1	bfg	β_2	2	bef	β_{25}
2	ab	$\beta_{12} + \beta_{46}$	1	abeg	$-\beta_{37}$
1	cg	β_3	2	ce	β_{35}
2	acf	β_{13}	1	acefg	$-\beta_{27}$
1	bcf	β_{23}	2	bcefg	$-\beta_{17}$
2	abcg	$-\beta_{57}$	1	abce	$-\beta_7$
2	df	β_4	1	defg	β_{45}
1	adg	$\beta_{14} + \beta_{26}$	2	ade	$\beta^*_{145} + \beta^*_{256}$
2	bdg	$\beta_{24} + \beta_{16}$	1	bde	$\beta_{245} + \beta_{156}$
1	abdf	β_6	2	abdefg	β_{56}
2	cdfg	β_{34}	1	cdef	$-\beta_{67}$
1	acd	$\beta_{134} + \beta_{236}$	2	acdeg	$-\beta_{247} - \beta_{167}$
2	bcd	$\beta_{234} + \beta_{136}$	1	bcdeg	$-\beta_{147} - \beta_{267}$
1	abcdfg	β_{36}	2	abcdef	$-\beta_{47}$

[a]Ref. 4 (p. 20).
[b]Asterisk denotes confounding with blocks.

TABLE 23. - PLAN 1/2; 7f; 16t/b; 4b - R = 5

[$X_0 = -X_3X_4X_5X_6X_7$; block confounding, $-X_1X_4X_5$, $-X_2X_5X_6$, $X_1X_2X_4X_6$.]

Block	Treatment	Estimated effects	Block	Treatment	Estimated effects (a)	Block	Treatment	Estimated effects (a)	Block	Treatment	Estimated effects (a)
1	(1)	β_0	2	eg	β_5	4	fg	β_6	3	ef	β_{56}
3	a	β_1	4	aeg	β_{15}	2	afg	β_{16}	1	aef	β_{156}
4	b	β_2	3	beg	β_{25}	1	bfg	β_{26}	2	bef	β^*_{256}
2	ab	β_{12}	1	abeg	β_{125}	3	abfg	β_{126}	4	abef	β_{1256}
1	cg	β_3	2	ce	β_{35}	4	cf	β_{36}	3	cefg	$-\beta_{47}$
3	acg	β_{13}	4	ace	β_{135}	2	acf	β_{136}	1	acefg	$-\beta_{147}$
4	bcg	β_{23}	3	bce	β_{235}	1	bcf	β_{236}	2	bcefg	$-\beta_{247}$
2	abcg	β_{123}	1	abce	β_{1235}	3	abcf	β_{1236}	4	abcefg	$-\beta_{1247}$
3	dg	β_4	4	de	β_{45}	2	df	β_{46}	1	defg	$-\beta_{37}$
1	adg	β_{14}	2	ade	β^*_{145}	4	adf	β_{146}	3	adefg	$-\beta_{137}$
2	bdg	β_{24}	1	bde	β_{245}	3	bdf	β_{246}	4	bdefg	$-\beta_{237}$
4	abdg	β_{124}	3	abde	β_{1245}	1	abdf	β^*_{1246}	2	abdefg	$-\beta_{1237}$
3	cd	β_{34}	4	cdeg	$-\beta_{67}$	2	cdfg	$-\beta_{57}$	1	cdef	$-\beta_7$
1	acd	β_{134}	2	acdeg	$-\beta_{167}$	4	acdfg	$-\beta_{157}$	3	acdef	$-\beta_{17}$
2	bcd	β_{234}	1	bcdeg	$-\beta_{267}$	3	bcdfg	$-\beta_{257}$	4	bcdef	$-\beta_{27}$
4	abcd	β_{1234}	3	abcdeg	$-\beta_{1267}$	1	abcdfg	$-\beta_{1257}$	2	abcdef	$-\beta_{127}$

[a]Asterisk denotes confounding with blocks.

TABLE 24. - DEFINING CONTRASTS WITH 8 FACTORS ON BLOCKS OF 16 TREATMENTS

Source	Defining contrasts		
	1/16 Replicate	1/8 Replicate	1/4 Replicate
X_5^2	$-X_1X_4X_5$		
X_6^2	$X_1X_3X_4X_6$	$X_1X_3X_4X_6$	
X_7^2	$X_2X_3X_4X_7$		
X_8^2	$-X_2X_3X_8$	$-X_2X_3X_8$	
$X_5^2X_6^2$	$-X_3X_5X_6$		
$X_5^2X_7^2$	$-X_1X_2X_3X_5X_7$	$-X_1X_2X_3X_5X_7$	$-X_1X_2X_3X_5X_7$
$X_5^2X_8^2$	$X_1X_2X_3X_4X_5X_8$		
$X_6^2X_7^2$	$X_1X_2X_6X_7$		
$X_6^2X_8^2$	$-X_1X_2X_4X_6X_8$	$-X_1X_2X_4X_6X_8$	$-X_1X_2X_4X_6X_8$
$X_7^2X_8^2$	$-X_4X_7X_8$		
$X_5^2X_6^2X_7^2$	$-X_2X_4X_5X_6X_7$	$-X_2X_4X_5X_6X_7$	
$X_5^2X_6^2X_8^2$	$X_2X_5X_6X_8$		
$X_5^2X_7^2X_8^2$	$X_1X_5X_7X_8$	$X_1X_5X_7X_8$	
$X_6X_7X_8$	$-X_1X_3X_6X_7X_8$		
$X_5^2X_6^2X_7^2X_8^2$	$X_3X_4X_5X_6X_7X_8$	$X_3X_4X_5X_6X_7X_8$	$X_3X_4X_5X_6X_7X_8$

TABLE 25. - PLAN 1/16; 8f; 16t/b; 1b - R = 3

[Defining contrasts given in table 24.]

Block	Treatment	Estimated effects
1	(1)	β_0
1	aef	$\beta_1 - \beta_{45}$
1	bgh	$\beta_2 - \beta_{38}$
1	abefgh	$\beta_{12} + \beta_{67}$
1	cfgh	$\beta_3 - \beta_{28} - \beta_{56}$
1	acegh	$\beta_{13} + \beta_{46}$
1	bcf	$-\beta_8 + \beta_{23} + \beta_{47}$
1	abce	$-\beta_{57} - \beta_{18}$
1	defg	$\beta_4 - \beta_{15} - \beta_{78}$
1	adg	$-\beta_5 + \beta_{14} + \beta_{36}$
1	bdefh	$\beta_{24} + \beta_{37}$
1	abdh	$-\beta_{68} - \beta_{25}$
1	cdeh	$\beta_{34} + \beta_{16} + \beta_{27}$
1	acdfh	$\beta_6 - \beta_{35}$
1	bcdeg	$\beta_7 - \beta_{48}$
1	abcdfg	$\beta_{58} + \beta_{17} + \beta_{26}$

TABLE 26. - PLAN 1/8; 8f; 16t/b; 2b - R = 3

[Defining contrasts given in table 24; block confounding, $-X_1X_4X_5$.]

Block	Treatment	Estimated effects	Block	Treatment	Estimated effects (a)
1	(1)	β_0	2	eg	β_5
2	afg	β_1	1	aef	$\beta_{15} + \beta_{78}$
1	bgh	$\beta_2 - \beta_{38}$	2	beh	β_{25}
2	abfh	β_{12}	1	abefgh	$-\beta_{37}$
1	cfgh	$\beta_3 - \beta_{28}$	2	cefh	β_{35}
2	ach	$\beta_{13} + \beta_{46}$	1	acegh	$-\beta_{27}$
1	bcf	$-\beta_8 + \beta_{23}$	2	bcefg	$-\beta_{58} - \beta_{17}$
2	abcg	$-\beta_{57} - \beta_{18}$	1	abce	$-\beta_7$
2	df	β_4	1	defg	β_{45}
1	adg	$\beta_{14} + \beta_{36}$	2	ade	$\beta^*_{145} + \beta^*_{356} + \beta^*_{478}$
2	bdfgh	β_{24}	1	bdefh	$-\beta_{67}$
1	abdh	$-\beta_{68}$	2	abdegh	$-\beta_{347} - \beta_{568} - \beta_{167}$
2	cdgh	$\beta_{34} + \beta_{16}$	1	cdeh	$\beta_{345} + \beta_{156} + \beta_{678}$
1	acdfh	β_6	2	acdefgh	β_{56}
2	bcd	$-\beta_{48}$	1	bcdeg	$-\beta_{147} - \beta_{367} - \beta_{458}$
1	abcdfg	β_{26}	2	abcdef	$-\beta_{47}$

[a]Asterisk denotes confounding with blocks.

TABLE 27.[a] - PLAN 1/4; 8f; 16t/b; 4b - R = 5

[Defining contrasts given in table 24; block confounding, $-X_1X_4X_5$, $X_1X_3X_4X_6$, $-X_3X_5X_6$.]

Block	Treatment	Estimated effects	Block	Treatment	Estimated effects (b)	Block	Treatment	Estimated effects (b)	Block	Treatment	Estimated effects (b)
1	(1)	β_0	2	eg	β_5	4	fh	β_6	3	efgh	β_{56}
3	agh	β_1	4	aeh	β_{15}	2	afg	β_{16}	1	aef	β_{156}
1	bgh	β_2	2	beh	β_{25}	4	bfg	β_{26}	3	bef	β_{256}
3	ab	β_{12}	4	abeg	$-\beta_{37}$	2	abfh	$-\beta_{48}$	1	abefgh	$-\beta_{367} - \beta_{458}$
4	cg	β_3	3	ce	β_{35}	1	cfgh	β_{36}	2	cefh	$\beta^*_{356} + \beta^*_{478}$
2	ach	β_{13}	1	acegh	$-\beta_{27}$	3	acf	β_{136}	4	acefg	$-\beta_{267}$
4	bch	β_{23}	3	bcegh	$-\beta_{17}$	1	bcf	β_{236}	2	bcefg	$-\beta_{167}$
2	abcg	$-\beta_{57}$	1	abce	$-\beta_7$	3	abcfgh	$-\beta_{567} - \beta_{348}$	4	abcefh	$-\beta_{67}$
3	dh	β_4	4	degh	β_{45}	2	df	β_{46}	1	defg	$\beta_{456} + \beta_{378}$
1	adg	β_{14}	2	ade	β^*_{145}	4	adfgh	$-\beta_{28}$	3	adefh	$-\beta_{258}$
3	bdg	β_{24}	4	bde	β_{245}	2	bdfgh	$-\beta_{18}$	1	bdefh	$-\beta_{158}$
1	abdh	$-\beta_{68}$	2	abdegh	$-\beta_{347} - \beta_{568}$	4	abdf	$-\beta_8$	3	abdefg	$-\beta_{58}$
2	cdgh	β_{34}	1	cdeh	$\beta_{345} + \beta_{678}$	3	cdfg	$\beta_{346} + \beta_{578}$	4	cdef	β_{78}
4	acd	β_{134}	3	acdeg	$-\beta_{247}$	1	acdfh	$-\beta^*_{238}$	2	acdefgh	β_{178}
2	bcd	β_{234}	1	bcdeg	$-\beta_{147}$	3	bcdfh	$-\beta_{138}$	4	bcdefgh	β_{278}
4	abcdgh	$-\beta_{457} - \beta_{368}$	3	abcdeh	$-\beta_{47}$	1	abcdfg	$-\beta_{38}$	2	abcdef	$-\beta_{467} - \beta_{358}$

[a]Ref. 4 (p. 21).
[b]Asterisk denotes confounding with blocks.

TABLE 28.[a] - PLAN 1/8; 7f; 16t/b; 1b - R = 4

$$\left[X_0 = X_1X_2X_3X_5 = X_2X_3X_4X_6 = X_1X_2X_4X_7 = X_1X_4X_5X_6 = X_3X_4X_5X_7 = X_1X_3X_6X_7 = X_2X_5X_6X_7.\right]$$

Block	Treatment	Estimated effects
1	(1)	β_0
1	aeg	β_1
1	befg	β_2
1	abf	$\beta_{12} + \beta_{35} + \beta_{47}$
1	cef	β_3
1	acfg	$\beta_{13} + \beta_{25} + \beta_{67}$
1	bcg	$\beta_{23} + \beta_{15} + \beta_{46}$
1	abce	β_5
1	dfg	β_4
1	adef	$\beta_{14} + \beta_{56} + \beta_{27}$
1	bde	$\beta_{24} + \beta_{36} + \beta_{17}$
1	abdg	β_7
1	cdeg	$\beta_{26} + \beta_{34} + \beta_{57}$
1	acd	$\beta_{134} + \beta_{126} + \beta_{237} + \beta_{245} + \beta_{356} + \beta_{467} + \beta_{157}$
1	bcdf	β_6
1	abcdefg	$\beta_{45} + \beta_{16} + \beta_{37}$

[a]Refs. 12 (p. 486) and 18 (p. 12-18).

TABLE 29.[a] - PLAN 1/16; 8f; 16t/b; 1b - R=4

$$\left[X_0 = X_1X_2X_3X_5 = X_2X_3X_4X_6 = X_1X_2X_4X_7 = X_1X_3X_4X_8 = X_1X_4X_5X_6 = X_3X_4X_5X_7 = X_2X_4X_5X_8 = X_1X_3X_6X_7 = X_1X_2X_6X_8 = X_2X_3X_7X_8 = X_2X_5X_6X_7 = X_3X_5X_6X_8 = X_1X_5X_7X_8 = X_4X_6X_7X_8.\right]$$

Block	Treatment	Estimated effects
1	(1)	β_0
1	aegh	β_1
1	befg	β_2
1	abfh	$\beta_{12} + \beta_{35} + \beta_{47} + \beta_{68}$
1	cefh	β_3
1	acfg	$\beta_{13} + \beta_{25} + \beta_{48} + \beta_{67}$
1	bcgh	$\beta_{23} + \beta_{15} + \beta_{46} + \beta_{78}$
1	abce	β_5
1	dfgh	β_4
1	adef	$\beta_{14} + \beta_{27} + \beta_{38} + \beta_{56}$
1	bdeh	$\beta_{24} + \beta_{36} + \beta_{17} + \beta_{58}$
1	abdg	β_7
1	cdeg	$\beta_{34} + \beta_{26} + \beta_{18} + \beta_{57}$
1	acdh	β_8
1	bcdf	β_6
1	abcdefgh	$\beta_{45} + \beta_{16} + \beta_{37} + \beta_{28}$

[a]Refs. 12 (p. 486) and 18 (p. 12-18).

TABLE 30. - ATTRIBUTES OF RECOMMENDED DESIGNS

Table	Replication	Factors, g	Treatments per block	Number of blocks	Resolution, R	Number of two-factor interactions, g(g - 1)/2	Number of estimable two-factor interactions (a)
2	1/2	4	8	1	4	6	0
3	Full	4	8	2	5	6	6
4	1/4	5	8	1	3	10	0
5	1/2	5	8	2	4	10	4
6	Full	5	8	4	5	10	10
7	1/2	5	16	1	5	10	10
9	1/8	6	8	1	3	15	0
10	1/4	6	8	2	4	15	0
11	1/2	6	8	4	4	15	9
12	Full	6	8	8	5	15	15
13	1/4	6	16	1	3	15	9
14	1/2	6	16	2	5	15	15
16	1/16	7	8	1	3	21	0
17	1/8	7	8	2	3	21	0
18	1/4	7	8	4	3	21	11
19	1/2	7	8	8	5	21	21
21	1/8	7	16	1	3	21	1
22	1/4	7	16	2	4	21	15
23	1/2	7	16	4	5	21	21
25	1/16	8	16	1	3	28	0
26	1/8	8	16	2	3	28	11
27	1/4	8	16	4	5	28	28
28	1/8	7	16	1	4	21	0
29	1/16	8	16	1	4	28	0

[a]Only unconfounded two-factor interaction estimators are counted.

TABLE 31. - COMPARISON OF TOTAL TREATMENTS (EXPERIMENTAL UNITS) REQUIRED WHEN FIRST BLOCK IS PERFORMED TO ESTIMATE FIRST-ORDER MODEL AT STATED NUMBER OF DESIGN CENTERS AND INTERACTION EXPERIMENT IS PERFORMED ONLY AT FINAL DESIGN CENTER

Factors	Design centers for first-order model	Treatments for first-order model		Treatments for completion of interaction model		Total number of units required	
		Blocks of size 8	Blocks of size 16	Blocks of size 8	Blocks of size 16	Blocks of size 8	Blocks of size 16
5	1	8	16	24	0	32	16
5	2	16	32	24	0	40	32
5	3	24	48	24	0	48	48
5	4	32	64	24	0	56	64
6	1	8	16	56	16	64	32
6	2	16	32	56	16	72	48
6	3	24	48	56	16	80	64
6	4	32	64	56	16	88	80
6	5	40	80	56	16	96	96
6	6	48	96	56	16	104	112

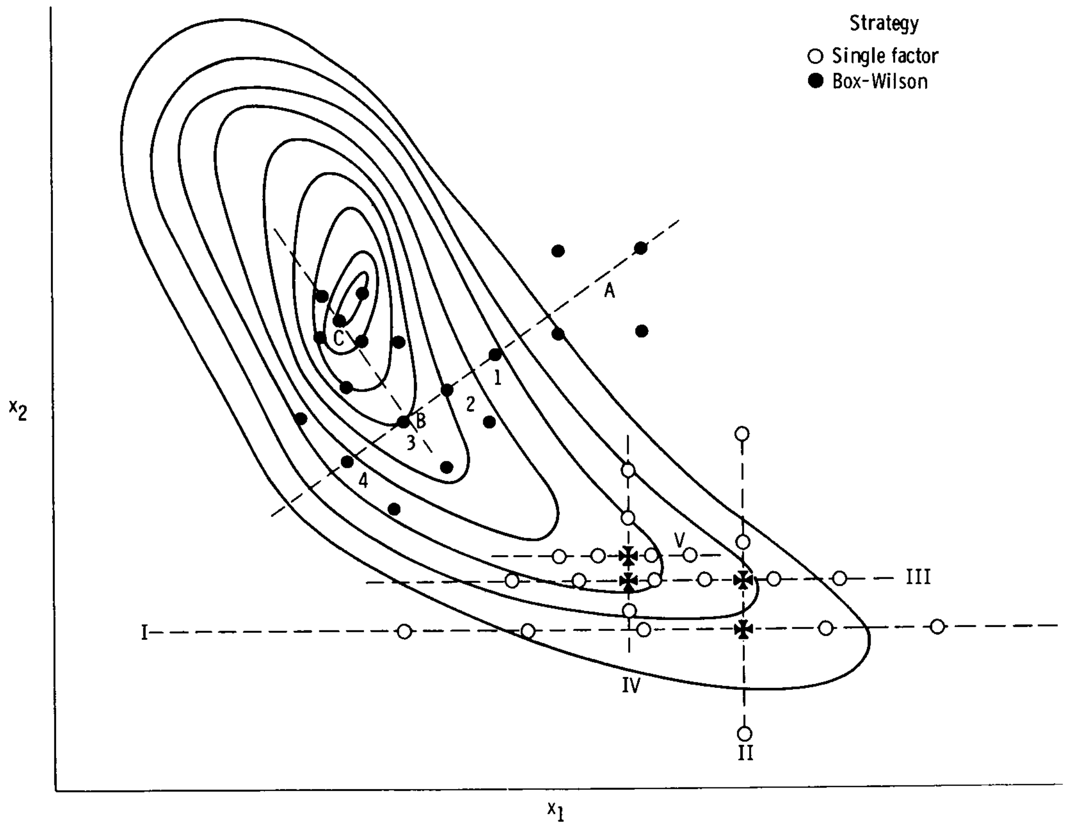

Figure 1. - Strategies for experimental attainment of optimum conditions.

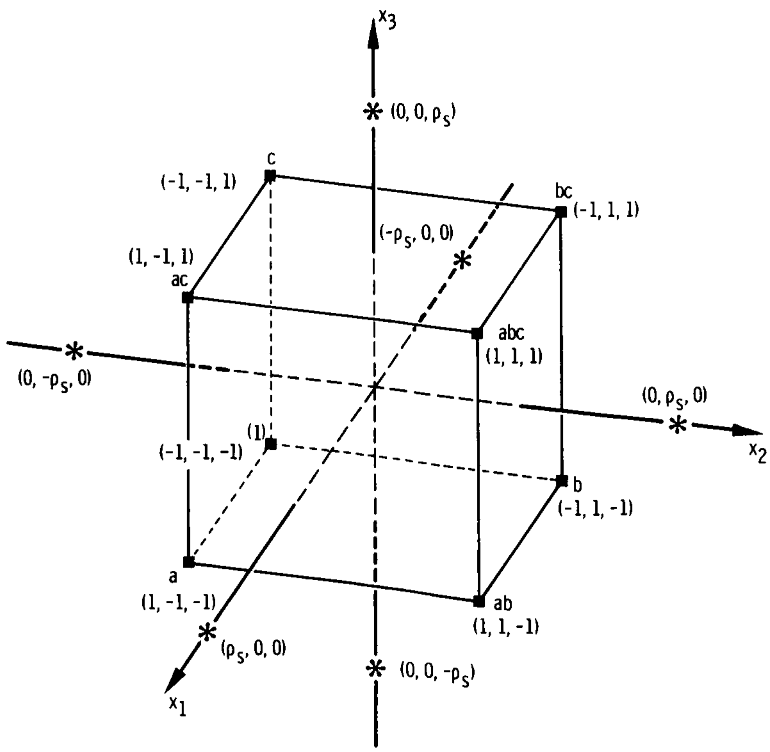

Figure 2. - Cube and star designs.

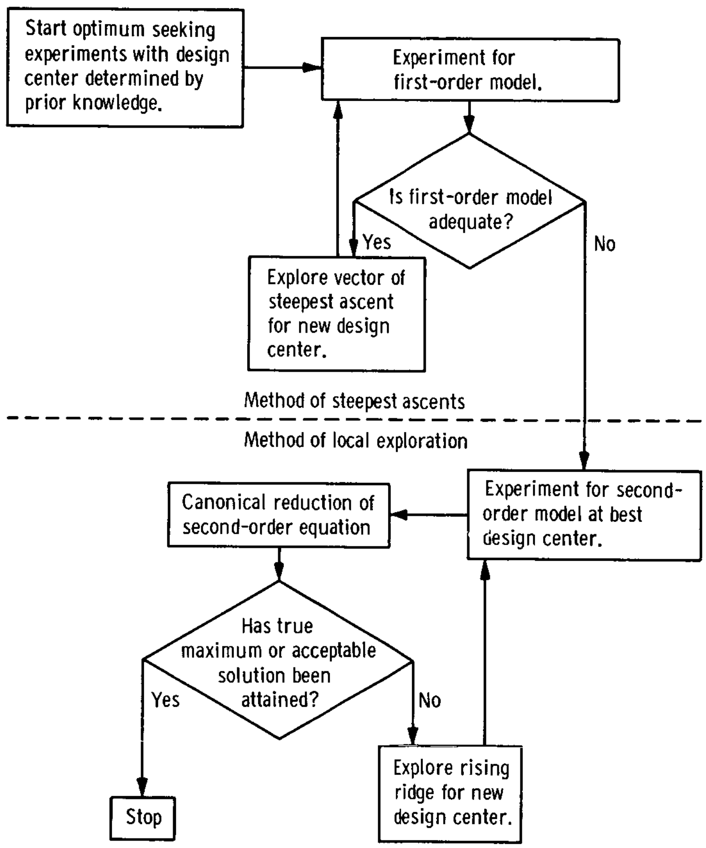

Figure 3. - Box-Wilson methods.

8110161

"The aeronautical and space activities of the United States shall be conducted so as to contribute . . . to the expansion of human knowledge of phenomena in the atmosphere and space. The Administration shall provide for the widest practicable and appropriate dissemination of information concerning its activities and the results thereof."

—NATIONAL AERONAUTICS AND SPACE ACT OF 1958

NASA SCIENTIFIC AND TECHNICAL PUBLICATIONS

TECHNICAL REPORTS: Scientific and technical information considered important, complete, and a lasting contribution to existing knowledge.

TECHNICAL NOTES: Information less broad in scope but nevertheless of importance as a contribution to existing knowledge.

TECHNICAL MEMORANDUMS: Information receiving limited distribution because of preliminary data, security classification, or other reasons.

CONTRACTOR REPORTS: Scientific and technical information generated under a NASA contract or grant and considered an important contribution to existing knowledge.

TECHNICAL TRANSLATIONS: Information published in a foreign language considered to merit NASA distribution in English.

SPECIAL PUBLICATIONS: Information derived from or of value to NASA activities. Publications include conference proceedings, monographs, data compilations, handbooks, sourcebooks, and special bibliographies.

TECHNOLOGY UTILIZATION PUBLICATIONS: Information on technology used by NASA that may be of particular interest in commercial and other non-aerospace applications. Publications include Tech Briefs, Technology Utilization Reports and Notes, and Technology Surveys.

Details on the availability of these publications may be obtained from:

SCIENTIFIC AND TECHNICAL INFORMATION DIVISION

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

Washington, D.C. 20546